The American Political Science Review

Articles and Essays by Marvin Zetterbaum, Thomas Schwartz, Neal E. Cutler, Lester M. Salamon and John J. Siegfried, George Von der Muhll, Peter H. Merkl, Richard M. Merelman

---

Richard F. Fenno, Jr.
U.S. House Members in Their Constituencies

Garrison Nelson
Partisan Patterns of House Leadership Change, 1789–1977

Barbara Deckard Sinclair
Party Realignment and the Transformation of the Political Agenda

Barry S. Rundquist, Gerald S. Strom, and John G. Peters
Corrupt Politicians and Their Electoral Support

Abraham H. Miller, Louis H. Bolce, and Mark Halligan
The J-Curve Theory and the Black Urban Riots

Published Quarterly by

The American Political Science Association

Vol. LXXI September 1977 No. 3

Provocative News

New **AMERICAN PUBLIC POLICY**
What Government Does and What Difference It Makes
ROBERT L. LINEBERRY

Lucid and lively, AMERICAN PUBLIC POLICY focuses on the twin themes of analyzing policy and applying policy to key political issues. Designed for introductory courses in public policy, this text clarifies the major concepts, theories, and methods in the study of domestic policy.

The author deals extensively with inequality, income, energy, and environment—topics which give the book a contemporary perspective. Part I illustrates a variety of approaches and techniques, such as evaluation research, cost benefit analysis, simulation, systems analysis, and experimentation. Part II is devoted to applying the policy methods to four substantive areas—cities, crime, inequality, and managing scarcity.

Tentative: 320 pages; $6.95/paper. August 1977. ISBN 0-06-044013-9.

New **POWER, POLICY, AND PARTICIPATION**
An Introduction to American Government
KENNETH SHERRILL and DAVID VOGLER

This timely and provocative new book is a comprehensive presentation of the institutional and historical aspects of the American political system. Based on the authors' experiences in politics and recent research, the text features previously unpublished survey data on mass behavior.

The stimulating questions the authors raise about the relationships among political participation, democratic theory, and the distribution of power encourage students to think critically and provide bases for productive group discussions. Chapter-length discussions of democratic theory, centralization and decentralization, strategies of influence, and the criminal justice system contribute to the book's uniqueness and contemporary appeal.

Tentative: 544 pages; $10.95. September 1977. ISBN 0-06-046119-5. Instructor's Manual.

CHALLENGE AND DECISION
Political Issues of Our Time, Fifth Edition
REO M. CHRISTENSON

Applauded through five editions, CHALLENGE AND DECISION is a provocative analysis of current domestic problems. The Fifth Edition features chapters on energy, inflation and unemployment, the twenty-seventh amendment and the feminist movement, pornography, and the federal budget. Other issues, including pollution, poverty, income distribution and tax reform, national health insurance, and crime, have been brought up-to-date.

224 pages; $6.50/paper. 1976. ISBN 0-06-041267-4.

Harper & Row
10 East 53d Street
New York, N.Y. 10022

To request examination copies, write to Lilian Schein, Dept. 446.
Please include course title, enrollment, and present text.

The American Political Science Review

Vol. LXXI September 1977 No. 3

CONTENTS

883 U.S. House Members in Their Constituencies: An Exploration — Richard F. Fenno, Jr.

918 Partisan Patterns of House Leadership Change, 1789–1977 — Garrison Nelson

940 Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925–1938 — Barbara Deckard Sinclair

954 Corrupt Politicians and Their Electoral Support: Some Experimental Observations — Barry S. Rundquist, Gerald S. Strom, and John G. Peters

964 The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory — Abraham H. Miller, Louis H. Bolce, and Mark Halligan

983 Equality and Human Need — Marvin Zetterbaum

999 Collective Choice, Separation of Issues and Vote Trading — Thomas Schwartz

1011 Demographic, Social-Psychological, and Political Factors in the Politics of Aging: A Foundation for Research in "Political Gerontology" — Neal E. Cutler

1026 Economic Power and Political Influence: The Impact of Industry Structure on Public Policy — Lester M. Salamon and John J. Siegfried

1044 Communications

1068 Editorial Comment

1070 Book Reviews and Essays

1070 Robert A. Dahl and the Study of Contemporary Democracy: A Review Essay — George Von der Muhll

1097 Trends in German Political Science: A Review Essay — Peter H. Merkl

1109 On Social Psychological Handy Work: An Interpretive Review of *The Handbook of Social Psychology,* Second Edition — Richard M. Merelman

Office of publication: Curtis Reed Plaza, Menasha, Wisconsin.
Foreign Agent: P. S. King and Staples, Ltd., Great Smith Street, Westminster, London.
Second class postage paid at Washington, D.C., and at additional mailing offices.
Composition by TypoGraphics, Columbia, Maryland.
Printed in the United States of America by George Banta Company, Inc., Menasha, Wisconsin.

Copyright 1977, by The American Political Science Association.

ARTICLES

883 **U.S. House Members in Their Constituencies: An Exploration.** The paper addresses itself to two questions left underdeveloped in the literature on representative-constituent relations. First, what does the representative see when he or she sees a constituency? Second, what consequences do these perceptions have for his or her behavior? The paper reverses the normal Washington-oriented view of representative-constituent relations and approaches both questions by examining the representative in his or her constituency. The paper's observations are drawn from the author's travels with seventeen U.S. House members while they were working in their districts. Member perceptions of their constituency are divided into the geographical, the reelection, the primary and the personal constituencies. Attention is then given to the home style of House members. Home style is treated as an amalgam of three elements – allocation of resources, presentation of self, explanation of Washington activity. An effort is made to relate home style to the various perceived constituencies. Some observations are made relating constituency-oriented research to the existing literature on representation.

By RICHARD F. FENNO, JR., Professor of Political Science, University of Rochester.

---

918 **Partisan Patterns of House Leadership Change, 1789–1977.** This study of 364 leadership selections in the U.S. House from 1789 through 1977 discovered that Democrats have a higher proportion of appointed leaders than Republicans; their leaders move between posts in an ordered succession; their appointed leaders are often "removed from above" by their elected ones; and their leaders are subjected to infrequent and unsuccessful caucus challenges. Republicans rely upon election to choose their leaders; their leaders' rate of interpositional mobility is very low; their appointed leaders were never removed by their elected ones; and their leaders face the contests at the same rate as the Democrats do, but the incidence of successful challenges is much greater. They are "removed from below."

Majority vs. minority status had little statistically significant impact upon leadership contests and what variation appeared indicated that challenges were more frequent in the majority party where the stakes are higher and the rewards are greater than in the minority. Regardless of electoral consequences, however, Republican leaders are more vulnerable to caucus defeat than Democratic ones, which lends further support to the contention that party identity is more important than party status.

By GARRISON NELSON, Associate Professor of Political Science, University of Vermont.

---

940 **Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925–1938.** According to Walter Dean Burnham, party realignments "result in significant transformation in the general shape of policy." Through the analysis of House roll-call data, the New Deal realignment is examined to determine whether, in fact, a significant transformation took place and, if so, what its characteristics were. It was hypothesized that if a new political agenda emerged at that time, at least some of the stable policy dimensions which Aage Clausen finds as characterizing the modern Congress should have developed during the New Deal period. In terms of content and level of partisan voting evoked, the government management and the agricultural policy dimensions do take their modern form during the New Deal. A social welfare dimension developed but had not, by the late 1930s, taken its modern shape. It is argued that a major transformation of policy did take place and that, in the process, the ideological distance between the parties increased. This realignment, however, did not immediately change regional voting patterns within each party.

By BARBARA DECKARD SINCLAIR, Associate Professor of Political Science, University of California, Riverside.

---

954 **Corrupt Politicians and Their Electoral Support: Some Experimental Observations.** This paper concerns the relationship between voters and corrupt politicians. An explanation is suggested for why voters would discount even credible information that a candidate is corrupt. Then the results of an experiment designed to test a necessary condition in this explanation are reported. The principal implication of this exploratory study is that corrupt elected officials are immune from electoral reprisal because voters rather easily trade off the information that a candidate is corrupt in return for other things they value in the candidate.

By BARRY S. RUNDQUIST, Visiting Associate Professor of Political Science, University of Illinois at Chicago Circle; GERALD S. STROM, Assistant Professor of Political Science, University of Illinois at Chicago Circle; and JOHN G. PETERS, Assistant Professor of Political Science, University of Nebraska-Lincoln.

---

964 **The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory.** A time-series analysis of individual level, perceptual data disconfirms the J-curve theory of the black urban riots (i.e., that they arise because a period of progress was followed by a sharp decline) and

suggests that ambiguities surrounding black people's perceptions of their economic situation *probably* led to the frustration that culminated in urban violence.

The methodological component of the research deals with such problems of relative deprivation-based research as: (1) the substitution of aggregate, objective-level indicators for perceptual theoretical concepts; (2) the correspondences between objective and perceptual data on both a point-by-point basis and across time-series patterns; (3) the empirical implications of failing to look at important subgroup distinctions; and (4) the crucial assumption of all forms of relative deprivation theory that future expectations of need fulfillment, especially in the period of rising satisfactions, are a function of current levels of need fulfillment. The research calls for modifications in the structure and application of relative deprivation theory in light of the findings in these areas.

By ABRAHAM H. MILLER, Professor of Political Science, University of Cincinnati; LOUIS H. BOLCE, Assistant Professor of Political Science, Fordham University, Lincoln Center; and MARK HALLIGAN, Law Student, Northwestern University Law School.

---

983 **Equality and Human Need.** This paper has the twofold purpose of exploring how and whether it may be said that value arises from human need and, in particular, how the value of equality may arise from an alleged human need for recognition. It traces two opposite dispositions toward recognition, one seeing it as destructive of the minimum conditions for political life, the other viewing it as the principal agency through which men achieve their humanity. The concept of a basic human need is then exposed to the criticism of "social apperception," which apparently renders meaningless the concept altogether. Nevertheless, two "faces" of recognition are explored – one affirming that common human nature in virtue of which all men are said to be equal, and the other, affirming the concrete specificity of each individual. The paper concludes by arguing that this second aspect of the drive for recognition, which is viewed by some as the primary political obligation, is actually not a legitimate aspiration of political life.

By MARVIN ZETTERBAUM, Professor of Political Science, University of California, Davis.

---

999 **Collective Choice, Separation of Issues and Vote Trading.** In legislatures and committees, a number of issues are voted on separately, leading to an outcome consisting of positions on each of these issues. I investigate the effects this separation of issues has on collective choices, assuming a very abstract collective choice model, whose assumptions are presupposed by many less abstract models, notably spatial models. Assuming the model, if there exists an undominated outcome (one to which no winning coalition prefers any other feasible outcome), it must be chosen in the absence of vote trading, although vote trading can (perversely) lead to a very different outcome. But vote trading does not necessarily lead to a "voting paradox" situation, contrary to several recent papers. The model enables us to define a natural solution concept for the case where every feasible outcome is dominated. Variations on this concept are explored. The effects of weakening the model are investigated.

By THOMAS SCHWARTZ, Associate Professor of Government, University of Texas at Austin.

1011 **Demographic, Social-Psychological, and Political Factors in the Politics of Aging: A Foundation for Research in "Political Gerontology."** "Political gerontology" is the study of the political aspects of aging and the aged. Although psychology and sociology have research subfields concerning aging, this interest is just beginning to develop systematically within political science. Consequently, this article describes theory and research from several disciplines which together provide a foundation for research in "political gerontology." Demographic analysis suggests that old people constitute a continuously growing component of the American population. Social-psychological analysis indicates that the aged are likely to engage in substantial political activity. Political analysis suggests that old people are likely to make increasing demands upon the political system. This multidisciplinary knowledge base, combined with the predicted increasing political salience of the aging population, suggests the contours of a research agenda for "political gerontology."

By NEAL E. CUTLER, Associate Professor of Political Science and Chief, Social Policy Laboratory, University of Southern California.

1026 **Economic Power and Political Influence: The Impact of Industry Structure on Public Policy.** Political scientists have devoted considerable attention to the ways in which economic power can be translated into political influence. Yet there has been little empirical research capable of confirming or denying general hypotheses about the political implications of various aspects of economic structure. This article seeks to begin filling this gap by first identifying five aspects of economic structure likely to affect an industry's political influence (firm size, industry size, market concentration, profitability, and geographic dispersion) and then testing these aspects by analyzing how well they account for variations among industries in their success at securing public policies of benefit to them, especially in two policy arenas: federal corporate income taxes and state excise taxes. What emerges most clearly from this analysis is an empirical confirmation of the popular hypothesis linking firm size to political influence with respect to both federal corporate tax policy and state excise tax policy. Beyond that, we find reasonably strong negative relationships between political influence and market concentration, profitability, and industry size – the latter lending interesting support to Mancur Olson's argument about the political disabilities of large groups. In the process, the article suggests a potentially fruitful new way to get beyond the case study approach in studying the impact of economic power on political influence, and thus a way to bring to bear more powerful methodological tools on this central issue of modern democracy.

By LESTER M. SALAMON, Associate Professor of Political Science and Institute of Policy Sciences, Duke University (currently on leave serving as Deputy Associate Director, Office of Management and Budget, Washington, D.C.), and JOHN J. SIEGFRIED, Associate Professor of Economics, Vanderbilt University.

1044 COMMUNICATIONS

From Karen Orren, Patrick Riley, Robert H. Horwitz, Lloyd S. Etheredge, Partha Chatterjee, Patrick J. McGowan and Robert M. Rood, Phillip C. Chapman, Fred H. Willhoite, Jr., Barry Ames and Robert H. Bates, Robert W. Jackman, William H. Riker, Russell Hardin, Donald J. Devine, Robert R. Simmons, B. C. Koh

---

1068 EDITORIAL COMMENT

1070 BOOK REVIEWS AND ESSAYS

1070 **Robert A. Dahl and the Study of Contemporary Democracy: A Review Essay.** Robert A. Dahl, *After the Revolution?;* "The City in the Future of Democracy," in the *American Political Science Review;* "The Concept of Power," in *Behavioral Science; Congress and Foreign Policy;* "A Critique of the Ruling Elite Model," in the *American Political Science Review;* with Ralph S. Brown, Jr., *Domestic Control of Atomic Energy;* "Letters to the Editor: Thomas J. Anton's 'Power, Pluralism, and Local Politics'," in *Administrative Science Quarterly; Modern Political Analysis; Pluralist Democracy in the United States: Conflict and Consent; Political Oppositions in Western Democracies* (editor and contributor); with Charles E. Lindblom, *Politics, Economics, and Welfare; Polyarchy: Participation and Opposition; A Preface to Democratic Theory; Regimes and Oppositions* (editor); "The Science of Public Administration: Three Problems," in *Public Administration Review;* with Edward R. Tufte, *Size and Democracy.*

By GEORGE VON DER MUHLL, Professor of Political Science, University of California, Santa Cruz.

1097 **Trends in German Political Science: A Review Essay.** Juergen Dittberner and Rolf Ebbighausen, eds., *Parteiensystem in der Legitimationskrise;* Ossip K. Flechteim, ed., *Die Parteien in der Bundesrepublik;* Bernd Guggenberger, *Wem neutzt der Staat? Kritik der neomarxistischen Staatstheorie;* Juergen Habermas, *Legitimationskrisen im Spaetkapitalismus;* Juergen Habermas and Niklas Luhmann, *Theorie der Gessellschaft oder Sozialtechnologie?;* Wilhelm Hennis, *Die Missverstandene Demokratie;* Werner Kaltefleiter, *Die Funktionem des Staatsoberhauptes in der parlamentarischen Demokratie;* Heinz Laufer, *Das foederative System der Bundesrepublik Dtuschland, eine Bilanz;* Claus Offe, *Strukturprobleme des dapitalistischen Staates;* Fritz Scharpf, *Demokratietheorie Zwischen Utopie und Anpassung;* Helmut Schelsky, *Die Arbeit tun die anderen;* Klaus von Beyme, *Die politischen Theorien der Gegenwart.*

By PETER H. MERKL, Professor of Political Science, University of California, Santa Barbara.

1109 **On Social Psychological Handy Work: An Interpretive Review of** *The Handbook of Social Psychology,* **Second Edition.** This paper reviews the new *Handbook of Social Psychology,* with a special eye towards its utility for political scientists. The review focuses on theory, methodology, substantive areas of social psychological research, and political applications of social psychological findings. Special attention is paid to *Handbook* articles of particular merit and application to political science. These include articles on cognitive theory, experimentation, observational analyses and sociometry, as well as articles which add to our knowledge of such politically important problems as reasoning, compliance, and decision making. Throughout, important findings relevant to the operations of politics are spotlighted. These include, *inter alia,* cognitive biases towards the perception of unequal influence, the "risky shift," constraints on selective perception, and characteristics of leadership behavior. Omissions, theoretical flaws, and errors due to the "datedness" of findings are also discussed.

By RICHARD M. MERELMAN, Professor of Political Science, University of Wisconsin-Madison.

1121 **Political Theory, History of Political Thought and Methodology**

A. Appadorai, ed., Documents on Political Thought in Modern India, Volume I. William L. Richter, p. 1121.
Douglas E. Ashford, *Ideology and Participation.* Roger W. Cobb, p. 1121.
Robert Benewick, R. N. Berki, and Bhikhu Parekh, eds., *Knowledge and Belief in Politics: The Problem of Ideology.* Charles F. Andrain, p. 1122.
Alexander M. Bickel, *The Morality of Consent.* Lief H. Carter, p. 1124.

Ki-Taek Chun, Sidney Cobb, and John R. P. French, Jr., *Measures for Psychological Assessment: A Guide to 3,000 Original Sources and Their Applications.* John B. McConahay, p. 1125.
Terry Nichols Clark, *Prophets and Patrons: The French University and the Emergence of the Social Sciences.* Alan Charles Kors, p. 1126.
Werner J. Dannhauser, *Nietzsche's View of Socrates.* George Grant, p. 1127.
Graeme Duncan, *Marx and Mill: Two Views of Social Conflict and Social Harmony.* John Maguire, p. 1129.
T. O. Elias, *The Modern Law of Treaties.* Milada Selucka, p. 1130.
John Ferguson, *Utopias of the Classical World.* Arlene W. Saxonhouse, p. 1131.
Paul Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge.* Ernest Nagel, p. 1132.
A. James Gregor, *The Fascist Persuasion in Radical Politics.* Roland Sarti, p. 1134.
Robert B. Harmon, *Political Science: A Bibliographical Guide to the Literature. Second Supplement.* Ihor Kamenetsky, p. 1135.
James Turner Johnson, *Ideology, Reason, and the Limitation of War: Religious and Secular Concepts, 1200–1740.* Roland A. Bainton, p. 1136.
Mortimer R. Kadish and Sanford H. Kadish, *Discretion to Disobey: A Study of Lawful Departures from Legal Rules.* Wilfrid E. Rumble, p. 1137.
V. G. Kiernan, *Marxism and Imperialism.* Kenneth E. Boulding, p. 1138.
Preston King, *The Ideology of Order: A Comparative Analysis of Jean Bodin and Thomas Hobbes.* John Sanderson, p. 1139.
Oscar Kraines, *The World and Ideas of Ernst Freund: The Search for General Principles of Legislation and Administrative Law.* Clyde E. Jacobs, p. 1140.
Leonard Krieger, *An Essay on the Theory of Enlightened Despotism.* Mark Gavre, p. 1141.
Vladimir V. Kusin, *The Intellectual Origins of the Prague Spring: The Development of Reformist Ideas in Czechoslovakia;* and Dusan Hamsik, *Writers Against Rulers.* Galia Golan, p. 1142.
Kenneth C. Land and Seymour Spilerman, eds., *Social Indicator Models.* Karl A. Fox, p. 1143.
Michel Launay, *Jean-Jacques Rousseau, Ecrivain Politique (1712–1762).* James H. Nichols, Jr., p. 1145.
Dominique Lecourt, *Marxism and Epistemology: Bachelard, Canguilhem, Foucault.* Bhikhu Parekh, p. 1146.
Stephen V. Monsma, *The Unravelling of America: Wherein the Author Analyzes the Inadequacy of Current Political Options and Responds With a Christian Approach to Government.* Gordon L. Shull, p. 1147.
George Dennis O'Brien, *Hegel on Reason and History: A Contemporary Interpretation.* Stanley Rosen, p. 1148.
A. N. Oppenheim and Judith Torney, *The Measurement of Children's Civic Attitudes in Different Nations.* David O. Sears, p. 1149.
Nicholas Piediscalzi and Robert G. Thobaben, eds., *From Hope to Liberation: Towards a New Marxist-Christian Dialogue.* Douglas Sturm, p. 1150.
J. G. A. Pocock, *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition.* Harvey C. Mansfield, Jr., p. 1151.
Lucian W. Pye, ed., *Political Science and Area Studies: Rivals or Partners?* Joseph LaPalombara, p. 1152.
Jean-Paul Sartre, *Between Existentialism and Marxism.* Paul Piccone, p. 1154.
Stuart A. Scheingold, *The Politics of Rights: Lawyers, Public Policy, and Political Change.* Charles S. Bullock, III, p. 1155.
Harold L. Sheppard and Neal Q. Herrick, *Where Have All the Robots Gone: Worker Dissatisfaction in the '70s.* Harold Wool, p. 1156.
Martin Shubik, *Games for Society, Business, and War: Towards a Theory of Gaming.* Harold Guetzkow, p. 1157.
Frederick Martin Stern, *Life and Liberty: A Return to First Principles.* R. J. Rowan, p. 1158.
Ralph M. Stogdill, *Handbook of Leadership: A Survey of Theory and Research.* Clayton P. Alderfer, p. 1159.
George R. Vickers, *The Formation of the New Left: The Early Years.* Dale Vree, p. 1160.
Leslie D. Wilcox, Ralph M. Brooks, George M. Beal, and Gerald E. Klongan, *Social Indicators and Societal Monitoring: An Annotated Bibliography.* Marian Lief Palley, p. 1162.
Willard Wolfe, *From Radicalism to Socialism: Men and Ideas in the Formation of Fabian Socialist Doctrines, 1881–1889.* David Miller, p. 1162.

1163 American Government and Politics

Henry J. Abraham, *Justices and Presidents: A Political History of Appointments to the Supreme Court.* Sheldon Goldman, p. 1163.
David W. Adamany and George E. Agree, *Political Money: A Strategy for Campaign Financing in America.* Herbert E. Alexander, p. 1164.
Robert P. Althauser and Sydney S. Spivack, in collaboration with Beverly M. Amsel, *The Unequal Elites.* Clifford J. Wirth, p. 1165.

William I. Bacchus, *Foreign Policy and the Bureaucratic Process: The State Department's Country Director System.* Elmer Plischke, p. 1166.

Howard Ball, *The Vision and the Dream of Justice Hugo L. Black.* Stephen P. Strickland, p. 1167.

Robert L. Bish, Robert Warren, Louis F. Weschler, James A. Crutchfield and Peter Harrison, *Coastal Resource Use: Decisions on Puget Sound;* and Melvin B. Moguloff, *Saving the Coast: California's Experiment in Intergovernmental Land Use Control.* Dean E. Mann, p. 1169.

Sterling Brubaker, *In Command of Tomorrow: Resource and Environmental Strategies for Americans.* Robert H. Pealy, p. 1170.

Douglass Cater and Stephen Strickland, *TV Violence and the Child: The Evolution and Fate of the Surgeon General's Report.* Ronald P. Abeles, p. 1171.

John M. Coleman, *Thomas McKean: Forgotten Leader of the Revolution.* James P. Whittenburg, p. 1172.

Paul R. Dommel, *The Politics of Revenue Sharing.* James P. Pfiffner, p. 1172.

Dennis W. Ducsik, *Shoreline for the Public: A Handbook of Social, Economic, and Legal Considerations Regarding Public Recreational Use of the Nation's Coastal Shoreline.* Carol S. Greenwald, p. 1173.

John A. Ferejohn, *Pork Barrel Politics: Rivers and Harbors Legislation, 1947–68.* Julius Margolis, p. 1174.

Jack D. Foner, *Blacks and the Military in American History: A New Perspective.* Dianne M. Pinderhughes, p. 1176.

Victor R. Fuchs, *Who Shall Live? Health, Economics, and Social Choice.* Michael S. Koleda, p. 1176.

John A. Gardiner and Michael A. Mulkey, eds., *Crime and Criminal Justice: Issues in Public Policy Analysis.* John Paul Ryan, p. 1177.

Edie N. Goldenberg, *Making the Papers: The Access of Resource-Poor Groups to the Metropolitan Press.* Dan Nimmo, p. 1178.

William B. Gwyn, *Barriers to Establishing Urban Ombudsmen: The Case of Newark.* Harris I. Effross, p. 1180.

Morton Halperin, with Priscilla Clapp and Arnold Kanter, *Bureaucratic Politics and Foreign Policy.* David Howard Davis, p. 1180.

Howard D. Hamilton and Sylvan H. Cohen, *Policymaking by Plebiscite: School Referenda.* Christine H. Rossell, p. 1181.

Stephen Hess, *The Presidential Campaign: The Leadership Selection Process After Watergate. An Essay.* Abraham Holtzman, p. 1182.

Jacob Judd and Irwin H. Polishook, eds., *Aspects of Early New York Society and Politics.* Richard E. Ellis, p. 1183.

Daniel Katz, Barbara A. Gutek, Robert L. Kahn and Eugenia Barton, *Bureaucratic Encounters: A Pilot Study in the Evaluation of Government Services.* Daniel Madar, p. 1184.

John H. Kessel, *The Domestic Presidency: Decision-Making in the White House.* Peter W. Sperlich, p. 1185.

Lauriston R. King, *The Washington Lobbyists for Higher Education.* Lawrence K. Pettit, p. 1187.

Allen V. Kneese and Charles L. Schultze, *Pollution, Prices and Public Policy.* John F. Sacco, p. 1187.

Paul A. Kurzman, *Harry Hopkins and the New Deal.* Thomas C. Blaisdell, Jr., p. 1189.

Karl Lamb, *As Orange Goes: Twelve California Families and the Future of American Politics.* Elizabeth Wirth Marvick and Dwaine Marvick, p. 1190.

Robert F. Lanzillotti, Mary T. Hamilton and R. Blaine Roberts, *Phase II in Review: The Price Commission Experience.* Robert A. Kagan, p. 1191.

Charles H. Levine, *Racial Conflict and the American Mayor.* Donald Haider, p. 1192.

Robert A. Levine, *Public Planning: Failure and Redirection.* Melton L. Spivak, p. 1193.

Stuart S. Nagel, ed., *Policy Studies in America and Elsewhere.* Robert H. Salisbury, p. 1194.

Peter S. Onuf, ed., *Maryland and the Empire, 1773: The Antilon-First Citizen Letters.* Thomas C. Barrow, p. 1195.

David J. Ott, Lawrence J. Korb, Thomas Gale Moore, Dave M. O'Neill, Attiat F. Ott, Rudolph G. Penner, and Thomas Vasquez, *Public Claims on U.S. Output: Federal Budget Options in the Last Half of the Seventies.* Charles Walcott, p. 1196.

Kevin P. Phillips, *Mediacracy: American Parties and Politics in the Communications Age.* Dale Vree, p. 1197.

Kevin P. Phillips and Paul H. Blackman, *Electoral Reform and Voter Participation: Federal Registration—A False Remedy for Voter Apathy.* Stanley Kelley, Jr., p. 1198.

John Pincus, ed., *School Finance in Transition: The Courts and Educational Reform.* Michael A. Cohen, p. 1199.

Jeffrey L. Pressman, *Federal Programs and City Politics: The Dynamics of the Aid Process in Oakland.* Russell D. Murphy, p. 1200.

Thomas W. Ryley, *A Little Group of Willful Men: A Study of Congressional/Presidential Authority.* Edward H. Buehrig, p. 1201.

Lester G. Seligman, Michael R. King, Chong Lim Kim, and Roland E. Smith, *Patterns of Recruitment: A State Chooses Its Lawmakers.* James A. Thurber, p. 1201.

Paul M. Sniderman, *Personality and Democratic Politics.* Richard M. Merelman, p. 1203.

David T. Stanley, *Managing Local Government Under Union Pressure;* and Harry H Wellington and Ralph K. Winter, Jr., *The Unions and the Cities.* Glenn W. Miller, p. 1204.

Richard J. Stillman II, *The Rise of the City Manager: A Public Professional in Local Government.* Jay M. Shafritz, p. 1206.
Walter I. Trattner, *From Poor Law to Welfare State: A History of Social Welfare in America.* Frank T. Colon, p. 1207.
Hanes Walton, Jr., *Black Republicans: The Politics of the Black and Tans.* Douglas Gatlin, p. 1208.
William D. White, *U.S. Tactical Airpower: Missions, Forces, and Costs.* Jack M. Schick, p. 1208.

1209 Comparative Government and Politics

Najmul Abedin, *Local Administration and Politics in Modernising Societies: Bangladesh and Pakistan.* George M. Platt, p. 1209.
Agribusiness Council, *Agricultural Initiative in the Third World.* Gary L. Olson, p. 1211.
Michael Barker, *Gladstone and Radicalism: The Reconstruction of Liberal Policy in Britain 1885–1894.* David Miller, p. 1212.
Elena L. Berger, *Labour, Race, and Colonial Rule: The Copperbelt from 1924 to Independence.* Richard L. Sklar, p. 1212.
Michael Brecher, *Israel, The Korean War and China: Images, Decisions and Consequences.* Edward N. Luttwak, p. 1213.
Harold Brookfield, *Interdependent Development.* Phillip Althoff, p. 1214.
Peter A. Busch, *Legitimacy and Ethnicity: A Case Study of Singapore.* Robert E. Gamer, p. 1215.
David Butler and Donald Stokes, *Political Change in Britain: The Evolution of Electoral Choice.* Alan S. Zuckerman, p. 1216.
Ronald H. Chilcote and Joel C. Edelstein, eds., *Latin America: The Struggle With Dependency and Beyond.* Marvin Alisky, p. 1217.
G. W. Choudhury, *The Last Days of United Pakistan.* Lawrence Ziring, p. 1219.
David Coates, *The Labour Party and the Struggle for Socialism.* Lyndelle D. Fairlie, p. 1220.
Gerard H. Corr, *The Chinese Red Army: Campaigns and Politics Since 1949.* Pierre M. Perrolle, p. 1221.
Mary Edmond, ed., *European Parliament Digest, Volume I; 1973.* D. J. Wenden, p. 1222.
John Ellis, *Armies in Revolution.* Craig McCaughrin, p. 1222.
John P. Entelis, *Pluralism and Party Transformation in Lebanon: Al-Kata'ib 1936–1970.* Michael C. Hudson, p. 1223.
Edward Friedman, *Backward Toward Revolution: The Chinese Revolutionary Party.* Lawrence Sullivan, p. 1225.
André-Paul Frognier, Vincent E. McHale, and Dennis Paranzino, *Vote, Clivages Socio-Politiques et Developpement Regional en Belgique.* A. Van den Brande, p. 1226.
George Ginsburgs, *Soviet Works on Korea: 1945–1970.* Robert R. Simmons, p. 1227.
R. Kenneth Godwin, ed., *Comparative Policy Analysis: The Study of Population Policy Determinants in Developing Countries.* William Petersen, p. 1227.
J. E. Greene, *Race vs. Politics in Guyana: Political Cleavages and Political Mobilization in the 1968 General Elections.* Robert W. Anderson, p. 1228.
Babette Gross, *Willi Münzenberg: A Political Biography.* Henry Krisch, p. 1229.
William F. Gutteridge, *Military Regimes in Africa.* Joseph P. Smaldone, p. 1230.
Howard Handelman, *Struggle in the Andes: Peasant Political Mobilization in Peru.* Carlos A. Astiz, p. 1231.
Gregory Henderson, Richard Ned Lebow and John S. Stoessinger, eds., *Divided Nations in a Divided World.* Young Whan Kihl, p. 1233.
Winston Hsieh, *Chinese Historiography on the Revolution of 1911.* Steven I. Levine, p. 1234.
G. F. Hudson, ed., *Reform and Revolution in Asia.* Bruce D. Larkin, p. 1235.
Charles A. Joiner, *The Politics of Massacre: Political Processes in South Vietnam.* William S. Turley, p. 1236.
J. H. Kalicki, *The Pattern of Sino-American Crises: Political-Military Interactions in the 1950s.* Charles W. Kegley, Jr., p. 1237.
Walid Kazziha, *Revolutionary Transformation in the Arab World: Habash and His Comrades from Nationalism to Marxism.* William B. Quandt, p. 1238.
Gavin Kennedy, *The Military in the Third World.* Fred R. von der Mehden, p. 1239.
Theofil I. Kis, *Le fédéralisme soviétique: Ses particularités typologiques.* David L. Williams, p. 1240.
Howard M. Leichter, *Political Regime and Public Policy in the Philippines: A Comparison of Bacolod and Iloilo Cities.* Thomas C. Nowak, p. 1241.
Victor T. LeVine, *Political Corruption: The Ghana Case.* Jon Kraus, p. 1242.
Colin Leys, *Underdevelopment in Kenya: The Political Economy of Neo-Colonialism 1964–1971.* Lynn Krieger Mytelka, p. 1243.
Neil McInnes, *The Communist Parties of Western Europe.* Thomas H. Greene, p. 1244.
David Morgan, *Suffragists and Liberals: The Politics of Woman Suffrage in Britain.* R. M. Punnett, p. 1246.
E. V. Neimeyer, Jr., *Revolution at Querétaro: The Mexican Constitutional Convention of 1916–1917.* Evelyn P. Stevens, p. 1246.

Donal B. Cruise O'Brien, *Saints and Politicians: Essays in the Organization of a Senegalese Peasant Society*. W. A. E. Skurnik, p. 1247.
Ersin Onulduran, *Political Development and Political Parties in Turkey;* and Ilkay Sunar, *State and Society in the Politics of Turkey's Development*. Walter F. Weiker, p. 1248.
Nissan Oren, *Revolution Administered: Agrarianism and Communism in Bulgaria*. F. Stephen Larrabee, p. 1250.
Robert L. Pfaltzgraff, Jr., ed., *Contrasting Approaches to Strategic Arms Control*. Cecil V. Crabb, Jr., p. 1251.
Terence Prittie, *Willy Brandt: Portrait of a Statesman*. Barbara Kellerman, p. 1252.
Sir James Robertson, *Transition in Africa: From Direct Rule to Independence. A Memoir*. Peter Duignan, p. 1254.
Stuart R. Schram, ed., *Authority, Participation and Cultural Change in China*. Michael Y. M. Kau, p. 1254.
Tony Sharp, *The Wartime Alliance and the Zonal Division of Germany*. Kenneth Hanf, p. 1255.
Martin Staniland, *The Lions of Dagbon: Political Change in Northern Ghana*. David R. Smock, p. 1257.
Gianni Statera, *Death of a Utopia: The Development and Decline of Student Movements in Europe*. Ehud Sprinzak, p. 1257.
Evelyn P. Stevens, *Protest and Response in Mexico*. Franz A. von Sauer, p. 1259.
Hung-chao Tai, *Land Reform and Politics: A Comparative Analysis*. James C. Scott, p. 1260.
Frank Tannenbaum, *The Future of Democracy in Latin America*. Robert D. Tomasek, p. 1261.
Richard C. Thornton, *China, The Struggle for Power, 1917–1972*. Susan H. Marsh, p. 1263.
Robert F. Turner, *Vietnamese Communism: Its Origins and Development*. Cecil Hobbs, p. 1264.
A. F. Upton, *Communism in Scandinavia and Finland: Politics of Opportunity*. John H. Hodgson, p. 1265.
Jon M. Van Dyke, *North Vietnam's Strategy for Survival*. William S. Turley, p. 1266.
A. E. Van Niekerk, *Populism and Political Development in Latin America*. Clifford Kaufman, p. 1267.
Martin Weinstein, *Uruguay: The Politics of Failure*. Ronald H. McDonald, p. 1268.
Allen S. Whiting, *The Chinese Calculus of Deterrence: India and Indochina*. Harold C. Hinton, p. 1269.
Robert Wolfe, ed., *Captured German and Related Records: A National Archives Conference*. Henry Cord Meyer, p. 1269.
Ken Young, ed., *Essays on the Study of Urban Politics*. Murray S. Stedman, Jr., p. 1270.
Kent Zetterberg, *Liberalism i Kris. Folkpartiet 1939–1945. (Liberalism in Crisis: The Liberal Party 1939–1945)*. Joseph B. Board, p. 1271.

1272 International Politics, Law, and Organization

Ludwig W. Adamec, *Afghanistan's Foreign Affairs to the Mid-Twentieth Century: Relations with the U.S.S.R., Germany, and Britain*. Leon B. Poullada, p. 1272.
Ian W. A. C. Adie, *Oil, Politics and Seapower: The Indian Ocean Vortex*. John Badgley, p. 1273.
Nicholas Balabkins, *West German Reparations to Israel*. Josef Joffe, p. 1274.
Dennis L. Bark, *Agreement on Berlin: A Study of the 1970–72 Quadripartite Negotiations*. Lawrence L. Whetten, p. 1275.
Nissim Bar-Yaacov, *The Handling of International Disputes by Means of Inquiry*. Boleslaw A. Boczek, p. 1276.
J. Bowyer Bell, Jr., *The Horn of Africa: Strategic Magnet in the Seventies*. John H. Spencer, p. 1277.
Nicholas Bethell, *The Last Secret*. Milos Martic, p. 1278.
Dimitri S. Bitsios, *Cyprus: The Vulnerable Republic*. Van Coufoudakis, p. 1279.
Paul Bock, *In Search of a Responsible World Society: The Social Teachings of the World Council of Churches*. Michael M. Gunter, p. 1280.
Michael Brecher, *Decisions in Israel's Foreign Policy*. Gil Carl AlRoy, p. 1280.
Philippe Bretton and Jean-Pierre Chaudet, *La Coexistence Pacifique*. Alain Pellet, p. 1282.
Richard Dean Burns and Edward M. Bennett, eds., *Diplomats in Crisis: United States – Chinese – Japanese Relations, 1919–1941*. Willard H. Elsbree, p. 1283.
Sergei B. Chetverikov, *Kto i kak delaet politiku SShA (Who makes policy in the USA and how it is made)*. Richard M. Mills, p. 1284.
Ralph N. Clough, *East Asia and U.S. Security*. Allen S. Whiting, p. 1285.
Gordon Connell-Smith, *The United States and Latin America: An Historical Analysis of Inter-American Relations*. Yale H. Ferguson, p. 1286.
Henry Delfiner, *Vienna Broadcasts to Slovakia, 1938–1939: A Case Study in Subversion*. Ivo D. Duchacek, p. 1286.
Roger L. Dial, ed., *Advancing and Contending Approaches to the Study of Chinese Foreign Policy*. Harold C. Hinton, p. 1287.
Richard A. Falk, *A Global Approach to National Policy*. Morton A. Kaplan, p. 1288.
Robert L. Gallucci, *Neither Peace Nor Honor: The Politics of American Military Policy in Viet-Nam*. H. Bradford Westerfield, p. 1289.

Melvin Gurtov, *The United States Against the Third World: Antinationalism and Intervention.* Bahgat Korany, p. 1291.

Milton O. Gustafson, ed., *The National Archives and Foreign Relations Research.* William I. Bacchus, p. 1292.

Stanley J. Heginbotham, *Cultures in Conflict: The Four Faces of Indian Bureaucracy.* David C. Potter, p. 1293.

Daniel Heradstveit, *Arab and Israeli Elite Perceptions.* William R. Thompson, p. 1295.

Donald R. Hodgman, *National Monetary Policies and International Monetary Cooperation;* and Gerald M. Meier, *Problems of a World Monetary Order.* Jonathan Story, p. 1296.

J. C. Hurewitz, ed., *The Middle East and North Africa in World Politics: A Documentary Record.* Volume I, *European Expansion, 1535–1914.* Roderic H. Davison, p. 1298.

N. Khac Huyen, *Vision Accomplished? The Enigma of Ho Chi Minh.* Charles A. Joiner, p. 1298.

J. P. Jain, *China, Pakistan and Bangladesh.* Talukder Maniruzzaman, p. 1300.

J. P. Jain, *India and Disarmament:* Volume I, *Nehru Era, An Analytical Study.* B. Ramesh Babu, p. 1300.

Michael Y. M. Kau, ed., *The Lin Piao Affair: Power Politics and Military Coup.* Edward E. Rice, p 1301.

Robert A. Kilmarx, *Soviet–United States Naval Balance.* Norman Polmar, p. 1302.

Max Kohnstamm and Wolfgang Hager, *A Nation Writ Large? Foreign Policy Problems Before the European Community.* Ghita Ionescu, p. 1303.

Arnold Krammer, *The Forgotten Friendship: Israel and the Soviet Bloc 1947–1953.* Ilana Kass, p. 1304.

Ellis S. Krauss, *Japanese Radicals Revisited: Student Protest in Postwar Japan.* William E. Steslicke, p. 1305.

Donald R. Lesh, ed., *A Nation Observed: Perspectives on America's World Role.* Harold S. Johnson, p. 1306.

Kurt London, ed., *The Soviet Impact on World Politics.* W. W. Kulski, p. 1307.

William Macomber, *The Angels' Game: A Handbook of Modern Diplomacy.* Alan K. Henrikson, p. 1309.

Fergus MacPherson, *Kenneth Kaunda of Zambia: The Times and the Man.* Thomas Patrick Melady, p. 1310.

R. D. McLaurin, *The Middle East in Soviet Policy.* Alvin Z. Rubinstein, p. 1311.

Carolyn McMaster, *Malawi: Foreign Policy and Development.* Patrick J. McGowan, p. 1311.

Carl H. McMillan, ed., *Chaiging Perspectives in East-West Commerce.* Joseph Hajda, p. 1312.

Arthur Mitchell, *Labour in Irish Politics, 1890–1930.* A. Maltby, p. 1313.

David J. Myers, *Democratic Campaigning in Venezuela: Caldera's Victory.* Philip B. Taylor, Jr., p. 1314.

M. V. Naidu, *Collective Security and the United Nations: A Definition of the UN Security System.* Edward Thomas Rowe, p. 1315.

H. G. Nicholas, *The United States and Britain.* Lloyd C. Gardner, p. 1315.

Ernest Nolte, *Deutschland und der Kalte Krieg.* Wolfram F. Hanrieder, p. 1316.

Sam C. Nolutshungu, *South Africa in Africa: A Study of Ideology and Foreign Policy.* Timothy M. Shaw, p. 1318.

Edgar Camacho Omiste, *Integracion Andina.* Lynn Krieger Mytelka, p. 1319.

Charles S. Pearson, *International Marine Environment Policy: The Economic Dimension.* Jack C. Plano, p. 1320.

Chester Ronning, *A Memoir of China in Revolution: From the Boxer Rebellion to the People's Republic.* J. K. Fairbank, p. 1321.

Royal United Services Institute for Defence Studies, ed., *Defence Yearbook 1974.* Ruth Leger Sivard, p. 1322.

George O. Sayles, *The King's Parliament of England.* John D. Lees, p. 1323.

Jürgen Schlumbohm, *Freiheit – Die Anfänge der bürgerlichen Emanzipationsbewegung in Deutschland im Spiegel ihres Leitwortes.* Robert A. Pois, p. 1324.

Claudio G. Segrè, *Fourth Shore: The Italian Colonization of Libya.* Clara M. Lovett, p. 1325.

Kalman H. Silvert et al., *The Americas in a Changing World. Selected Papers.* William M. Berenson, p. 1326.

Robert R. Simmons, *The Strained Alliance: Peking, P'yongyang, Moscow, and the Politics of the Korean Civil War.* B. C. Koh, p. 1328.

W. Scott Thompson, *Unequal Partners, Philippine and Thai Relations With the United States 1965–1975.* H. B. Jacobini, p. 1330.

Constantine V. Vaitsos, *Intercountry Income Distribution and Transnational Enterprises.* John P. Renninger, p. 1331.

R. J. Vincent, *Nonintervention and International Order.* D. W. Bowett, p. 1332.

Ranyard West, *International Law and Psychology; Two Studies: The Intrusion of Order and Conscience and Society.* Kenneth D. Auerbach, p. 1332.

John White, *The Politics of Foreign Aid.* M. Glen Johnson, p. 1334.

J. Wilczynski, *Technology in Comecon: Acceleration of Technological Progress Through Economic Planning and the Market.* Bohdan Harasymiw, p. 1335.

Willem A. Veenhoven and Winifred Crum Ewing, eds., *Case Studies on Human Rights and Fundamental Freedoms: A World Survey.* Ernst B. Haas, p. 1336.

G. M. A. Grube, trans., *Plato's Republic.* Thomas L. Pangle, p. 1336.

The American Political Science Association

1527 New Hampshire Avenue, N.W., Washington, D.C. 20036

President: SAMUEL H. BEER, Harvard University; *President-Elect:* JOHN C. WAHLKE, University of Iowa; *Vice Presidents:* PHILIP E. CONVERSE, University of Michigan; MATTHEW HOLDEN, JR., Public Service Commission, State of Wisconsin; FRANK J. SORAUF, University of Minnesota; *Secretary:* FRED I. GREENSTEIN, Princeton University; *Treasurer:* DINA A. ZINNES, Indiana University; *Program Co-Chairmen:* HARVEY C. MANSFIELD, JR., Harvard University; JAMES Q. WILSON, Harvard University; *Executive Director:* EVRON KIRKPATRICK, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036; *Managing Editor, APSR:* NELSON W. POLSBY, University of California, Berkeley.

COUNCIL: (Above-named Officers *ex officiis*), 1976–1978: JAMES DAVID BARBER, Duke University; SUZANNE BERGER, Massachusetts Institute of Technology; JAMES B. CHRISTOPH, Indiana University; DANIEL J. ELAZAR, Temple University; HERBERT JACOB, Northwestern University; M. KENT JENNINGS, University of Michigan; INEZ SMITH REID, Barnard College; JAMES N. ROSENAU, University of Southern California; 1977–79: MARGUERITE ROSS BARNETT, Howard University; ROBERT T. HOLT, University of Minnesota; MARTIN LANDAU, University of California, Berkeley; DAVID R. MAYHEW, Yale University; ROBERT D. PUTNAM, University of Michigan; CLEMENT E. VOSE, Wesleyan University; JAMES Q. WILSON, Harvard University; BETTY H. ZISK, Boston University.

FORMER PRESIDENTS: FRANK J. GOODNOW, ALBERT SHAW, FREDERIC N. JUDSON, JAMES BRYCE, A. LAWRENCE LOWELL, WOODROW WILSON, SIMEON E. BALDWIN, ALBERT BUSHNELL HART, W. W. WILLOUGHBY, JOHN BASSETT MOORE, ERNST FREUND, JESSE MACY, MUNROE SMITH, HENRY JONES FORD, PAUL S. REINSCH, LEO S. ROWE, WILLIAM A. DUNNING, HARRY A. GARFIELD, JAMES W. GARNER, CHARLES E. MERRIAM, CHARLES A. BEARD, WILLIAM B. MUNRO, JESSE S. REEVES, JOHN A. FAIRLIE, BENJAMIN F. SHAMBAUGH, EDWARD S. CORWIN, WILLIAM F. WILLOUGHBY, ISIDOR LOEB, WALTER J. SHEPARD, FRANCIS W. COKER, ARTHUR N. HOLCOMBE, THOMAS REED POWELL, CLARENCE A. DYKSTRA, CHARLES GROVE HAINES, ROBERT C. BROOKS, FREDERIC A. OGG, WILLIAM ANDERSON, ROBERT E. CUSHMAN, LEONARD D. WHITE, JOHN M. GAUS, WALTER F. DODD, ARTHUR W. MACMAHON, HENRY R. SPENCER, QUINCY WRIGHT, JAMES K. POLLOCK, PETER H. ODEGARD, LUTHER H. GULICK, PENDLETON HERRING, RALPH J. BUNCHE, CHARLES MCKINLEY, HAROLD D. LASSWELL, E. E. SCHATTSCHNEIDER, V. O. KEY, JR., R. TAYLOR COLE, CARL B. SWISHER, EMMETTE S. REDFORD, CHARLES S. HYNEMAN, CARL J. FRIEDRICH, C. HERMAN PRITCHETT, DAVID B. TRUMAN, GABRIEL A. ALMOND, ROBERT A. DAHL, MERLE FAINSOD, DAVID EASTON, KARL W. DEUTSCH, ROBERT E. LANE, HEINZ EULAU, ROBERT E. WARD, AVERY LEISERSON, AUSTIN RANNEY, JAMES MACGREGOR BURNS.

The American Political Science Review

Department of Political Science, University of California, Berkeley 94720

EDITORIAL BOARD: NELSON W. POLSBY, *Managing Editor, University of California, Berkeley;* CHARLES O. JONES, *Managing Editor Elect, University of Pittsburgh;* PHILIP SIEGELMAN, *Book Review Editor, San Francisco State University;* SHLOMO AVINERI, *The Hebrew University of Jerusalem;* LUCIUS BARKER, *Washington University;* BRIAN BARRY, *University of British Columbia;* RICHARD A. BRODY, *Stanford University;* SAMUEL D. COOK, *Dillard University;* ELMER E. CORNWELL, *Brown University;* S. RUFUS DAVIS, *Monash University;* ROBERT J. JACKSON, *Carleton University;* ROBERT JERVIS, *University of California, Los Angeles;* DALE ROGERS MARSHALL, *University of California, Davis;* RUSSELL D. MURPHY, *Wesleyan University;* JOAN M. NELSON, *Washington, D.C.;* J. ROLAND PENNOCK, *Swarthmore College;* SAMUEL L. POPKIN, *University of California, San Diego;* ROBERT D. PUTNAM, *University of Michigan;* DOUGLAS W. RAE, *Yale University;* AUSTIN RANNEY, *American Enterprise Institute;* GIOVANNI SARTORI, *University of Florence;* MICHAEL J. SHAPIRO, *University of Hawaii;* JAY STARLING, *Southern Methodist University;* STEVEN V. STEPHENS, *Baltimore, Maryland;* GEORGE E. VON DER MUHLL, *University of California, Santa Cruz;* RICHARD A. WATSON, *University of Missouri;* RICHARD WINTERS, *Dartmouth College.* **MANUSCRIPT EDITOR:** ELLEN Y. SIEGELMAN. **EDITORIAL ASSISTANT:** BETSEY COBB. **EDITORIAL INTERNS:** ROBERT ARSENEAU, JONATHAN BENDOR, BETH CAPELL, DON CHISHOLM, DAVID FLANDERS, STEPHEN GENCO, EVELYN DEBORAH JAY, JOHN Q. JOHNSON, PAUL D. KARPS, JAMES I. LENGLE, DAVID B. MAGLEBY, DAVID RICHMAN, JENNY RING, STEVEN ROSENSTONE, STUART A. ROSS, SERGE TAYLOR, PHILIP J. WILSON, JOHN ZALLER.

About the American Political Science Review

The American Political Science Review, published quarterly during the months of March, June, September, and December, is supplied to all APSA members. Membership dues are charged according to the following schedule:

Membership Category	*Dues*
Regular	
Annual Income	
Under $12,000	$25
$12,000–14,999	$30
$15,000–19,999	$35
$20,000–24,999	$40
$25,000–29,999	$45
$30,000 and over	$50
Student and Retired	$12
Family	$ 5
Life	$1000
Institutional (includes *APSR* and *PS*)	$50 domestic $52 foreign

Student memberships are limited to five years per person. Foreign currency at the official exchange rate will be accepted for foreign subscriptions and foreign membership fees. The equivalent of $3 for additional postage should be added for foreign subscriptions.

Current issues are priced at $10.50 per copy; for *back issues prior* to 1970 address Walter J. Johnson, Inc., 355 Chestnut St., Norwood, New Jersey 07648; for issues 1970 and later send request directly to the American Political Science Association.

Applications for membership, orders for the *Review,* and remittances should be addressed to the Executive Director, The American Political Science Association, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036. *Notices of change of address* should be received in the Washington office by the 25th day of the months before publication.

INSTRUCTIONS TO CONTRIBUTORS

Address correspondence about contributions to the *Review* to Charles O. Jones, *American Political Science Review,* Department of Political Science, University of Pittsburgh, Pittsburgh, Pennsylvania 15260.

Please submit *four* copies of a manuscript to be considered for publication. *Unfortunately, due to increased postage costs, we can no longer return manuscripts to the author.* Manuscripts must be typed and mimeographed or photocopies, and with all written material double-spaced (including quotations). Please do not send the original typescript. Since manuscripts are sent out anonymously for evaluation, the author's name and affiliation should appear only on a separate covering sheet and all footnotes identifying the author should also appear on a separate sheet. An abstract of less than 150 words should accompany the manuscript.

The footnote form is that used by most scientific journals. Explanatory notes only will be permitted for most manuscripts (see below); references will be listed alphabetically at the end of the manuscript; and in-text references will be made by inserting author or title, year, and page number, if appropriate. For illustration see Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science* (Reading, Mass.: Addison-Wesley, 1975). Note: Authors of manuscripts on the history of political thought may, if they prefer, use the traditional footnote form when referring to the individual theorist's work; the reference form should be used when referring to secondary works.

All manuscripts will be examined carefully for length. Referees will be asked to recommend where cuts might be made, without affecting the principal contribution. Manuscripts over 30 pages will be reviewed with particular care to determine if such length is warranted. Except in extraordinary circumstances, manuscripts over 50 pages will be returned to the author. Resubmission may follow compliance with the page limit. Manuscripts must be prepared as noted above, double-spaced throughout, with standard margins. Tables, charts, and references will be included in the page count.

Communications must follow the form of regular manuscripts (including reference listings where appropriate). They must be brief and to the point.

Figures should be drawn on white paper with India ink and the original tracings or drawings retained by the author (for transmission later to the printer). Copies only should accompany the manuscript. Tables should be typed on a separate page. Insert a guide line, e.g., "Table 1 about here," at the appropriate place in the manuscript.

Address books intended for review to Paul A. Beck, Book Review Editor, *American Political Science Review,* Department of Political Science, Mervis Hall, University of Pittsburgh, Pittsburgh, Pennsylvania 15260. Information, including News and Notes, for the Association's news-journal, *PS,* should be sent to 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036; advertising reprint and circulation correspondence should be sent to the Executive Director at the Washington office. Domestic claims for non-receipt of issues must be made within six months of the month of publication; overseas claims, one year. Advertising information and rates are available from Elizabeth E. Horton, Adv. Manager, APSA, 1527 New Hampshire Avenue, N.W., Washington, D.C. 20036.

Articles and notes appearing in the *Review* before the June 1953 issues were indexed in *The Reader's Guide to Periodical Literature.* Current issues are indexed by *The International Political Science Abstracts* and the *Social Sciences and Humanities Index.* Microfilm of the *Review,* beginning with Volume 1, may be obtained from University Microfilms, 313 North First Street, Ann Arbor, Michigan 48106. A *Cumulative Index of the Review,* Volumes 1–62; 1906–1968, may also be obtained from University Microfilms. Articles appearing in the *Review* are listed regularly in *ABC Pol Sci* and *Current Contents: Behavioral, Social & Management Sciences.* Book reviews are indexed in *Book Review Index.*

U.S. House Members in Their Constituencies: An Exploration*

RICHARD F. FENNO, JR.
University of Rochester

Despite a voluminous literature on the subject of representative-constituency relationships, one question central to that relationship remains underdeveloped. It is: what does an elected representative see when he or she sees a constituency? And, as a natural follow-up, what consequences do these perceptions have for his or her behavior? The key problem is that of perception. And the key assumption is that the constituency a representative reacts to is the constituency he or she sees. The corollary assumption is that the rest of us cannot understand the representative-constituency relationship until we can see the constituency through the eyes of the representative. These ideas are not new. They were first articulated for students of the United States Congress by Lewis Dexter.[1] Their importance has been widely acknowledged and frequently repeated ever since. But despite the acceptance and reiteration of Dexter's insights, we still have not developed much coherent knowledge about the perceptions members of Congress have of their constituencies.

A major reason for this neglect is that most of our research on the representative-constituency linkage gets conducted at the wrong end of that linkage. Our interest in the constituency relations of U.S. senators and representatives has typically been a derivative interest, pursued for the light it sheds on some behavior – like roll call voting – in Washington. When we talk with our national legislators about their constituencies, we typically talk to them *in Washington* and, perforce, in the Washington context. But that is a context far removed from the one in which their constituency relationships are created, nurtured, and changed. And it is a context equally far removed from the one in which we might expect their perceptions of their constituencies to be shaped, sharpened or altered. Asking constituency-related questions on Capitol Hill, when the House member is far from the constituency itself, could well produce a distortion of perspective. Researchers might tend to conceive of a separation between the representative "here in Washington" and his or her constituency "back home," whereas the representative may picture himself or herself as a part of the constituency – me *in* the constituency, rather than me *and* the constituency. As a research strategy, therefore, it makes some sense to study our representatives' perceptions of their constituencies while they are actually in their constituencies – at the constituency end of the linkage.[2]

Since the fall of 1970, I have been traveling with some members of the House of Represen-

---

*My I.O.U.'s lie scattered all over the United States among my friends in and out of academia. They are too numerous to list here. But Theodore Anagnoson, Viktor Hofstetter, John Kingdon, and Herbert McClosky deserve special thanks. So do the Russell Sage Foundation and the University of Rochester for their financial help. A slightly different version of this article was delivered under the title "Congressmen in Their Constituencies: An Exploration," at the American Political Science Association Convention in San Francisco, September, 1975. It is a part of a larger study, *Home Style: U.S. House Members in their Constituencies* (Boston: Little Brown, forthcoming).

[1] Dexter's seminal article was "The Representative and His District," *Human Organization* 16 (Spring, 1957), 2–14. It will be found, revised and reprinted, along with other of Dexter's works carrying the same perspective in: Lewis Dexter, *The Sociology and Politics of Congress* (Chicago: Rand McNally, 1969).

[2] Political science studies conducted in congressional constituencies have been few and far between. The most helpful to me have been: *On Capitol Hill,* ed. John Bibby and Roger Davidson (New York: Holt, Rinehart and Winston, 1967); *On Capitol Hill,* ed. John Bibby and Roger Davidson (Chicago: Dryden, 1972); John Donovan, *Congressional Campaign: Maine Elects a Democrat* (New York: Holt, Rinehart and Winston, 1957); Charles Jones, "The Role of the Campaign in Congressional Politics," in *The Electoral Process,* ed. Harmon Zeigler and Kent Jennings (New York: Prentice-Hall, 1966); John Kingdon, *Candidates for Office: Beliefs and Strategies* (New York: Random House, 1966); David Leuthold, *Electioneering in a Democracy* (New York: Wiley, 1968). And no one interested in this subject should miss Richard Harris's superb "How's It Look?", *New Yorker,* April 8, 1967. The most recent study is: *The Making of Congressmen,* ed. Alan Clem (North Scituate: Duxbury, 1976).

tatives while they were in their districts, to see if I could figure out – by looking over their shoulders – what it is they see there. These expeditions, designed to continue through the 1976 elections, have been totally open-ended and exploratory. I have tried to observe and inquire into anything and everything the members do. Rather than assume that I already know what is interesting or what questions to ask, I have been prepared to find interesting questions emerging in the course of the experience. The same with data. The research method has been largely one of soaking and poking – or, just hanging around. This paper, therefore, conveys mostly an impressionistic feel for the subject – as befits the earliest stages of exploration and mapping.

As of June 1976, I had accompanied fourteen sitting House members, two House members-to-be and one House member-elect in their districts – for a minimum of two, a maximum of ten, and an average of five days each – sometimes at election time, sometimes not. In eleven cases I have accompanied the member on more than one trip; in six cases I have made only one trip. Since I am a stranger to each local context and to the constellation of people surrounding each member, my confidence in what I see and hear increases markedly when I can make similar observations at more than one point in time. In ten cases I have supplemented my trips to the district with a lengthy interview in Washington. In the district, I reconstruct my record from memory and from brief jottings, as soon after the event as is feasible. In Washington, I take mostly verbatim notes during the interview and commit them to tape immediately thereafter.

I have tried to find a variety of types of members and districts, but I make no pretense at having a group that can be called representative, much less a sample. The seventeen include nine Democrats and eight Republicans. Geographically, three come from two eastern states; six come from five midwestern states; three come from three southern states; five come from three far western states. Since I began, one has retired, one has been defeated and one has run for the Senate. There is some variation among them in terms of ideology, seniority, ethnicity, race, sex,[3] and in terms of safeness and diversity of district. But no claim is made that the group is ideally balanced in any of these respects.

Perceptions of the Constituency

The District: The Geographical Constituency. What then do House members see when they see a constituency? One way they perceive it – the way most helpful to me so far – is as a nest of concentric circles. The largest of these circles represents the congressman's broadest view of his constituency. This is "the district" or "my district." It is the entity to which, from which, and in which he travels. It is the entity whose boundaries have been fixed by state legislative enactment or by court decision. It includes the entire population within those boundaries. Because it is a legal entity, we could refer to it as the legal constituency. It captures more of what the congressman has in mind when he conjures up "my district," however, if we label it the *geographical constituency*. We retain the idea that the district is a legally bounded space and emphasize that it is located in a particular place.

The Washington community is often described as a group of people all of whom come from somewhere else. The House of Representatives, by design, epitomizes this characteristic; and its members function with a heightened sense of their ties to place. There are, of course, constant reminders. The member's district is, after all, "the Tenth District of *California*." Inside the chamber, he is "the gentleman from *California*"; outside the chamber he is Representative X (D. *California*). So, it is not surprising that when you ask a congressman, "What kind of district do you have?", the answer often begins with, and always includes, a geographical, space-and-place, perception. Thus, the district is seen as "the largest in the state, twenty-eight counties in the southeastern corner" or "three layers of suburbs to the west of the city, a square with the northwest corner cut out." If the boundaries have been changed by a recent redistricting, the geography of "the new district" will be compared to that of "the old district."

---

[3] The group contains sixteen men and one woman. In the title of the paper and in the Introduction, I have deliberately employed the generic language "House member," "member of Congress," "Representative," and "his or her" to make it clear that I am talking about men and women. And I have tried to use the same language wherever the plural form appears in the paper. That is, I have tried to stop using the word "congressmen." In the body of the paper, however, I shall frequently and deliberately use "congressman" and "his" as generic terms. Stylistically, I find this a less clumsy form of the third person singular than "congressperson," followed always by "his or her." This usage has the additional special benefit, here, of camouflaging the one woman in the group. Where necessary, I have used pseydonyms for these seventeen members in the text.

[4] Additional evidence will be found in: Donald R. Matthews and James A. Stimson, *Yeas and Nays: Normal Decision Making in the U.S. House of Representatives* (New York: Wiley, 1975), pp. 28–31.

If one essential aspect of "the geographical constituency" is seen as its location and boundaries, another is its particular internal make-up. And House members describe their districts' internal makeup using political science's most familiar demographic and political variables – socioeconomic structure, ideology, ethnicity, residential patterns, religion, partisanship, stability, diversity, etc. Every congressman, in his mind's eye, sees his geographical constituency in terms of some special configuration of such variables. For example,

> Geographically, it covers the northern one-third of the state, from the border of (state X) to the border of (state Y), along the Z river – twenty-two counties. The basic industry is agriculture – but it's a diverse district. The city makes up one-third of the population. It is dominated by the state government and education. It's an independent minded constituency, with a strong attachment to the work ethic. A good percentage is composed of people whose families emmigrated from Germany, Scandinavia and Czechoslovakia. I don't exactly know the figures, but over one-half the district is German. And this goes back to the work ethic. They are a hardworking, independent people. They have a strong thought of 'keeping the government off my back, we'll do all right here.' That's especially true of my out-counties.

Some internal configurations are more complex than others. But, even at the broadest level, no congressman sees, within his district's boundaries, an undifferentiated glob.[4] And we cannot talk about his relations with his "constituency" as if he did.

All of the demographic characteristics of the geographical constituency carry political implications. But as most Representatives make their first perceptual cut into "the district," political matters are usually left implicit. Sometimes, the question "what kind of district do you have?" turns up the answer "it's a Democratic district." But much more often, that comes later. It is as if they first want to sketch a prepolitical background against which they can later paint in the political refinements. We, of course, know – for many of the variables – just what those political refinements are likely to be. (Most political scientists would guess that the district just described is probably more Republican than Democratic – which it is.) There is no point to dwelling on the general political relevance of each variable. But one summary characterization does seem to have special usefulness as a background against which to understand political perceptions and their consequences. And that characteristic is the relative homogeneity or heterogeneity of the district.

As the following examples suggest, members of Congress do think in terms of the homogeneity or heterogeneity of their districts – though they may not always use the words.

> It's geographically compact. It's all suburban – no big city in the accepted sense of the word and no rural area. It's all white. There are very few blacks, maybe 2 per cent. Spanish surnamed make up about 10 per cent. Traditionally, it's been a district with a high percentage of home ownership.... Economically, it's above the national average in employment ... the people of the district are employed. It's not that it's very high income. Oh, I suppose there are a few places of some wealth, but nothing very wealthy. And no great pockets of poverty either. And it's not dominated by any one industry. The X County segment has a lot of small, clean, technical industries. I consider it very homogeneous. By almost any standard, it's homogeneous.

> This district is a microcosm of the nation. We are geographically southern and politically northern. We have agriculture – mostly soy beans and corn. We have big business – like Union Carbide and General Electric. And we have unions. We have a city and we have small towns. We have some of the worst poverty in the country in A County. And we have some very wealthy sections, though not large. We have wealth in the city and some wealthy towns. We have urban poverty and rural poverty. Just about the only thing we don't have is a good sized ghetto. Otherwise, everything you can have, we've got it right here.

Because it is a summary variable, the perceived homogeneity-heterogeneity characteristic is particularly hard to measure; and no metric is proposed here. Intuitively, both the number and the compatibility of significant interests within the district would seem to be involved. The greater the number of significant interests – as opposed to one dominant interest – the more likely it is that the district will be seen as heterogeneous. But if the several significant interests were viewed as having a single lowest common denominator and, therefore, quite compatible, the district might still be viewed as homogeneous. One indicator, therefore, might be the ease with which the congressman finds a lowest common denominator of interests for some large proportion of his geographical constituency. The basis for the denominator could be any of the prepolitical variables. We do not think of it, however, as a political characteristic – as the equivalent, for instance, of party registration or political safeness.[5] The proportion of people in the district who have to be included would be a subjective judgment – "enough" so that the congressman saw his geographical constituency as more homogeneous than heterogeneous, or vice versa. All we can say is that the less actual or potential conflict he sees among district interests, the more likely he is to see his district as homogeneous. Another indicator might be the extent to which the geographical constituency is congruent with a natural community. Districts that are purely artificial (sometimes purely political) creations of districting practices, and which pay no attention to pre-existing communities of interest are more likely to be heterogeneous.[6] Pre-existing communities or natural communities are more likely to have such homogenizing ties as common sources of communication, common organizations, and common traditions.

The Supporters: The Re-election Constituency. Within his geographical constituency, each congressman perceives a smaller, explicitly political constituency. It is composed of the people he thinks vote for him. And we shall refer to it as his *re-election constituency*. As he moves about the district, a House member continually draws the distinction between those who vote for him and those who do not. "I do well here"; "I run poorly here." "This group supports me"; "this group does not." By distinguishing supporters from nonsupporters, he articulates his baseline political perception.

House members seem to use two starting points – one cross sectional and the other longitudinal – in shaping this perception. First, by a process of inclusion and exclusion, they come to a rough approximation of the upper and lower ranges of the re-election constituency. That is to say, there are some votes a member believes he almost always gets; there are other votes he believes he almost never gets. One of the core elements of any such distinction is the perceived partisan component of the vote – party identification as revealed in registration or poll figures and party voting. "My district registers only 37 per cent Republican. They have no place else to go. My problem is, how can I get enough Democratic votes to win the general election." Another element is the political tendencies of various demographic groupings.

> My supporters are Democrats, farmers, labor – a DFL operation – with some academic types.... My opposition tends to be the main street hardware dealer. I look at that kind of guy in a stable town, where the newspaper runs the community – the typical school board member in the rural part of the district – that's the kind of guy I'll never get. At the opposite end of the scale is the country club set. I'll sure as hell never get them, either.

Starting with people he sees, very generally, as his supporters, and leaving aside people he sees, equally generally, as his nonsupporters, each congressman fashions a view of the people who give him his victories at the polls.

The second starting point for thinking about the re-election constituency is the congressman's idea of who voted for him "last time." Starting with that perception, he adds or subtracts incrementally on the basis of changes that will have taken place (or could be made by him to take place) between "last time" and "next time." It helps him to think about his re-election constituency this way because that is about the only certainty he operates with – he won last time. And the process by which his desire for re-election gets translated into his perception of a re-election constituency is filled with uncertainty. At least that is my strong impression. House members see re-election uncertainty where political scientists would fail to unearth a single objective indicator of it. For one thing, their perceptions of their supporters and nonsupporters are quite diffuse. They rarely feel certain just who did vote for them last time. And even if they do feel fairly sure about that, they may perceive population shifts that threaten established calculations. In the years of my travels, moreover, the threat of redistricting has added enormous uncertainty to the make-up of some re-election constituencies. In every district, too, there is the uncertainty which follows an unforeseen external event – recession, inflation, Watergate.

Of all the many sources of uncertainty, the most constant – and usually the greatest – involves the electoral challenger. For it is the challenger who holds the most potential for altering any calculation involving those who voted for the congressman "last time." "This time's" challenger may have very different sources of political strength from "last time's" challenger. Often, one of the major off-year uncertainties is whether or not the last challenger will try again. While it is true that House members campaign all the time, "the campaign" can be said to start only when the challenger is known. At that point, a redefinition of the re-election constituency may have to take place. If the challenger is chosen by primary, for example, the congressman may inherit support from the loser.[7] A conservative southern Republican, waiting for the Democratic primary to determine whether his challenger would be a black or a white (both liberal), wondered about the shape of his re-election constituency.

---

[5] Marginal districts probably tend to be heterogeneous. Safe districts probably are both heterogeneous and homogeneous. On the relationship of electoral conditions to homogeneity and heterogeneity, I owe a lot to my conversations with Morris Fiorina. See his *Representatives, Roll Calls and Constituencies* (Boston: Lexington, 1974).

[6] On the usefulness of the distinction, see Donald Stokes and Warren Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly* 26 (Winter, 1962), 531–546. In M. Barone, G. Ujifusa, and D. Matthews, *Almanac of American Politics* (Boston: Gambit, 1974), the authors often describe congressional districts as artificial and containing no natural community of interest. See for example, pages 86, 133, 146, 162, 376, 402, 419, 373, 616.

> It depends on my opponent. Last time, my opponent (a white moderate) and I split many groups. Many business people who might have supported me, split up. If I have a liberal opponent, all the business community will support me.... If the black man is my opponent, I should get more Democratic votes than I got before. He can't do any better there than the man I beat before. Except for a smattering of liberals and radicals around the colleges, I will do better than last time with the whites.... The black vote is 20 per cent and they vote right down the line Democratic. I have to concede the black vote. There's nothing I can do about it.... [But] against a white liberal, I would get some of the black vote.

The shaping of perceptions proceeds under conditions of considerable uncertainty.

The Strongest Supporters: The Primary Constituency. In thinking about their political condition, House members make distinctions within their re-election constituency – thus giving us a third, still smaller concentric circle. Having distinguished between their nonsupporters and their supporters, they further distinguish between their routine or temporary supporters and their very strongest supporters. Routine supporters only vote for them, often merely following party identification; but others will support them with a special degree of intensity. Temporary supporters back them as the best available alternative; but others will support them regardless of who the challenger may be. Within each re-election constituency are nested these "others" – a constituency perceived as "my strongest supporters," "my hard core support," "my loyalists," "my true believers," "my political base." We shall think of these people as the ones each congressman believes would provide his best line of electoral defense in a primary contest, and label them *the primary constituency.*[8] It will probably include the earliest of his supporters – those who recruited him and those who tendered identifiably strong support in his first campaign – thus, providing another reason for calculating on the basis of "last time." From its ranks will most likely come the bulk of his financial help and his volunteer workers. From its ranks will least likely come an electoral challenger.

A protected congressional seat is as much one protected from primary defeat as from general election defeat. And a primary constituency is something every congressman must have.

> Everybody needs some group which is strongly for him – especially in a primary. You can win a primary with 25,000 zealots.... The most exquisite case I can give you was in the very early war years. I had very strong support from the anti-war people. They were my strongest supporters and they made up about 5 per cent of the district.

The primary constituency, I would guess, draws a special measure of a congressman's interest; and it should, therefore, draw a special measure of ours. But it is not easy to delineate – for us or for them. Asked to describe his "very strongest supporters," one member replied, "That's the hardest question anyone has to answer." The primary constituency is more subtly shaded than the re-election constituency, where voting provides an objective membership test. Loyalty is not the most predictable of political qualities. And all politicians resist drawing invidious distinctions among their various supporters, as if it were borrowing trouble to begin classifying people according to fidelity. House members who have worried about or fought a primary recently may find it somewhat easier. So, too may those with heterogeneous districts whose diverse elements invite differentiation. Despite some difficulty, most members – because it is politically prudent to do so – make some such distinction, in speech or in action or both. By talking to them and watching them, we can begin to understand what those distinctions are.

Here are two answers to the question, "who are your very strongest supporters?"

> My strongest supporters are the working class – the blacks and labor, organized labor. And the

---

[7] See Donald B. Johnson and James R. Gibson, "The Divisive Primary Revisited: Party Activists in Iowa," *American Political Science Review* 68 (March, 1974), 67–77.

[8] The term is that of Leo Snowiss, "Congressional Recruitment and Representation," *American Political Science Review* 60 (September, 1966), 627–639.

> people who were in my state legislative district, of course. The fifth ward is low-income, working class and is my base of support. I grew up there; I have my law office there; and I still live there. The white businessmen who are supporting me now are late converts – very late. They support me as the least of two evils. They are not a strong base of support. They know it and I know it.
>
> I have a circle of strong labor supporters and another circle of strong business supporters. . . . They will 'fight, bleed and die' for me, but in different ways. Labor gives you the manpower and the workers up front. You need them just as much as you need the guy with the two-acre yard to hold a lawn party to raise money. The labor guy loses a day's pay on election day. The business guy gets his nice lawn tramped over and chewed up. Each makes a commitment to you in his own way. You need them both.

Each description reveals the working politician's penchant for inclusive thinking. Each tells us something about a primary constituency, but each leaves plenty of room for added refinements.

The best way to make such refinements is to observe the congressman as he comes in contact with the various elements of his re-election constituency. Both he and they act in ways that help delineate the "very strongest supporters." For example, the author of the second comment above drew a standing ovation when he was introduced at the Labor Temple. During his speech, he spoke directly to individuals in the audience. "Kenny, it's good to see you here. Ben, you be sure and keep in touch." Afterward, he lingered for an hour drinking beer and eating salami. At a businessman's annual Christmas luncheon the next day, he received neither an introduction nor applause when the main speaker acknowledged his presence, saying, "I see our congressman is here; and I use the term 'our' loosely." This congressman's "circle of strong labor supporters" appears to be larger than his "circle of strong business supporters." And the congressman, for his part, seemed much more at home with the first group than he did with the second.

Like other observers of American politics, I have found this idea of "at homeness" a useful one in helping me to map the relationship between politicians and constituents[9] – in this case the perception of a primary constituency. House members sometimes talk in this language about the groups they encounter.

> I was born on the flat plains, and I feel a lot better in the plains area than in the mountain country. I don't know why it is. As much as I like Al [whom we had just lunched with in a mountain town], I'm still not comfortable with him. I'm no cowboy. But when I'm out there on that flat land with those ranchers and wheat farmers, standing around trading insults and jibes and telling stories, I feel better. That's the place where I click.

It is also the place where he wins elections – his primary constituency. "That's my strong area. I won by a big margin and offset my losses. If I win next time, that's where I'll win it – on the plains." Obviously, there is no one-to-one relationship between the groups with whom a congressman acts and feels most at home and his primary constituency. But it does provide a pretty good unobtrusive clue.[10] So I found myself fashioning a highly subjective "at homeness index" to rank the degree to which each congressman seems to have support from and rapport with each group.

I recall, for example, watching a man whose constituency is dominantly Jewish participating in an afternoon installation-of-officers ceremony at a Young Men's Hebrew Association attended by about forty civic leaders of the local community. He drank some spiked punch, began the festivities by saying, "I'm probably the first tipsy installation officer you ever had," told an emotional story about his own dependence on the Jewish "Y," and traded banter with his friends in the audience throughout the proceedings. That evening as we prepared to meet with yet another and much larger (Democratic party) group, I asked where we were going. He said, "We're going to a shitty restaurant to have a shitty meal with a shitty organization and have a shitty time." And he did – from high to low on the "at homeness index." On the way home, after the meal, he talked about the group.

> Ethnically, most of them are with me. But I don't always support the party candidate, and they can't stand that. . . . This group and half the other party groups in the district are against me. But they don't want to be against me too strongly for fear I might go into a primary and

---

[9] For example, Theodore H. White, *The Making of the President: 1960* (New York: Atheneum, 1961), pp. 276–278; Marshall Frady, *Wallace* (New York: New American Library, 1969), pp. 2–4, 38–39; Theodore H. White, *The Making of the President: 1972* (New York: Atheneum, 1973), pp. 147, 414–415, 456–457.

[10] On recent trips, wherever possible, I have also asked each congressman at the end of my visit, to rank order the events of the visit in terms of their "political importance" to him and in terms of the degree to which he felt "at home" or "comfortable" in each situation.

> beat them. So self-preservation wins out.... They know they can't beat me.

Both groups are Jewish. The evening group was a part of his re-election constituency, but not his primary constituency. The afternoon group was a part of both.

The Intimates: The Personal Constituency. Within the primary constituency, each member perceives still a fourth, and final, concentric circle. These are the few individuals whose relationship with him is so personal and so intimate that their relevance to him cannot be captured by their inclusion in any description of "very strongest supporters." In some cases they are his closest political advisers and confidants. In other cases, they are people from whom he draws emotional sustenance for his political work. We shall think of these people as his *personal constituency*.

One Sunday afternoon, I sat in the living room of a congressman's chief district staff assistant watching an NFL football game – with the congressman, the district aide, the state assemblyman from the congressman's home county, and the district attorney of the same county. Between plays, at halftime and over beer and cheese, the four friends discussed every aspect of the congressman's campaign, listened to and commented on his taped radio spots, analyzed several newspaper reports, discussed local and national personalities, relived old political campaigns and hijinks, discussed their respective political ambitions. Ostensibly they were watching the football game; actually the congressman was exchanging political advice, information, and perspectives with three of his six or seven oldest and closest political associates.

Another congressman begins his weekends at home by having a Saturday morning 7:30 coffee and doughnut breakfast in a cafe on the main street of his home town with a small group of old friends from the Rotary Club. The morning I was there, the congressman, a retired bank manager, a hardware store owner, a high school science teacher, a retired judge, and a past president of the city council gossiped and joked about local matters – the county historian, the library, the board of education, the churches and their lawns – for an hour. "I guess you can see what an institution this is," he said as we left. "You have no idea how invaluable these meetings are for me. They keep me in touch with my home base. If you don't keep your home base, you don't have anything."

The personal constituency is, doubtless, the most idiosyncratic of the several constituencies. Not all members will open it up to the outside observer. Nine of the seventeen did, however; and in doing so, he usually revealed a side of his personality not seen by the rest of his constituencies. "I'm really very reserved, and I don't feel at home with most groups – only with five or six friends," said the congressman after the football game. The relationship probably has both political and emotional dimensions. But beyond that, it is hard to generalize, except to say that the personal constituency needs to be identified if our understanding of the congressman's view of his constituency is to be complete.

In sum, my impression is that House members perceive four constituencies – geographical, re-election, primary, and personal – each one nesting within the previous one.

Political Support and Home Style

What, then, do these perceptions have to do with a House member's behavior? Our conventional paraphrase of this question would read: what do these perceptions have to do with behavior at the other end of the line – in Washington? But the concern that disciplines the perceptions we have been talking about is neither first nor foremost a Washington-oriented concern. It is a concern for political support at home. It is a concern for the scope of that support – which decreases as one moves from the geographical to the personal constituency. It is a concern for the stability of that support – which increases as one moves from the geographical to the personal constituency. And it ultimately issues in a concern for manipulating scopes and intensities in order to win and hold a sufficient amount of support to win elections. Representatives, and prospective representatives, think about their constituencies because they seek support there. They want to get nominated and elected, then renominated and re-elected. For most members of Congress most of the time, this electoral goal is primary. It is the prerequisite for a congressional career and, hence, for the pursuit of other goals. And the electoral goal is achieved – first and last – not in Washington but at home.

Of course, House members do many things in Washington that affect their electoral support at home.[11] Political scientists interpret a great deal of their behavior in Washington in exactly that way – particularly their roll-call votes. Obviously, a congressman's perception of his several constituencies will affect such things as his roll-call voting, and we could, if we wished, study the effect. Indeed, that is the very direction in which our conditioned research reflexes would normally carry this investigation. But my experience has turned me in another – though not, as we shall see an unrelated – direction. I have been watching House members work to maintain or enlarge their political support at home, by going to the district and doing things there.

---

[11] Two of the best studies ever on the point are David Mayhew, *Congress: The Electoral Connection* (New Haven: Yale University Press, 1974), and John Kingdon, *Congressmen's Voting Decisions* (New York: Harper and Row, 1973).

Our Washington-centered research has caused us systematically to underestimate the proportion of their working time House members spend in their districts. As a result, we have also underestimated its perceived importance to them. In all our studies of congressional time allocation, time spent outside of Washington is left out of the analysis. So, we end up analyzing "the average work week of a congressman" by comparing the amounts of time he spends in committee work, on the floor, doing research, handling constituent problems – but all of it in Washington.[12] Nine of my members whose appointment and travel records I have checked carefully for the year 1973 – a nonelection year – averaged 28 trips to the district and spent an average of 101 working (not traveling) days in their districts that year. A survey conducted in 419 House offices covering 1973, indicates that the average number of trips home (not counting recesses) was 35 and the number of days spent in the district during 1973 (counting recesses) was 138.[13] No fewer than 131, nearly one-third, of the 419 members went home to their districts *every single weekend.* Obviously, the direct personal cultivation of their various constituencies takes a great deal of their time; and they must think it is worth it in terms of winning and holding political support. If it is worth *their* time to go home so much, it is worth *our* time to take a commensurate degree of interest in what they do there and why.

As they cultivate their constituencies, House members display what I shall call their *home style.* When they discuss the importance of what they are doing, they are discussing the importance of *home style* to the achievement of their electoral goal. At this stage of the research, the surest generalization one can make about home style is that there are as many varieties as there are members of Congress. "Each of us has his own formula – a truth that is true for him," said one. It will take a good deal more immersion and cogitation before I can improve upon that summary comment. At this point, however, three ingredients of home style appear to be worth looking at. They are: first, the congressman's allocation of his personal resources and those of his office; second, the congressman's presentation of self; and third, the congressman's explanation of his Washington activity. Every congressman allocates, presents, and explains. The amalgam of these three activities for any given representative constitutes (for now, at least) his home style. His home style, we expect, will be affected by his perception of his four constituencies.

Home Style: Allocation of Resources

Every representative must make a basic decision with regard to his home style: "How much and what kinds of attention shall I pay to home?" Or to put it another way: "Of all the resources available with which to help me do my job, which kinds and how much of each do I want to allocate directly to activity in the district?" There are, of course, many ways to allocate one's resources so that they affect the district. Our concern is with *resources allocated directly to the district.* Of these, we propose to look first, at the congressman's *time* and second, at the congressman's *staff.* The congressman's decision about how much time he should spend physically at home and his decision about how much of his staff he should place physically in the district are decisions which give shape to his home style.

Of all the resources available to the House member, the scarcest and most precious one, which dwarfs all others in posing critical allocative dilemmas, is his time. Time is at once what the member has least of and what he has the most control over. When a congressman divides up his time, he decides by that act what kind of congressman he wants to be. He must divide his time in Washington. He must divide his time at home. The decision we are concerned with here is the division of his time between Washington and home. When he is doing something at home, he must give up doing some things in Washington, and vice versa. So he chooses and he trades off; and congressmen make different allocative choices and different allocative trades. In this section, we shall focus on the frequency with which various congressmen returned to their districts in 1973.

---

[12] The basic research was done by John Saloma, and is reported in his *Congress and the New Politics* (Boston: Little Brown, 1969), Chapter 6; in Donald Tacheron and Morris Udall, *The Job of the Congressman* (Indianapolis: Bobbs Merrill, 1966), pp. 280–288; and in *Guide to the Congress of the United States* (Washington: Congressional Quarterly, 1971), pp. 532ff. But no one has expanded Saloma's work. A pioneer work, which would have given us a wider perspective, but which seems to have been neglected is Dorothy H. Cronheim, "Congressmen and Their Communication Practices" (Ph.D. dissertation, University of Michigan, Department of Political Science, 1957).

[13] These surveys will be described in the next section of the paper. We shall not, however, again use the figures on total number of days spent in the district. They seem less reliable than the others, when checked against the few cases in which I have the complete record. Also, it should be noted that the number of cases for which the total number of days was collected was 401.

"This is a business, and like any business you have to make time and motion studies," said one member. "All we have is time and ourselves, so we have to calculate carefully to use our time productively." It is not true, of course, that "all" the congressman has is time and himself. The office carries with it a large number of ancillary resources – a staff, office space, office expense allowances, free mailing privileges, personal expense allowances, etc. – all of which draw attention when the advantages of incumbency are detailed. Each congressman chooses how he will utilize these resources. The most important of these choices are choices about how to use his staff. And among the key choices about staff is how to allocate them between Washington and the district. In this section we shall focus particularly on one indicator of that decision – the *percentage of his total expenditures on staff salaries allocated by him to the salaries of his district staff*.[14]

The information on trips home and staff allocation was collected on Capitol Hill in June 1974. Six students, each of whom had just finished working for four months, full-time in a congressman's office, conducted a survey by visiting each member's office and talking to his or her administrative assistant or personal secretary. The question about trips home usually produced an educated estimate.[15] The questions about staff yielded more precise answers. Each student presented the Clerk of the House's *Report* for 1973, with its list of each representative's staff members and their salaries; and the respondents simply designated which staff members were located in the district. A briefer, follow-up survey was conducted by four students with similar "Hill experience" in May 1975. This survey added to the store of information on those 1973 members who were still in Congress.[16] For 1973, it should be noted, each member was allowed a maximum of sixteen staff members and a maximum payroll of $203,000. And for the two-year period 1973–1974 (the 93rd Congress) each member was reimbursed for thirty-six round trips to his district. Members were not, of course, required to use any of these allowances to the maximum.

On the matter of trips home, there is evidence of personal attentiveness to the district and of variation in that attentiveness. The average number of 1973 trips per member was thirty-five, and the median was thirty. The range went from a low of four trips to a high of three hundred and sixty-five.[17] One can ask: "for which kinds of House members is their frequent physical presence in the district an important part of their home style and for which kinds is it less so?" At this early stage, we can present only a few suggestive relationships. In order to do so, we have categorized the frequency of trips home into *low* (less than 24), medium (24–42), and high (more than 42). The categories are based on the responses to the question and to the appearance of reasonable cutting points in the data.[18] These categories have been cross-tabulated with a number of variables that should be expected to correlate with the frequency of home visits.

---

[14] Other kinds of data were collected which might also be useful as an indication of district staff strength. Three of them correlated very highly with the indicator being used, so that it does not appear we are missing much by relying on one indicator. The measure we are using in the article – per cent of staff expenditures allocated to district staff – showed a correlation of .861 with number of people on the district staff, of .907 with the per cent of total staff members allocated to the district, and of .974 with the dollar amounts spent on district staff. Also recorded were the rank, in the total staff hierarchy, of the highest paid person on the district staff, as another indicator of district staff allocation practices. That indicator has not been used in the article, but it might be noted that the range is from first (i.e., the highest paid district staffer is the highest paid of all the congressman's staffers) to more than ninth (anything above nine was not recorded).

[15] See footnote 18.

[16] The 1974 interviewers were Larry Fishkin, Nancy Hapeman, Bruce Pollock, Kenneth Sankin, Fred Schwartz, and Jacob Weinstein. The 1975 interviewers were Joel Beckman, Joanne Doroshow, Arthur Kreeger, and Susan Weiner. Sandra Bloch, Fishkin, Hapeman, and Schwartz helped with the analysis. During the later stages of the analysis, I have leaned particularly heavily on Viktor Hofstetter.

[17] There were eleven members who went home every night – eight from Maryland, two from Virginia, and one from Pennsylvania. In computing averages, they were coded at ninety-eight trips (more than anyone else) rather than at 365, so as to minimize distortion. Also, so as to minimize distortion, caused by these cases, we have used the *median* number of trips in the analysis of this section rather than the average number of trips.

[18] The most common replies were "every week," "once a month," "twice a month," "every other

One standard supposition would be that representatives in electoral jeopardy will decide to spend more of their time at home than will representatives whose seats are well protected. As a generalization, however, this supposition receives no confirmation when our conventional measures of electoral safeness are used. As Table 1 shows, the frequency of trips home does not increase as electoral margins decrease. Indeed, there is just not much of a relationship at all.[19] It might be noted, in this connection, that objective measures of marginality have not fared particularly well in producing consistent findings whenever they have been used.[20] My own experience leads me to believe that only *subjective* measures of electoral safety are valid. House members feel more uncertainty about re-election than is captured by any arbitrary electoral margin figures. Furthermore, uncertainty about their primary election situation is totally untouched by such figures. The point is that subjective assessments of electoral safeness might be more strongly correlated with trips home.

A related hunch would be that the longer a congressman is in office, the less time he will spend at home. Part of the argument here overlaps with the previous one – the longer in office, the more secure the seat. But the more important part of the reasoning would be that with seniority comes increased influence and responsibility in the House and, hence, the need to spend more time in Washington. These suppositions are supported by our data – but not as strongly and as consistently as we had imagined would be the case. A simple correlation between terms of service and number of trips home shows that as seniority increases, home visits decrease – as we would expect. But the correlation coefficient (Pearson's r) is an exceedingly weak –.235. When the data are grouped, however, the nature and strength of the relationship becomes clearer.

Table 2 pictures the relationship between the three categories of personal attentiveness and three levels of seniority. The summary statistics continue to be unimpressive, because for the middle levels of seniority no allocative pattern is evident. But looking at the lowest and highest levels of seniority, it is clear that the frequency of home visits is much greater for the low seniority group than it is for the high seniority group. The relationship between length of service and trips home, we conclude, is not a consistent, linear relationship. But for those at the beginning of their House careers and those farthest along in their House careers, their longevity is likely to be one determinant of their decisions on time allocation. Congressional newcomers appear to be more single-minded in pursuing of the electoral goal than are the veterans of the institution.

A third reasonable guess would be that the more time-consuming and expensive it is to get to his district, the less frequently a congressman will make the trip. Leaving money aside (but recalling that for 1973–1974, each member was provided with a "floor" of thirty-six trips), we would expect to find that as distance from Washington increases, the number of trips home decreases. It is not easy to get a measure of distance that captures each member's travelling time. For now, we shall use region as a surrogate for distance, on the theory that if any relationship is present, it will show up in a regional breakdown. And it does. Table 3 indicates that the members nearest Washington, D.C. (East) spend a good deal more time at home than do the members who live farthest away from the Capitol (Far West). The less of his Washington time a member has to give up in order to get home, the more likely he is to go

---

week," "between once and twice a month," "three or four times a month," etc. Congressmen placed in the "low" category were those whose staffers were unwilling to go as high as "twice a month." Congressmen whose trips were reported as "once a week" or more fell into the "high" category. But some respondents said "every week except for a few" or "every week, but maybe he missed one or two here or there." So, we decided to try to capture that sense by including in the "high" category people who were reported to have made somewhat less than fifty-two trips. (Doubtless, those who said they went home every week missed a few too.) Since a sizeable group had forty trips and none had forty-one or forty-two, the cut was made at that point, which made 43+ the "high category. The middle category were those who remained – people who went home at least twice a month (twenty-four trips) but not as often as forty-three times in 1973.

[19] The frequency of 1973 home visits does not bear any relationship to whether the member's electoral margin declined, increased, or remained the same between 1972 and 1974.

[20] See Fiorina, *Representatives, Roll Calls and Constituencies,* Chapter One. For an analysis of why House members ought to worry, see Robert Erikcson, "A Reappraisal of Competition for Congressional Office: How Careers Begin and End," paper presented at Conference on Mathematical Models of Congress, Aspen, Colorado, 1974. The one piece of research which dovetails best with my research is Warren Miller, "Majority Rule and the Representative System of Government" in *Cleavages, Ideologies and Party Systems,* ed. Erik Allardt and Yrjo Littunen (Helsinki, 1964), Chapter 10. Miller uses subjective marginality as the measure of competitiveness and relates it to the policy attitudes of the congressman and his re-election constituency.

Table 1. Trips Home and Electoral Margin

Election Margin (1972)	Frequency of Trips Home (1973) Low (0–23)	Medium (24–42)	High (43+)	Total
Less than 55%	21 (29%)	28 (38%)	24 (33%)	73 (100%)
55–60%	18 (32%)	18 (32%)	20 (36%)	56 (100%)
61–65%	22 (27%)	17 (20%)	44 (53%)	83 (100%)
More than 65%	68 (33%)	66 (32%)	73 (35%)	207 (100%)
	129	129	161	419

Note: Gamma = –.03

home – at least at the extremes of distance. If distance is a factor for the other three regional groups, it does not show up here. Our guess is that the distance is a far more problematical factor in those cases. We shall, however, return to the regional variable shortly.

A persistent dilemma facing every member of Congress involves the division of time between work and family. And one of its earliest manifestations comes with the family decision whether to remain at home or move to Washington. If (for whatever reason) the family decides to remain in the district, we would expect the House member to go home more often than if the family moves to Washington. Table 4 shows that this is very much the case.[21] Five times the percentage of representatives whose families remain at home fall into the high category of trips home as do represen-

Table 2. Trips Home and Seniority

Seniority	Frequency of Trips Home Low (0–23)	Medium (24–42)	High (43+)	Total
Low (1–3 terms)	34 (22%)	44 (28%)	78 (50%)	156 (100%)
Medium (4–7 terms)	43 (28%)	59 (38%)	52 (34%)	154 (100%)
High (8+ terms)	52 (48%)	26 (24%)	31 (28%)	189 (100%)
	129	129	161	419
Mean seniority	7.0 terms	5.0 terms	4.7 terms	

Note: Gamma = –.30

Table 3. Trips Home and Region

Region	Frequency of Trips Home Low (0–23)	Medium (24–42)	High (43+)	Total
East	5 (5%)	20 (20%)	76 (75%)	101 (100%)
South	36 (35%)	32 (30%)	36 (35%)	104 (100%)
Border	9 (26%)	10 (29%)	16 (45%)	35 (100%)
Midwest	29 (28%)	47 (44%)	30 (28%)	106 (100%)
Far West	50 (69%)	20 (27%)	3 (4%)	73 (100%)
	129	129	161	419

Regions:
East: Conn., Me., Mass., N.H., N.J., N.Y., Pa., R.I., Vt.
South: Ala., Ark., Fla., Ga., La., Miss., N.C., S.C., Tenn., Texas, Va.
Border: Del., Ky., Md., Mo., Okla., W.Va.
Midwest: Ill., Ind., Iowa, Kans., Mich., Minn., Neb., N.D., Ohio, S.D., Wisc.
Far West: Alaska, Ariz., Calif., Colo., Hawaii., Idaho, Mont., Nev., N.M., Oregon, Utah, Wash., Wyo.

---

[21] The number of cases is lower here than for the other parts of the analysis because the data were collected in 1975 – after a number of the 1973 congressmen were no longer available for questioning.

Table 4. Trips Home and Family Residence

Family Residence	Frequency of Trips Home Low (0–23)	 Medium (24–42)	 High (43+)	Total
Washington area	87 (41%)	89 (42%)	37 (17%)	213 (100%)
District	3 (4%)	6 (8%)	69 (88%)	78 (100%)
Unmarried	5 (14%)	12 (32%)	20 (54%)	37 (100%)
	95	107	126	328

tatives whose families are in Washington – 88 per cent to 17 per cent. To put the finding somewhat differently, the average number of 1973 trips for members with families in Washington was twenty-seven, for members with families in the district it was fifty-two, and for unmarried members it was forty-four trips. Whether family decisions produce the home style or whether a home style decision produces the family decision remains an unanswered question. Either process seems perfectly plausible. It is clear, however, that some decisions about home style are family-related decisions.

To sum up, a House member's decision on how to allocate his time between home and Washington is affected: (1) by his seniority, if it is very low or very high; (2) by the distance from Washington to home, if that distance is very long or very short; and (3) by the place where his family is located, whether his family moves to Washington or remains in the district. A congressman's electoral margin, objectively measured, has little effect on his time allocations. How, if at all, these factors are interrelated, and how strongly each factor contributes to the allocative pattern are matters for later analysis.

Members of Congress also decide what kind of staff presence they wish to establish in the district. Here, too, we find great variation. On the percentage of total staff expenditure allocated to district staff, the range, in 1973, went from 0 to 81 per cent. We might think that a member who decides to give a special degree of personal attention to "home" would also decide to give a special degree of staff attentiveness to "home." But the relationship between the two allocative decisions does not appear to be strong. Using percentage of total staff expenditure on district staff (as we shall throughout this section) as the measure of *district staff strength*, we find a very weak correlation (Pearson's r = .20) between a congressman's decision on that matter and the number of trips he takes home. Table 5 clusters and cross-tabulates the two allocative decisions. For our measure of district staff strength we have divided the percentage of expenditures on district staff into thirds. The lowest third ranges from 0–22.7 per cent; the middle third ranges from 22.8–33.5 per cent; the highest third ranges from 33.6–81 per cent. The cross-tabulation also shows a pretty weak overall relationship. For now, therefore, we shall treat the two decisions as if they were made independently of one another and, hence, are deserving of separate examination.

What kinds of members, then, emphasize the value of a large district staff operation? Once again, it turns out, they are not members in special electoral trouble. Table 6 displays the total lack of any discernible impact of electoral situation (objectively measured) on district staff strength. Nor, as indicated in Table 7, does seniority make any difference in staff allocative decisions. That it does not adds strength to the idea that the relationship between seniority and home visits discovered earlier is accounted for – as we have suggested – by career-and-goal factors rather than by electoral factors.

Table 5. Trips Home and District Staff Expenditures

District Staff Expenditures	Frequency of Trips Home Low (0–23)	 Medium (24–42)	 High (43+)	Total
Lowest 1/3	53 (39%)	42 (31%)	40 (30%)	135 (100%)
Middle 1/3	42 (30%)	55 (40%)	41 (30%)	138 (100%)
Highest 1/3	31 (23%)	32 (23%)	75 (54%)	138 (100%)
	126	129	156	411

Note: Gamma = .28

Table 6. District Staff Expenditures and Electoral Margin

Electoral Margin 1972	District Staff Expenditures (1973)			
	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Less than 55%	20 (27%)	28 (38%)	26 (35%)	74 (100%)
55–60%	20 (36%)	20 (36%)	16 (28%)	56 (100%)
61–65%	24 (29%)	29 (35%)	29 (35%)	82 (100%)
More than 65%	72 (36%)	61 (30%)	68 (34%)	201 (100%)
	136	138	139	413

Note: Gamma = –.04

Table 7. District Staff Expenditures and Seniority

Seniority	District Staff Expenditures			
	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Low (1–3 terms)	44 (29%)	54 (35%)	56 (36%)	155 (100%)
Medium (4–7 terms)	44 (29%)	56 (37%)	51 (34%)	151 (100%)
High (8+ terms)	47 (44%)	28 (26%)	32 (30%)	107 (100%)
	136	138	139	413

Note: Gamma = –.13

Table 8. District Staff Expenditures and Family Residence

Family Residence	District Staff Expenditures			
	Lowest 1/3	Middle 1/3	Highest 1/3	Total
Washington area	85 (40%)	77 (37%)	48 (23%)	210 (100%)
District	21 (28%)	17 (23%)	37 (49%)	75 (100%)
Unmarried	8 (22%)	10 (28%)	18 (50%)	36 (100%)
	114	104	103	321

The other variables discussed earlier – family residence and distance – do not have the obvious implications for staff allocation that they have for the member's own time dilemmas. It might be that if his family is in the district, and if he plans to be home a lot, a congressman might decide to have a big district staff operation to work with him there. That supposition receives some support in Table 8. Another possibility is that members might decide to use a strong district staff to compensate for their lack of personal attention via trips home. However, this idea is not supported in Table 8, nor in Table 9, which seeks to uncover regional and distance patternings of district staffs. Representatives who live nearest to Washington (East) and who tend to go home the most do tend to have large district staffs. But representatives who live farthest away and tend to go home the least show only a slight tendency to compensate by allocating heavy expenditures to their district staffs.

Table 9, however, does reveal some regional allocation patterns that did not appear when we looked for regional patterns in home visits. Region, it appears, captures a good deal more than distance, particularly in relation to staff allocations. The southern and border regions emerge with distinctive patternings here. To a marked degree, House members from these two areas eschew large staff operations in their districts. Scanning our two regional tabulations (Tables 3 and 9), we note that every region save the Midwest reveals a noteworthy pattern of resource allocation. In the East we find high frequency of home visits and large district staffs; in the Far West, we find a low frequency of home visits; in the southern and border regions, we find small district staffs. Again, the two types of allocative decisions appear to be quite distinct and independent. Region, we tentatively conclude, has a substantial affect on home style.

But regions are composites of several states; and while regional regularities often reflect state regularities, they can also hide them. Both situations have occurred in this instance. Figure 1 displays state-by-state allocation patterns – of personal and district staff attentiveness. For each state delegation, we have computed the

Table 9. District Staff Expenditures and Region

Region	District Staff Expenditures: Lowest 1/3	Middle 1/3	Highest 1/3	Total
East	16 (16%)	31 (31%)	52 (53%)	99 (100%)
South	47 (46%)	36 (35%)	19 (19%)	102 (100%)
Border	18 (55%)	8 (24%)	7 (21%)	33 (100%)
Midwest	39 (37%)	34 (32%)	33 (31%)	106 (100%)
Far West	16 (22%)	29 (40%)	28 (38%)	73 (100%)
	136	138	139	413

Regions:
East: Conn., Maine, Mass., N.H., N.J., N.Y., Pa., R.I., Vt.
South: Ala., Ark., Fla., Ga., La., Miss., N.C., S.C., Tenn., Texas, Va.
Border: Del., Ky., Md., Mo., Okla., W.Va.
Midwest: Ill., Ind., Iowa, Kans., Mich., Minn., Neb., N.D., Ohio, S.D., Wisc.
Far West: Alaska, Ariz., Calif., Colo., Hawaii, Idaho, Mont., Nev., N.M., Oregon, Utah, Wash., Wyo.

mean number of trips home made by its members in 1973; and we have divided the state delegations into those whose averages fell above and below the median number of trips for all House members, i.e., 30 trips, Also, for each state delegation, we computed an average of the percentage of staff expenditures allocated to the district staff by its members; and we have divided the state delegations into those whose averages fell above and below the median percentage for all House members, i.e., 29 per cent. The result is a crude fourfold classification of states according to their combined personal and staff resource allocations to "home." The underlined states fall strongly into their particular patterns; the others display weaker tendencies. Each state is identified, also, by its regional classification.

There are, as Figure 1 shows, distinctive state allocative patterns. Some were foreshadowed in the regional patterns discussed earlier. For example, the eastern states cluster in the high-trips home category; and the far western states cluster in the low-trips home category. Southern and border states cluster in the weak-district staff category. Other state patterns, however, appear here for the first time. The large number of states clustering in the low-trips-home/small-district-staff category, for example were totally obscured in our regional data.

Explanations for these various state clusters are more difficult; and only a few guesses can be made here. The sharp separation between eastern and far western states in trips home is doubtless a function of distance. And – it now appears more strongly – some far western state representatives do compensate for the infrequency of their home visits by maintaining a relatively large staff presence in the district. Yet if California members invest heavily in this compensatory allocative strategy,[22] why don't the members from Washington and Oregon do likewise?

The decision of most southern and border state representatives to spend relatively little on their district staff operations may be explainable by a tradition of nonbureaucratized, highly personalized politics in those areas. Northeasterners may be accustomed to coping with bureaucrats – legislative or otherwise – whereas southern and border state residents would expect to deal directly with the elective officeholder. Yet, in terms of the amount of personal attentiveness to their districts, southern delegations vary widely. At first glance, that variation seems to be related to distance – with states in the near South receiving more attention than states in the far South. But the marked difference among, say, Kentucky, Tennessee, and Alabama would seem to require a more complex explanation. And, speaking of complexity, the similar disposition of resources by the unexpected mix of delegations in the low-trips-home/small-district-staff category (in the lower right hand corner of Figure 1) defies even an explanatory guess at this stage of our study.

The allocative elements of home style vary across regions and among states within regions. The relevance of state delegations to patterns of resource allocation at home will come as no surprise to students of Congress. For there is virtually no aspect, formal or informal, of the legislative process on Capitol Hill that has not already revealed the importance of the state

---

[22] An excellent study of district staff operations in California, containing many stimulating comments on the general subject is: John D. Macartney, "Political Staffing: A View From the District" (Ph.D. dissertation, University of California, Los Angeles, Department of Political Science, 1975).

	Personal Attentiveness	
District Staff Attentiveness	**Above the Median in Trips Home**	**Below the Median in Trips Home**
Above the Median in District Staff Expenditures	*Connecticut (E)* *Massachusetts (E)* *New York (E)* *Tennessee (S)* Illinois (MW) Maine (E) New Hampshire (E) Pennsylvania (E) Rhode Island (E) South Carolina (S) Vermont (E)	*California (FW)* Colorado (FW) Hawaii (FW) Idaho (FW) Iowa (MW) Kansas (MW) New Mexico (FW) Wyoming (FW)
Below the Median in District Staff Expenditures	*Kentucky (B)* *Maryland (B)* *North Carolina (S)* *Virginia (S)* *West Virginia (B)* Delaware (B) Indiana (MW) Montana (FW) Ohio (MW)	*Alabama (S)* *Arizona (FW)* *Florida (S)* *Louisiana (S)* *Minnesota (MW)* *Oklahoma (B)* *Oregon (FW)* *Washington (FW)* *Wisconsin (MW)* Arkansas (S) Michigan (MW) Nebraska (MW) South Dakota (MW) Texas (S) Utah (FW)

N.B. States which fall five trips or more above or below the median *and* whose district staff expenditures fall 5 per cent or more above or below the median are *italicized.* States not listed fall on the median in one or both instances. Regional classifications are in parentheses.

Figure 1. Allocation Patterns: By State

delegation.[23] How much that importance is the product of extensive communication among delegation members and how much the product of similar expectations emanating from similar districts, has not been definitively answered. Nor can it be here. All we can say is that both are probably involved. State delegation members probably talk to one another about their allocative practices and follow one another's example and advice. Also, certain expectations and traditions probably develop within states, or sections of states, so that members feel constrained to make resource allocations that are not too far out of line with those expectations.

The ambiguity of this discussion raises one of the broadest questions concerning home style. Is a congressman's home style something he chooses and then imposes upon his district or is it something that is imposed upon him by the kind of district he represents? We shall worry that question and work our way toward an answer as we proceed. At this point it appears that either or both patterns can hold: for, while state regularities testify to district influences on home style, some states display no regularities; and all states display enough idiosyncratic behavior to testify to the presence of individual choice.

Home style is, then, partly a matter of place – i.e., it is affected by the nature of the congressman's geographical constituency. That constituency is, after all, the closest thing to a "given" in his nest of perceptions. But home style is also partly a matter of individual choice. And in this respect, it can be affected by his perception of his other three constituencies. That, indeed, is what we expect to find as we move to discuss the other elements of home style.

---

[23] For example, Aage Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's, 1973); Barbara Deckard, "State Party Delegations in the U.S. House of Representatives: A Study in Group Cohesion," *Journal of Politics*, 34 (February, 1972), 199–222; John Ferejohn, *Pork Barrel Politics* (Stanford: Stanford University Press, 1974); John Kessel, "The Washington Congressional Delegation," *Midwest Journal of Political Science*, 8 (February, 1964), 1–21; Matthews and Stimson, *Yeas and Nays.*

Home Style:
Presentation of Self

Most House members spend a substantial proportion of their working time "at home." Even those we placed in the "low frequency" category return to their districts more often than we would have guessed – over half of them go home more than once (but less than twice) a month.[24] What, then, do they do there? Much of what they do is captured by Erving Goffman's idea of *the presentation of self.*[25] That is, they place themselves in "the immediate physical presence" of others and then "make a presentation of themselves to others." A description of all the settings in which I have watched members of Congress making such presentations or "performances" as Goffman calls them, would triple the size of this article. But, surely, I have logged – during my thirty visits and ninety-three days in seventeen districts – nearly every circumstance concocted by the mind of man for bringing one person into the "immediate physical presence" of another.

In all such encounters, says Goffman, the performer will seek to control the response of others to him by expressing himself in ways that leave the correct impressions of himself with others. His expressions will be of two sorts – "the expression that he gives and the expression that he gives off." The first is mostly verbal; the second is mostly nonverbal. Goffman is particularly interested in the second kind of expression – "the more theatrical and contextual kind" – because he believes that the performer is more likely to be judged by others according to the nonverbal than the verbal elements of his presentation of self. Those who must do the judging, Goffman says, will think that the verbal expressions are more controlable and manipulable by the performer; and they will, therefore, read his nonverbal "signs" as a check, on the reliability of his verbal "signs." Basic to this reasoning is the idea that, of necessity, every presentation has a largely "promissory character" to it. Those who listen to and watch the presentation cannot be sure what the relationship between them and the performer really is. So the relationship must be sustained, on the part of those watching, by inference. They "must accept the individual on faith." In this process of acceptance, they will rely heavily on the inferences they draw from his nonverbal expressions – the expressions "given off."

Goffman does not talk about politicians; but politicians know what Goffman is talking about. Goffman's dramaturgical analogues are appropriate to politics because politicians, like actors, perform before audiences and are legitimized by their audiences. The response politicians seek from others is *political support.* And the impressions they try to foster are those that will engender political support. House member politicians believe that a great deal of their support is won by the kind of individual self they present to others, i.e., to their constituents. More than most people, they believe that they can manipulate their "presentation of self." And more than most other people, they consciously try to manipulate it. Certainly, they believe that what they say, their verbal expression, is an integral part of their "self." But, like Goffman, they place special emphasis on the nonverbal, "contextual" aspects of their presentation. At least, the nonverbal elements must be consistent with the verbal ones. At most, the expressions "given off" will become the basis on which they are judged. Like Goffman, members of Congress are willing to emphasize the latter because, with him, they believe that their constituents will more readily discount what they say than how they say it or how they act in the context in which they say it. In the member's own language, constituents want to judge you "as a person." The comment I have heard most often from the constituents of my representatives is: "He's a good man," or "She's a good woman," unembossed by qualifiers of any sort. Constituents, say House members, want to "size you up" or "get the feel of you" "as a person," or "as a human being." And the largest part of what members mean when they say "as a person" is what Goffman means by "expressions given off."

So members of Congress go home to present themselves "as a person" – and to win the accolade, "He's a good man," "She's a good woman." With Goffman, they know there is a "promissory character" to the presentation. And their object is to present themselves "as a person" in such a way that the inferences drawn by those watching will be supportive ones. The representative's word for these supportive inferences is *trust.* It is a word they use a great deal. If a constituent trusts a House member, the constituent says something like: "I am willing to put myself in your hands temporarily; I know you will have opportunities to hurt me – though I may not know when those opportunities occur; I assume that you will not hurt me and I'm not going to worry about your doing so until it is proven beyond any doubt that you have betrayed that trust." The ultimate response members of Congress seek is political support; but the instrumental response they seek is trust. The presentation of self – what is "given" in words and "given off" as a person – will be calculated to win trust. "If people like you and trust you as an individual," members often say, "they will vote for you." So trust becomes central to the congressman-constituent relationship. Constituents, for their part – as Goffman would emphasize – must rely on trust. They must "accept on faith" that the congressman is what he says he is and will do what he says he will do. House members, for their part, are quite happy to emphasize trust. It helps to allay the uncertainties they feel about their support relationship with their various constituencies. If they are uncertain about how to work for support directly, they can always work indirectly to win a degree of personal trust that will increase the likelihood of support, or decrease the likelihood of opposition.

---

[24] Fifty-six per cent, or 72 or 129.

[25] Erving Goffman, *The Presentation of Self in Everyday Life* (New York: Doubleday, 1959). The language I have quoted appears in the Introduction and Conclusion.

Trust is, however, a fragile relationship. It is not an overnight or a one-time thing. It is hard to win; and it must be constantly renewed and rewon. So it takes an enormous amount of time to build and maintain constituent trust. That is what House members believe. That is why they spend so much of their working time at home. Much of what I have observed in my travels can be explained as a continuous and continuing effort to win (for new members) and to maintain (for old members) the trust of their various constituencies. Most of the communication I have heard and seen is not overtly political at all. It is, rather, part of a ceaseless effort to reinforce the underpinnings of trust in the congressman or congresswoman "as a person." Viewed from this perspective, the archetypical constituent question is not "what have you done for me lately" but "how have you looked to me lately." House members, then, make a strategic calculation that helps us understand why they go home so much. Presentation of self enhances trust; enhancing trust takes time; therefore, presentation of self takes time.

Of the "contextual," "expressions given off" in the effort to win and hold constituent trust, three seem particularly ubiquitous. First, the congressman conveys to his constituents a sense of his *qualification*. Contextually and verbally, he gives them the impression that "I am qualified to hold the office of United States Representative." "I understand the job and I have the experience necessary to do a good job." "I can hold my own – or better – in any competition inside the House." All members try to convey their qualifications. But it is particularly crucial that any nonincumbent convey this sense of being "qualified." For him, it is the threshold impression – without which he will not be taken seriously as a candidate for Congress. Qualification will not ensure trust, but it is at least a precondition.

Second, the congressman conveys a sense of *identification* with his constituents. Contextually and verbally he gives them the impression that "I am one of you." "I think the way you do and I care about the same things you do." "You can trust me because we are like one another." The third is a sense of *empathy* conveyed by the congressman to his constituents. Contextually and verbally, he gives them the impression that "I understand your situation and I care about it." "I can put myself in your shoes." "You can trust me because – although I am not one of you – I understand you." Qualification, identification, and empathy are all helpful in the building of constituent trust. To a large degree, these three impressions are conveyed by the very fact of regular personal contact at home. That is, "I prove to you that I am qualified," or "I prove to you that I am one of you," or "I prove to you that I understand you" by coming around frequently "to let you see me, to see you, and to meet with you." Contrariwise, "if I failed to come home to see and be seen, to talk and be talked to, then you would have some reason to worry about trusting me." Thus do decisions about the allocation of resources affect the frequency of and opportunity for the presentation of self.

Once he is home, what kind of a presentation does he make there? How does he decide what presentation to make? How does he allocate his time among his perceived constituencies? How does he present himself to these various constituencies? What proportion of competence, identification, or empathy (or other expressions) does he "give off"? In short, what kinds of home styles are presented; and how do they differ among House members? I shall work toward an answer to these questions by discussing the styles of two representatives.

Presentation of Self: A Person-to-Person Style. While it is probably true that the range of appropriate home styles in any given district is large, it is also probably true that in many geographical constituencies there are distinct limits to that range. Congressman A believes there is a good "fit" between his kind of district and his kind of home style. He thinks of his geographical constituency as a collection of counties in a particular section of his state – as southern, rural, and conservative. And he believes that certain presentations of self would not be acceptable there. "I remember once," he told a small group at dinner before a college lecture,

> when I was sitting in the House gallery with a constituent listening to Congressman Dan Flood speak on the floor. Dan is a liberal from Wilkes Barre, Pennsylvania. He is a former Shakespearian actor and his wife is a former opera singer. Dan was wearing a purple shirt and a white suit; and he was sporting his little waxed moustache. My constituent turned to me and asked 'what chance do you think a man like that would have of getting elected in our district?' And I said, 'exactly the same chance as I would have of getting elected in Wilkes Barre, Pennsylvania.'

The expressions "given off" by a former actor with a purple shirt, a white suit, and a waxed moustache would be suicidal in Congressman A's district. Indeed, two days earlier as we got out of the car in one of his county seats, Congressman A said apprehensively, "see my brown shirt? This will be the first time that these people have ever seen me in anything but a white shirt." Brown – possibly; purple – never.

Congressman A sees his geographical constituency as a homogeneous, natural community. And he thinks of himself as totally at one with that community – a microcosm of it. Three generations of his family have lived there and served as its leaders and officeholders. He himself held two elective offices within the district before running for Congress. He has been steeped in the area he represents.

> I should write a book about this district – starting with the Indians. It's a very historic district and a very cohesive district – except for Omega County. Nobody knows it like I do.
>
> One thing that ties the district together is the dominance of the textile industry and the dependence of the people of the district – employer and employee – on the textile industry.... If I were hostile to the textile industry, it would be fatal. But that could never happen because I feel so close to the textile industry.
>
> I represent a district in which my constituents and I have total mutual confidence, respect and trust – 95 per cent, nearly 100 per cent.

Congressman A feels a deep sense of identification with his constituents. It is this sense of identification that he conveys – verbally and nonverbally – when he presents himself to them.

"In my state," he says, "only a person-to-person campaign will work." So, when he goes home, he "beats the bushes," and "ploughs the ground," in search of face-to-face contact with the people of his district. From county to county, town to town, up and down main street, in and out of county courthouses, through places of business, into homes and back yards, over country roads and into country stores, from early morning till late at night: ("Anyone who hears a knock on the door after 11:00 p.m. knows it's me") he "mixes and mingles" conveying the impression that he is one of them. In each encounter, he reaches (if the other person does not provide it) for some link between himself and the person he is talking with – and between that person and some other person. There is no conversation that does not involve an elaboration of an interpersonal web and of the ties that bind its members one to the other. In the forefront, always, are ties of family: Congressman A possesses an encyclopedic memory for the names and faces, dates and places of family relations, and for the life-cycle events of family – birth, marriage, moving, sickness, and death. His memory and his interest serve him equally well in finding other common ground – be it rivers, plants and trees, farms, crops and businesses, hunting, fishing and football, land, buildings and automobiles, home, church, and country. He devours the district's history; on one trip he was absorbed in a county history and genealogy, on another the memoirs of U. S. Grant. He continually files, sorts, arranges and rearranges his catalogues of linkages – person-to-person, place to place, event to event, time to time.

The Congressman muses a lot about the keys to success in person to person relationships.

> Do you remember Miss Sharp back in the post office? She had never met me before, but she called me Sam. That's the way people think of me. No person will ever vote against you if he's on a first name basis with you. Did you know that?
>
> When I'm campaigning I sometimes stop in a country store and buy some salmon and crackers and share them with everyone there – and buy more if need be. Do you know that a man who eats salmon and crackers with you will vote for you? And if a man takes a bite of your chewing tobacco – or better still if he gives you a bite of his chewing tobacco – he'll not only vote for you, he'll fight for you.
>
> People feel they can talk to me. When they are talking, they feel that I'm listening to what

> they have to say. Some people have the ability to make others feel that way and some don't. They feel that if they come to me with a problem, I'll do everything I can to help them.

The expression he tries to give off in all his person to person dealings is that he knows them, that they know him "as a person," that they are all part of the same community, and that his constituents, therefore, have every reason to make favorable inferences about him. "They know me," he says, "and they trust me."

Since he perceives his geographical constituency as a group of counties, it is natural to find him conveying this sense of identification in terms of counties. In three different counties – none of them his place of residence – he verbalized his relationship with the people who lived there as follows: Chatting with a group of businessmen riding to lunch after a meeting with the officials of Alpha County on water and sewer problems, he said,

> Did you know that an Alpha County man saved my grandfather's life in the Civil War? In the battle of Williamsville, my grandfather was badly wounded and Lieutenant Henry from Henryville picked him up and carried him off the field – just a bloody uniform with pieces of bone sticking out. An orderly stopped him and said, 'What are you doing carrying that corpse?' Lieutenant Henry said, 'That's no corpse; that's Captain McDonough; and so long as there's a spark of life in him, I'm going to try to save him.' He did and my grandfather lived. My roots go deep in Alpha County.

Giving an after dinner speech to the Women's Business and Professional Club of Beta County, he said,

> I feel as much at home in Beta County as I do anyplace on earth. I can't begin to describe to you the frustrations I feel when I see these crazy social experiments (in Washington).... These frustrations would make me a nervous wreck or worse if I could not come back home to be with you, my friends and neighbors, my supporters and my constituents. I come home to refresh my spirit and renew my strength, here in the heart of our district, where my family's roots go deep. To me, this truly is 'holy ground.'

Speaking to a sesquicentennial celebration in adjacent Gamma County, he began,

> I have never recognized the artificial boundaries that separate our two counties. I have felt as much at home in Gamma County – our county – among my friends and neighbors as if I had been born here, raised here, and lived here every day of my life.

Later that evening, he reflected on his appearance at the sesquicentennial, which he ranked as the most important event of my four-day visit. "When Marvin introduced me today and said that there weren't five people out there, out of 4000, who didn't know me, he was probably right. And those who don't know me think they do."

His repeated use of the term "at home" suggests that Congressman A perceives the people whom he meets as his primary constituency. When asked to describe "his very strongest supporters," he explained: "My strongest supporters are the people who know me and whom I have known and with whom I have communicated over the years . . . in my oldest counties, that means 30–40 years." He does not perceive his primary constituency in demographic terms but in terms of personal contacts. In a district seen as homogeneous there are few benchmarks for differentiation. And the one clear benchmark – race – is one he never mentions in public and only rarely in private. His primary constituents are the rural and small town whites who know him (or feel that they know him) personally. He seems to be, as he was once introduced, "equally at home with blue denim and blue serge, with rich folks and po' folks" – so long as the blue denim is nonunion. Standing around in a dusty, brown field swapping hunting jokes with a group of blue-collar friends and sitting in an antique-filled livingroom talking business with the president of a textile company rank equally high on his "at homeness" index. His primary constituency, as may be the case in homogeneous districts, is quite amorphous – as demographically amorphous as V. O. Key's classic "friends and neighbors" victory pattern. But it is sizeable enough and intense enough to have protected Congressman A, for a considerable number of terms, from any serious primary challenge.

Congressman A does not come home a lot, falling into our low-frequency category (0–23) of trips home. He spent eighty working days there in 1973. When he does, he spends most of his time where it is strategically profitable (and personally comfortable) – with his primary constituency. There, he reinforces his ties to the group of greatest importance to him in his traditionally one-party district. He explained, for example, why he took time out of a crowded Washington work week to fly home to the installation of officers of a Boy Scout Council.

> I wanted to make it because of who they were. They were Boy Scout leaders from six of my

> counties – the men who make scouting here a viable movement. They have given me some of my strongest support since I have been in politics. And they have never asked anything of me but to give them good government. So when they ask, I sure don't want to pass up the opportunity to meet with 90–100 of them. I knew about 90 per cent of them. And the other 10 per cent I know now. Some of those I hadn't met were sons of men I had known.

The scout leaders, of course, want more from him than "good government." They want his time and his personal attention. And he, believing these to be the essence of his home style, happily obliges.

When he mentioned that he had also left Washington once to speak at a high school graduation, I asked whether a high paid staff assistant in the district might relieve him of some of these obligations. (Some members in my group have just such an assistant who attends meetings "in the name of the congressman" when he cannot come.) Congressman A answered,

> It wouldn't work. People want to see the congressman – me. At the high school commencement exercises, I could have sent the most scholarly person I could find, to make a more erudite, comprehensive and scholarly exposition than I made. If I had done so, the people there wouldn't have enjoyed one bit of anything he said. And they would never have forgiven me for not being there.

He has a small district staff – three people, one full-time office, and one-half time office – and when he is home, he is as apt to pick up someone's personal problems and jot them down on the back of an envelope as he tours around as he is to find out about these problems from his district aides. Congressman A, at home, is a virtual one-man band. His home style is one of the hardest to delegate to others; and he has no inclination to do so.

He allocates relatively little of his time to his larger re-election constituency. Omega County, singled out earlier as out of the district's mold, is not rural, is populated heavily by out-of-staters, and has experienced rapid population growth. Congressman A admits he does not feel "at home" there. Yet he still gets a sizeable percentage of Omega County's votes – on grounds of party identification. He explained why he didn't spend time among these re-election constituents.

> It is so heterogeneous, disorganized, and full of factions. . . . I don't spend very much time there. Some of my good friends criticize me and say I neglect it unduly. And they have a point. But I can get 50 per cent of the vote without campaigning there at all; and I couldn't get more than 75 per cent if I campaigned there all the time. If I did that, I would probably lose more votes than I gained, because I would become identified with one of the factions, and half the people would hate me. On top of that, I would lose a lot of my support elsewhere in the district by neglecting it. It's just not worth it.

There is another reason besides time costs and political benefits. It is that Congressman A's home style is totally inappropriate for Omega County, and he avoids the personal unpleasantness that would be involved in trying to campaign there. Strategically, Congressman A will accept any increment of support he can get beyond his primary constituency. ("The black people who know me know that I will help them with their problems.") But he allocates very little of his time to the effort.

Congressman A's presentation of self places very little emphasis on articulating issues. The Congressman's own abilities and inclinations run to cultivating personal, face-to-face relationships with individuals. The greater the social, psychological, and physical distance between himself and others, the less he is at home, regardless of the situation. And he was clearly least "at home" at a college, in a lecture-plus-question-and-answer format. He accepts invitations of this sort to discuss issues. But he does nothing to generate such engagements; nor does he go out of his way to raise issues in his dealings with others at home. On the single occasion when he broke this pattern, he tested out his potentially controversial position with his primary constituents (i.e., the American Legion post in his home town), found it to be acceptable, and articulated it often thereafter. Congressman A's home style does, however, take place *within an issue context*. There is widespread agreement in the district, and very strong agreement within his primary constituency, on the major issues of race, foreign aid, government spending and social conservatism. The district voted for George Wallace in 1968. Thus while Congressman A's home style is apparently issueless, it may depend for its very success on an underlying issue consensus.

There are, therefore, strategic reasons as well as personal reasons for Congressman A not to focus heavily on specific issues. To do so would be unnecessary and potentially divisive. Congressman A is protective of his existing constituency relations and will not want to risk alienating any of his support by introducing or escalating controversy of any kind. He is a stabilizer, a maintainer. And so, when asked to speak formally, he often responds with communitarian homilies. "I believe if ever there was a promised land, that land is America; and if ever there was a chosen people, those people are Americans." "If a man isn't proud of his heritage, he won't leave a heritage to be proud of. And that goes for his family, his community and his country." These utterances are not the secret of his success. But they do testify, again, to his continuing efforts to articulate a sense of community, to construct and reconstruct a web of enduring personal relationships and to present himself as totally a part of that web. If he gets into electoral difficulty, Congressman A will resort not to a discussion of "the issues," but to an increased reliance on his person-to-person home style. And, so long as his strategic perceptions are accurate, he will remain a congressman.

Presentation of Self: An Issue-Oriented Style. If Congressman B's geographical constituency places any constraints on an appropriate home style, he is not very aware of them. He sees his district as heterogeneous.

> It is three worlds; three very different worlds. . . . It has a city – which is an urban disaster. It has suburbs – the fastest growing part of the district. . . . It has a rural area which is a place unto itself.
>
> We spent all afternoon talking to the Teamsters in the city; and then we went to a cocktail party in a wealthy suburb. That's the kind of culture shock I get all the time in this district – bam! bam! bam!

The "three worlds" are not just different. They are also socially and psychologically separated from one another.

> Actually the people in the three worlds don't know the others are even in the district. They are three separate worlds. In the city, they call it the city district; in the rural area, they call it their district. And both of them are shocked when they are told that they each make up only one-quarter of the district.

The other half are the suburbs – which are, themselves very disparate. A few suburbs are linked to the city; most are not. Some are blue collar; others are affluent. Some are WASP; others are ethnic. The district is, then, perceived not only as diverse and artificial, but as segmented as well. The possibilities for an acceptable presentation of self would seem to be limitless.

Congressman B's past associations in the district do not incline him toward a style peculiar to any one of "the three worlds." His district ties are not deep; he is a young man who went to college, worked and got his political feet wet outside his district and his state. Nor are the ties strong; he grew up in a suburb in which he probably feels less "at home" ("We lost that stupid, friggin' town by 1000 votes last time.") than anywhere in the district. When he first thought about running, he knew nothing about the district. "I can remember sitting in the livingroom here, in 1963, looking at the map of the district, and saying to myself, 'X? Y? I didn't know there was a town called X in this district. Is there a town called Y?' I didn't know anything about the district." Furthermore, he didn't know any people there. "We started completely from scratch. I was about as little known in the district as anyone could be. In the city, I knew exactly two people. In the largest suburb, I didn't know a single person." He has (unlike Congressman A) absolutely no sense that "only a person like myself" can win in his district. Indeed, he thinks the opponent he first defeated was better suited to the district and should have won the election. "If I were he, I'd have beaten me." In terms of a geographical constituency and an individual's immersion in it, it is hard to imagine two more different perceptions of me-in-the-constituency than those of Congressman A and Congressman B.

Congressman B has not been in office very long. Not only did he begin from scratch, but he has been scratching ever since. He lost his first race for Congress; he succeeded in his second; and he now represents an objectively (and subjectively) marginal district. His entire career has been spent reaching out for political support. As he has gone about identifying and building first a primary and then a re-election constituency, he has simultaneously been evolving a political "self" and methods of presenting that "self" to them.

His earliest campaign promises were promises about the allocation of resources. He pledged to return to the district every week and to open three district offices, one in each of the "three worlds." These commitments about home style were contextually appropriate, if not contextually determined. For a candidate who neither knew nor was known in the district, pledges of attentiveness would seem almost mandatory. Furthermore, they allowed him to differentiate his proposed style from that of the incumbent – who was not very visible in the district and who operated one office there staffed by two people. Also, these pledges allowed him to embroider his belief that "a sense of distance has developed between the people and the government," necessitating efforts to "humanize" the relationship. And finally, his pledges gave him a lowest common denominator appeal based on style to a district with palpably diverse substantive interests. In 1973, Congressman B made thirty trips home, spent 109 working days there, operated three district offices and assigned one-half of his total staff of fourteen to the district. Promises have turned into style. "We have given the impression of being hardworking – of having a magic carpet, of being all over the place. It's been backbreaking, but it's the impression of being accessible."

Congressman B's actual presentation of self, i.e., what he does when he goes home, has evolved out of his personal interests and talents. He was propelled into active politics by his opposition to the Vietnam War. And his political impulses have been strongly issue-based ever since. He is severely critical of most of what has gone on in American public life for the last ten years. And he espouses a series of programmatic remedies – mostly governmental – for our social ills. He is contemptuous of "old line" politicians who are uninterested in issues and who campaign "by putting on their straw hats and going to barbeques." Riding to a meeting at which he was to address one of his aging town committees, he shouted, "We don't want any old pols or town committees. Give me housewives who have never been in politics before." Whereupon, he rehearsed the opening lines of "the speech I'd like to give" to the town committee. "It will be a stirring speech. 'My fellow political hacks. We are gathered together to find every possible way to avoid talking about the issues.' " This comment, together with his running mimicry of the "old pols," exemplifies what Goffman calls a performance in the "back region," i.e., behind the scenes where the individual's behavior is sharply differentiated from, and serves to accent, his presentation of self to the audience in the "front region."[26]

Congressman B presents himself in the "front region," i.e., in public, as a practitioner of an open, issue-based, and participatory politics. It was his antiwar stand particularly, and his issue-orientation generally that attracted the largest element of his primary constituency. These were the antiwar activists – young housewives, graduate students, and professionals – who created, staffed, and manned the large volunteer organization that became his political backbone. In the end, his volunteers became skilled in the campaign arts – organizing, coordinating, polling, canvassing, targeting, mailing, fund raising, scheduling, advancing, leafleting – even "bumper-stickering." "We organized and ran a campaign the likes of which people in this district had never seen. Neither party had done anything like it." Lacking a natural community to tie into and lacking any widespread personal appeal (or basis for such), Congressman B turned to the only alternative basis for building support – an organization. The "strongest supporters" in his organization did not support him because they knew him or had had any previous connection with him. The bond was agreement on the central issues and on the importance of emphasizing the issues. That agreement was the only "qualification" for the office that mattered to them. Within this group, the sense of identification between candidate and supporters was nearly total. He was "one of them." They trusted him. And they, with some trade union help (especially financial), gave him a victory in his initial primary.

In reaching for broader electoral support, Congressman B has been guided, in addition to his commitment to "the issues," by a personal penchant for talking about them. That is, he is an exceptionally verbal person; and he has evolved a suitably verbal home style. He places special emphasis on articulating, explaining, discussing, and debating issues. In each campaign (whether he be challenger or incumbent) he has pressed for debates with his opponent; and his assessment of his opponents focuses on their issue positions and their verbal facility. ("He's very conservative and, I understand, more articulate than the last guy. I felt sorry for him; he was so slow.")

In his first two campaigns the main vehicle for presenting himself to his prospective election-re-election constituency was "the coffee." He would sit in a living room or a yard, morning, afternoon, and evening (sometimes as often as eight or ten times each day) with one or two dozen people, stating his issue positions, answering their questions, and engaging in give and take. At the verbal level, the subject was substantive problems. But Congressman B knew that expressions "given off" were equally important.

> People don't make up their minds on the basis of reading all our position papers. We have twenty-six of them, because some people are interested. But most people get a gut feeling about the kind of human being they want to represent them.

Thus, his display of substantive knowledge and his mental agility at "the coffees" would help convey the impression that "as a human being" he was qualified for the office. And, not relying wholly on these expressions given off, he would remind his listeners, "No congressman can represent his people unless he's quick on his feet, because you have to deal with 434 other people – each of whom got there by being quick on his feet." Coffees were by no means the only way Congressman B presented himself. But it was his preferred method. "The coffees are a spectacular success. They are at the heart of the campaign." Strategically, they were particularly successful in the suburban swing area of the district. But he tried them everywhere – even in the city, where they were probably least appropriate.

---

[26] Goffman, Chapter 3, especially p. 128. Often comments made in the "back regions" provide clues for "at homeness" estimates. "We're having breakfast tomorrow with a businessman's group. They're really a bunch of hoodlums."

Once in office, he evolved a natural extension of the campaign coffee – a new vehicle which allowed him to emphasize, still, his accessibility, his openness and his commitment to rational dialogue. It is "the open meeting," held twice a year, in every city and town in the district – nearly 200 in each session of Congress. Each postal patron gets an invitation to "come and 'have at' your congressman." And, before groups of 4 to 300, in town halls, schools, and community centers, he articulates the issues in a question and answer format. The exchanges are informative and wide-ranging; they are punctuated with enthusiasm and wit. The open meetings, like the coffees, allow Congressman B to play to his personal strengths – his issue interests and his verbal agility. In the coffees, he was concerned with conveying threshold impressions of qualification, and his knowledge and status reinforce that impression in the open meetings. But in the open meetings, he is reaching for some deeper underpinnings of constituent trust. He does this with a presentation of self that combines identification and empathy. "I am not exactly one of you," he seems to tell them, "but we have a lot in common, and I feel a lot like you do." He expresses this feeling in two ways.

One expression "given" and "given off" in the open meetings is the sense that the give-and-take format requires a special kind of congressman and a special kind of constituency and involves them, therefore, in a special kind of relationship. In each meeting I attended, his opening remarks included two such expressions.

> One of the first pieces of advice I got from a senior member of my party was: 'Send out lots of newsletters, but don't mention any issues. The next thing you know, they'll want to know how you vote.' Well, I don't believe that.
>
> My colleagues in Congress told me that the questionnaires I sent you were too long and too complicated and that you would never answer it. Well, 5000 have been filled out and returned already – before we've even sent them all out.

At the same time that he exhibits his own ability to tackle any question, explain any vote, and debate any difference of opinion, he massages the egos of his constituents by indicating how intelligent, aware, and concerned they are to engage with him in this new, open, rational style of politics. At the conclusion of an emotional debate with a group of right-to-lifers, whose views he steadfastly opposed, he summed up: "I don't want to pat myself on the back, but there aren't too many congressmen who would do what I am doing here today. Most of them dig a hole and crawl in. I respect your opinions and I hope you will respect mine." The "pat on the back" is for *them* as well as him. And the expression "given off" is that of a special stylistic relationship. From that relationship, he hopes, will flow an increasing measure of constituent trust.

A second, related, expression "given off" is the sense that Congressman B, though he is a politician, is more like his constituents than he is like other politicians. It is not easy for him to convey such an impression, because the only thing his potential re-election constituents know about him is that he is a politician. They do not know him from any prior involvement in a community life. So he works very hard to bind himself to his constituents by disassociating himself from "the government" and disavowing his politician's status. He presents himself as an antipolitician, giving off the feeling that, "I'm just as fed up with government and the people who run it as you are." Since he is a congressman-politician, he is unrelentingly harsh in his criticism of Congress and his fellow legislators.

> As you know, I'm one of the greatest critics of Congress. It's an outrageous and outmoded institution.
>
> All Congress has ever done since I've been in Congress is pass the buck to the president and then blame him for what goes wrong.... Congress is gutless beyond my power to describe to you.
>
> Most members of Congress think that most people are clods.... Most of the guys down there are out of touch with their districts.... We aren't living in the 1930s anymore. Of course some members of Congress are.... I could never understand the lack of congressional sensitivity to the problems of the elderly. There are so many of them there.

A politician seeking to convey the impression that he is not a politician, Congressman B hopes to build constituent trust by inviting them to blend their cynicism with his.

The presentation of self – an accessible, issue-oriented, communicative antipolitician – at the open meetings is a lowest common denominator presentation. It can win support in each of "the three worlds" without losing support in any. For it is the style, not the issue content that counts most in the re-election constituency. Congressman B is completely comfortable in the setting. "That was fun," he says after each open meeting. And, occasionally, "it's more fun when there's some hostility." But it is the format more than the audience that makes him feel really "at home." He is not a person-to-person campaigner. "Two of my friends in Congress hold office hours and see people one at a time. That would be a horribly inefficient use of my time. I can see fifty at once. Besides, they don't want to get involved in a give and take." He, on the other hand, keeps his distance from the personal problems of his constituents, inviting them to talk with the staff members who accompany him to the open meetings. Of course, he meets people face-to-face – all the time. But he does not know or seek out much about them as individuals, not much that would build anything more than a strictly political connection. An aptitude for names and faces, a facility with tidbits of personal information and small talk, an easy informality in face-to-face relations – these are not his natural personal strengths. But they are not the keys to his success with his re-election constituency. He has evolved a home style that does not call for person-to-person abilities in large supply.

The open meetings remain the centerpiece of his home style. "They are the most extraordinary thing we've ever done, and the most important." He sees them as vehicles which help him reach out to and expand his re-election constituency. For he remains a builder instead of a stabilizer in his constituency relations.

> Politically, these open meetings are pure gold. Fifty may come, but everybody in town gets an invitation.... I do know that none of our loyalists come to the meetings. They know the meetings are nonpartisan. Maybe one or two of them will show up, but mostly they are new faces.

They have given him entree into the least supportive, rural areas of his district, where he recruits support and neutralizes the more intense opposition. At first, he says, "in some of these towns they didn't know what to say to a Democrat. They probably hadn't met one except for people who fixed their toilets." Yet at the open meetings, "we've had better turnouts, proportionately, in the rural area." "And we get a lot of letters from people there who say they disagree with us but respect our honesty and independence." In time – but only in time – interest and respect may turn into the supportive inferences that connote trust.

But as Congressman B spends more and more of his time at home cultivating an expanding re-election constituency, his oldest and strongest supporters have felt neglected. So Congressman B has a more complex strategic problem, in terms of allocating his time, than Congressman A.

> When we began, we had the true believers working their hearts out. It was just like a family. But the more you gain in voters, and the more you broaden your constituency, the more the family feels hurt. Our true believers keep asking me, 'Why don't you drink with us?' 'Why don't you talk to me personally anymore?' I have to keep talking to them about the need to build a larger majority. I have to keep telling them that politics is not exclusive; it is inclusive. It is not something that can be done in the living room.

The true believers are not threatening a total loss of support; but declining enthusiasm would present a serious support problem. One way Congressman B may deal with the problem is to come home more, so that he can give the necessary time to the true believers. He does come home more than Congressman A, perhaps partly because his strategic problems at home require it. Still, Congressman B emphasizes identifying and building support beyond the primary constituency in "the three worlds." And he finds the open meetings the most effective (and most comfortable) vehicle for him. "What more could anyone ask," he says, "than to have the congressman come to their town personally?" His primary constituents do ask something more. And, so long as he gives it to them, he will remain a congressman.

Presentation of Self: Constituency Constraints and Constituency Careers. Our description of the person-to-person and the issue-oriented styles is exemplary, not exhaustive.[27] Speculatively, however, presentation of self would seem to be explainable by three kinds of factors – *contextual, personal, and strategic.*

---

[27] The two styles are the most common among my group. They may be the most common among all representatives. In his study of representation, Paul Peterson uses two very similar analytical categories, "particularistic representation" (person-to-person) and "universalistic representation" (issue-oriented) Paul Peterson, "Forms of Representation: Participation of the Poor in the Community Action Program," *American Political Science Review*, 64 (June, 1970), 491–501. Other styles will be elaborated in a later, lengthier study, of which this paper is a part.

Contextually, a representative thinks about his constituency relations in terms of me-in-the-constituency. That perception predates his service in Washington and cannot be understood by drawing inferences from his Washington behavior. Part of the content of that perception involves a sense of fit – a good fit as in the case of Congressman A, a nonfit as in the case of Congressman B, and a bad fit as in the case of one congressman (not in my group) who refers to his district as "outer Mongolia." A congressman's sense of fit will, in turn, be affected by whether he sees the district as homogeneous or heterogeneous. Good fits are more likely in homogeneous districts. But the reverse side of the coin is that home styles are more likely to be imposed upon the congressman in homogeneous districts. If Congressman A did not represent his district, someone who performed similarly at home probably would. In a heterogeneous district – Congressman B's case – home style is much more a matter of individual choice, and is more likely to be imposed by the congressman on his district. Thus, upon further analysis of the state-by-state resource allocation data, we would expect to find the most idiosyncratic patterns appearing in the most heterogeneous districts. Homogeneous districts, in sum, impose more stylistic constraints on a congressman than do heterogeneous districts.

A second contextual impact on the presentation of self, however, may produce contrary tendencies. Once a congressman has imposed a particular presentational style upon his district, his successors may feel constrained to continue that style. That is, a congressman's home style may be influenced by the previous congressman's home style. Congressman B deliberately chose a style that contrasted with his predecessor's in order to help develop an identifiable political self. It is equally plausible (and it happened in my group) that expectations about style could be so strongly implanted in the district by a predecessor, that the new congressman dare not change. Similarly, a choice of home style by imitation or by contrast can occur with reference to a neighboring congressman – if the congressman choosing a style has reason to believe that some of his constituents are likely to compare him to that neighbor. Regardless of district make-up, then, under certain conditions one congressman's style may be shaped by the style of another congressman, past or present.

From the cases of representatives A and B, it seems clear that the presentation of self is also shaped by each individual's inclinations and talents. Every congressman has some latitude in deciding how to present himself "as a person." That is not to say that House members *like* to do all the things they do at home. More members than I would have expected described themselves as "shy," "reserved," or "not an extrovert." But they go home and present themselves anyway. And they try to do what they are most comfortable doing and try not to do that which they are most uncomfortable doing. Congressman A seeks out person-to-person relationships, but does not encourage issue-oriented meetings. Congressman B seeks out issue-oriented meetings, but does not encourage person-to-person relationships. Experience, interest, abilities – all the personal attributes of a congressman's self – help shape his presentation of that self to others.

Strategically, each congressman must decide how he will allocate his time when he is at home. And it is of some help to think of this strategic problem in terms of his perceived constituencies. From our two cases, we might generalize that a man in a homogeneous district will spend most of his time with his primary constituency. Homogeneous districts are most likely to be perceived as protected in the general election, so that the strategic problem is to hold sufficient primary constituency support to ward off a primary challenger. By the same token, the primary constituency in a homogeneous district is probably more amorphous and less easily defined than it is in a heterogeneous district. Thus a concentration of effort in the primary constituency does not mean that any less time will be required to cultivate it.

By contrast, in a district perceived to be both heterogeneous and electorally unprotected, the congressman will spend relatively more time in his re-election constituency. But he faces a problem of balance. He will play "the politics of inclusion" by spending time expanding his re-election constituency, partly on the assumption that his strongest supporters have no inclination to go elsewhere. Yet he cannot neglect the primary constituency unduly, since their loyalty and intensity of commitment are necessary to sustain a predictably difficult election campaign. He may, of course, be able to allocate resources other than his time, i.e., votes, to keep his primary constituency content.[28] But there is every evidence from my experience that the congressman's strongest supporters are more – not less – demanding of the congressman's time than his other constituencies.

---

[28] See Morris Fiorina, "Electoral Margins, Constituency Influence and Policy Moderation: A Critical Assessment," *American Politics Quarterly,* 1 (October, 1973), 479–498.

A strategic problem in allocating time, alluded to briefly in discussing Congressman B, involves the presentation of self to one's strongest opponents – to the people each congressman believes he "will never get." House members handle the problem differently. But most of them will accept (and some will solicit) opportunities to present themselves before unfriendly constituents. The strategic hope is that displaying themselves "as a person" may reduce the intensity of the opposition, thus neutralizing their effect within the district. Intense opposition is what every congressman wants least of. Any time spent cooling the ardor of the opposition is time usefully spent, for it may mean less intense support for the challenger. Functionally, the same accents used in presenting one's self to supporters apply to a presentation to opponents – the emphasis on qualification, the effort at identification, the projection of empathy. In other words, the process of allaying hostility differs little from the process of building trust. That makes it easier for House members to allocate some time – probably minor – to a strategy of neutralization.

Students of Congress are accustomed to thinking about a congressman's *career in the House* – his early adjustments, his rise in seniority, his placement on the ladders of committee and party, the accumulation of his responsibilities, the fluctuations of his personal "Dow Jones Average." But House members also pursue a *career in the constituency*. Congressman B's evolution from "scratch" to a concern with his primary constituency to a concern with his re-election constituency gives evidence of such a constituency career. He was as much a newcomer in the district as newcomers are (or were) purported to be in the House. He had to work out an appropriate home style there just the way each new House member adapts to the House as an institution. Congressman B has been, and is, in the expansionist phase of his career, continually reaching out for increments of support. Congressman A, by comparison, is in a more protectionist phase of his constituency career. He believes that he has, over a considerable period of time, won the trust of his constituents (i.e., his primary constituents). He is working mainly to reinforce that trust; to protect the support he already has. Congressman B does not talk about constituent trust; he never says, "my constituents trust me." His presentation of self is designed to build trust, but, as we have said, it takes time.

The idea of a career in the constituency helps to highlight an important fact about the congressman as an elective politician. As any textbook treatment of incumbency tells us, the congressman is a particularly long-lived political species. He has been making or will make presentations of self to his constituents *for a long time*. And they have been looking at or will look at him "as a person" for a long time. Relative to politicians with briefer constituency careers – like presidents – a congressman's political support will depend especially heavily upon his presentation of self. That, of course, is precisely what House members themselves tell us whenever we have asked. They tell us that their "personal record and standing" or their "personalities" are more important in explaining their election than "issues" or "party identification."[29] They tell us, in other words, that their home style – especially their presentation of self – is the most important determinant of their political support. The idea of a lengthy constituency career helps us understand why this might be true. For it makes home style into a durable, consistent long term factor in congressional electoral politics. In any congressional electoral analyses patterned after our presidential electoral analyses, home style may have to be elevated to a scholarly status heretofore reserved only for party identification and issue voting.[30]

---

[29] See Stokes and Miller, "Party Government," p. 542; and Charles S. Bullock, III, "Candidate Perceptions of Causes of Election Outcome," paper delivered at American Political Science Association Convention, New Orleans, 1973, Tables 7, 8. In his analysis of ten congressional campaigns in 1962, David Leuthold found that "probably more than half the appeals (of the candidates) . . . were based on the qualities of the candidate or his opponent." Leuthold, *Electioneering in a Democracy*, p. 113. He believes that voters are looking for ability, concern, and similarity to themselves. That – in the somewhat different words, qualification, empathy, and identification – is what my representatives think the voters want. See Leu-thold, pp. 23–24.

[30] The first people to view congressional incumbency, in the light of SRC analyses, as a long-term force were Robert Arseneau and Raymond Wolfinger, "Voting Behavior in Congressional Elections," paper delivered at American Political Science Association Convention, New Orleans, 1973. Observations consistent with those here, made in different contexts are David Mayhew's emphasis on "the expected incumbent differential" and Charles Jones's conclusion that House campaigns are less likely to be "issue-oriented" than "image-oriented" and "issue-involved." Home style contributes to the congressman's "incumbent differential" and to his "image." Mayhew, *The Electoral Connection;* Jones, "The Role of the Campaign in Congressional Politics." From an election analysis perspective Walter Dean Burnham has recently emphasized the "office specific" nature of elections and the need to develop ways of looking at congressional

Home Style: Explanation of Washington Activity

When members of Congress are at home, they do something that is closely allied with, yet separable from, the presentation of self to their constituencies. They explain what they have done in Washington. For some House members, their Washington activities are central to their presentation of self. One congressman, for example, routinely began every speech before every district group as follows:

> I have represented this district for the last twenty years. And I come to you to ask for a two-year renewal of my contract. I'm running because I have a twenty-year investment in my job and because I think you, as my constituents, have an investment in my seniority. In a body as large as the House of Representatives with 435 elected, coequal members, there has to be a structure if we're ever going to get anything done. And it takes a long time to learn that structure, to learn who has the power and to learn where to grease the skids to get something done. I think I know the structure and the people in the House better than any newcomer could. And I believe I can accomplish things for you that no newcomer could.

He wants his constituents to see him "as a person" in terms of his importance in Washington. By contrast, neither Congressman A nor Congressman B makes his Washington activity central to his presentation of self. But whether or not his behavior in Washington is central to his presentation of self, every House member spends some time at home explaining and justifying his Washington behavior to his various constituencies. He tells them what he has done and why. What he says, how he says it, and to whom can be viewed as a distinctive aspect of his home style.

The objective of every congressman's explanations – our usage of *explanation* incorporates the idea of *justification* as well – is political support. And just as a congressman chooses, subject to constraints, a presentational style, so too does he choose, subject to constraints, an explanatory style. When most people think of explaining what goes on in Washington to constituents, they think of explaining votes. But we should conceptualize the activities subject to explanation more broadly than that. A House member will explain any part of his activity in Washington if he thinks that part of his activity is relevant to the winning and holding of support at home. Just what kinds of behavior he thinks his various constituencies want or need to have explained to them is an empirical matter; but one which bulks especially large among my representatives is their effectiveness (or lack of it) inside the House on behalf of their constituencies. Often this explanation of one's internal influence also entails a more general explanation of the workings of Congress.

In the case of our Congressman A, for example, press reports that he had lost a committee assignment of importance to the district because of his lack of power within the House, posed a major explanatory problem.

> Nothing is more damaging to a congressman in his district than to have his constituents believe that he doesn't have the power to get something he wants of that nature. . . . It might have been the only issue in the next campaign. . . . That would have been all that was needed – and only that – to defeat me. . . . No one in the world would believe my explanation, so I had to try for the next vacancy on the committee and I had to win. [Which he did, before the next election.]

For Congressman A, that is, explaining his internal House influence (or lack of it) might be more crucial to protecting his home support than explaining his votes. "I worked my head off to get that building," he said, as we drove by a new federal building in one of his county seats. "The people here were fixing to run someone against me if I hadn't produced it. People think you just have to wave a magic wand to get an appropriation, when most of it is just standing in line waiting your turn." Obviously, such an explanation of "how Congress really works" would not have satisfied his constituents in that county. So he had to produce.

The range of possible activities requiring a home explanation extends well beyond voting. Still, voting is the Washington activity we most easily recognize; and we can make most of our comments in that context. From John Kingdon's splendid discussion of "explaining,"[31] we know that, at the time they decide to vote, House members are very aware that they may be called upon to explain their vote to some of their constituents. Moreover, says Kingdon, the anticipated need to explain influences their decision on how to vote. They may cast a certain vote only if and when they are con-

---

elections that are not simply imitative of what we have done for presidential elections. Walter Dean Burnham, "Insulation and Responsiveness in Congressional Elections," *Political Science Quarterly*, 90 (Fall, 1975), 411–435.

[31] Kingdon, *Congressmen's Voting Decisions*, pp. 46–53.

vinced that they have a satisfactory explanation in hand. Or they may cast a certain vote because it is the vote least likely to require an explanation. Kingdon is interested in finding out why members of Congress vote the way they do. But along the way, he helps make the case for finding out why members of Congress explain the way they do. For, if the anticipated need to explain has the effect on voting that Kingdon suggests – i.e., if it makes voting more complicated and more difficult than it otherwise would be – then the act of explaining must be as problematical for House members as the act of voting. House members believe that they can win and lose constituent support through their explanations as well as through their votes. To them, therefore, voting and explaining are interrelated aspects of a single strategic problem. If that is the way House members see it, then it might be useful for political scientists to look at it that way, too – and to spend a little less of our time explaining votes and a little more of our time explaining *explanations.*

Members are, of course, called upon to vote much more often than they are called upon to explain. That is, they are never called upon to explain all their votes. Their uncertainty about which votes they will have to explain, however, leads them to prepare explanations for more votes than they need to, the need being enforced on them by dissatisfied constituents and, primarily, by the electoral challenger. The challenger, particularly, controls the explanatory agenda – or, better, tries to do so. All the uncertainty that the challenger produces for the perception of constituency, the challenger also brings to the problem of explanation.

Representatives will strike different postures regarding the need to explain. Some will explain their votes only when they feel hard pressed by constituents and/or challenger to do so. They will follow the congressional adage that "if you have to explain, you're in trouble." And their explanatory practices, if not their voting practices, will be calculated to keep them out of trouble. Other members bend over backward to explain every vote that any constituent might construe as controversial – sometimes well in advance of the vote. It is our hunch that the more issue-oriented a congressman's presentation of self, the more voluminous will be his explanations. Our Congressman B, for example, produces a heavy volume of explanation. "We have explained every difficult vote. Anyone who gives a twit about how I vote has the opportunity to know it. We have explained our votes on all the toughies." Given his presentational style, it is hard to see him adopting any other explanatory style. In both cases, the content of what he says is less important than the fact that he says it – i.e., than his style.

I shall resist the temptation to spell out all the possible relationships between the presentational and explanatory aspects of home style. But it seems obvious that they exist. We would, at the least, expect to find a "strain toward compatibility" operating between the two. And we would expect to find the presentation of self – as the centerpiece of home style – to be the more controlling aspect in the relationship. We would further expect both aspects of home style to be influenced by the same constituency constraints. For example, we would expect the broadest perception of me-in-the-constituency – the sense of fit the congressman has with his various constituencies – to underlie the choice of explanatory styles, just as it underlies the choice of presentational styles. A good illustration of this latter point lies outside my particular group of House members. It lies, instead, in the explanation of their historic impeachment vote by two members of the House Judiciary Committee.

In the face of extraordinary constituency interest, two Judiciary Committee Republicans took to statewide television and radio to explain their upcoming vote in favor of impeaching their own party's president. Representative Lawrence Hogan of Maryland cast his explanation heavily in terms of voting his individual conscience against the acknowledged wishes of many prospective constituencies – primary and re-election – in his announced try for the Maryland governorship. To accent his act of conscience, he acknowledged the grave risks to his career.

> I know that many of my friends, in and out of Congress, will be very displeased with me. I know that some of my financial contributors (who have staunchly supported Richard Nixon and me) will no longer support me. I know that some of my long-time campaign workers will no longer campaign for me. But to those who were my campaign workers back in my first campaign, I want to remind you of something. Remember, I was running for Congress as a Republican in an area that was registered 3–1 Democratic, and in an effort to convince Democrats that they should vote for me, a Republican, I quoted John F. Kennedy who said: 'Sometimes party loyalty demands too much.' Remember that?
>
> Well, those words have been coming back to haunt me in recent weeks. Clearly, this is an occasion when 'party loyalty demands too much.' To base this decision on politics would not only violate my own conscience, but would

> also be a breach of my own oath of office to uphold the Constitution of the United States. This vote may result not only in defeat in my campaign for governor of Maryland, but may end my future political career. But that pales into insignificance when weighted against my historic duty to vote as my conscience dictates.[32]

Representative William Cohen cast his explanation in terms of voting as the people of his state of Maine would vote if they were in his shoes. He did not say they were pressuring him to vote, but that he and they, as members of the same community, thought alike on fundamental matters.

> I have tried to put all of these events into the context of a political system that I know well, that of the state of Maine. I have asked myself some questions.
>
> What if the governor of Maine ordered his aides to keep a list of those people who supported his opponents? What if he tried to have the state treasurer's department conduct audits of those who voiced dissent? What if he ordered that state police to investigate those who were critical of his policies or speeches? What if he asked aides to lie before legislative committees and judicial bodies? What if he approved of burglaries in order to smear and destroy a man's credibility? What if he obtained information that was to be presented to a grand jury for the purpose of helping his advisors design a strategy for defense?
>
> What would the people of Maine say? You and I both know that the people of Maine would not stand for such a situation, for it is inconsistent with our principles and our constitutional system of government.[33]

Hogan and Cohen offered their listeners very different kinds of explanations for the same vote. It is our hunch that underlying the difference in their explanatory styles are different perceptions of me-in-the-constituency. Cohen sees himself, we would guess, as part of a fairly homogeneous re-election-plus-primary constituency. He identifies strongly with it; and he has a confortable sense of fit with this broad constituency. That being so, he would neither perceive nor explain his vote in terms that set his conscience against theirs. His explanation was, therefore, communitarian. "I am one of you, and the issue is our conscience as a community." Hogan, we would guess, perceives his geographical constituency to be heterogeneous, and has no strong sense of fit with any large or differentiable element of it. Almost surely he feels uncertainty about his prospective statewide constituency; but it seems that he feels uncertainty, even, about his relationships with the primary constituency in his district. It is hard, therefore, for him to find any communitarian bases for explanation. His explanation was individualistic. "I am following my conscience and disregarding the opinions of my constituents."

Neither explanation is inherently "better" than the other. Each had an element of strategic calculation to it. That is, each man chose his words deliberately; and, presumably, neither wanted to lose the next election by virtue of his explanation. That being the case, the acceptability of the two explanations by the Maryland and Maine constituencies becomes a question of some moment. We lack sufficient information to assess the actual impact of the two explanations on each man's political support. But we can try a more general answer. To the degree that Maryland voters already perceived Hogan to be "a man of conscience," and to the degree that Maine voters already perceived Cohen to be "one of us," their explanations probably would have been accepted. On the other hand, to the degree that Maryland voters had already come to view Hogan as a self-serving opportunist, and to the degree that Maine voters had already come to view Cohen as a spineless follower of the herd, their explanations probably would not have been accepted. That is, the credibility of any given explanation probably depends less on the content of the explanation itself than on its compatibility with some previously established perception of the explainer "as a person." "Issues" said one member, "are not as important as the treatment of issues."

Their reasoning goes something like this: There probably are, in every district, one or two issues on which the congressman is constrained in his voting by the views of his re-election constituency. (Whether he *feels* constrained – which depends on whether or not he agrees with those constituent views – is beside the point.) But on the vast majority of votes, a congressman can do as he wishes – provided only that he can, if and when he needs to, explain his vote to the satisfaction of interested constituents. The ability to get his explanations accepted at home is, then, the essential underpinning of his voting leeway in Washington. Thus the question arises, how can the congressman act so as to increase the likelihood that his explanations will be accepted at home. And the answer the House members give is: he can win and hold constituent trust. The more your various constituencies trust you, House members reason, the less likely they are to require an explanation of your votes and the more likely they are to accept your explanation when they do require it. The winning of trust, we have said earlier, depends largely on the presentation of self. Presentation of self, then, not only helps win votes at election time; it also makes voting in Washington easier. So Congressmen make a strategic calculation. Presentation of self enhances trust; trust enhances the acceptability of explanations; the acceptability of explanations enhances voting leeway; therefore, presentation of self enhances voting leeway.

---

[32] *Congressional Record*, Daily Edition, July 23, 1974, H6962–H6963.

[33] *Congressional Record*, Daily Edition, August 1, 1974, E5209–E5210.

When I asked Congressman C if he wasn't more liberal than his district, he said:

> Hell, yes, but don't quote me on that. It's the biggest part of my problem – to keep people from thinking I'm a radical liberal. How do you explain to a group of Polish Catholics why you voted to abolish the House Internal Security Committee or why you voted against a bill to keep Jane Fonda from going to North Vietnam? How do you explain that? You can't.

When queried, later, on a TV interview to comment on his opponent's charge that he was "ten times more radical than George McGovern," Congressman C answered in terms of identification and trust. He said simply, "if he means by that that I'm some kind of wild-eyed radical, people around here know me better than that." Later still, he mused out loud about how he managed this problem.

> It's a weird thing how you get a district to the point where you can vote the way you want to without getting scalped for doing it. I guess you do it in two ways. You come back here a lot and let people see you, so they get a feel for you. And, secondly, I go out of my way to disagree with people on specific issues. That way, they know you aren't trying to snow them. And when you vote against their views, they'll say, 'Well, he's got his reasons.' They'll trust you. I think that's it. If they trust you, you can vote the way you want to and it won't hurt.

A pair of examples from two different types of districts will illustrate variations on this theme.

Congressman D perceives his urban, predominantly black district to be homogeneous. His re-election constituency is "the whole black community"; and his primary constituency consists of the civic-minded, middle-class activists who have organized black politics in his city. As tends to be the case in homogeneous districts, the line between his re-election and his primary constituencies is not sharp. Congressman D, who was born, raised, and employed in his district, sees himself as a microcosm of both groups. Voting in Congress, therefore, is rarely problematical for him. "I don't have any trouble knowing what the black community thinks or wants. . . . I don't have any trouble voting. When I vote my conscience as a black man, I vote right for the district." Though he does not feel constrained, he, of course, is. "If I voted against civil rights legislation, my people would probably ask me why I did that. But I never would do it." On one occasion when it might have looked to them as though he had, he explained and they were satisfied.

> When I come home, I go to the church groups and tell them what's been going on in Washington and explain to them why I voted as I did. For instance, I explained to them that I voted against the Voting Rights Bill (1970) because it was a fraud. Nixon wanted to get the fifty-seven Registrars working in the South out of there. After they heard me on the Voting Rights Bill, they went home mad. I know my people will agree with me.

Congressman D falls in the "medium frequency" category in trips home, having made thirty such trips in 1973. "I meet with church groups and other groups; and I let people see me just to let them know I haven't lost touch with them." The "other group" with which he meets most frequently (once a month formally and more often informally) is his district's political organization – "my political lifeline." "The more people see me working for them in our organization, the more popular I become, the more they trust me and are proud of me." From this presentation of self comes, then, the essential condition for his voting leeway in Washington.

> The fact is that I have the freedom to do almost anything I want to do in Congress and it won't affect me a bit back home. My constituents don't know how I vote, but they know *me* and they trust me. . . . They say to themselves, 'Everything we know about him tells us he's up there doing a good job for us.' It's a blind faith type of thing.

Congressman E describes his district as "heterogeneous – one-third urban, one-third suburban, one-third rural." His primary constituency is the rural area, where three generations of his family have lived. At the very moment we turned off a four-lane highway onto a back road on our way to a small town fair, he said,

> It must be terrible to be without roots, without a place to call home. I have a profound sense of identification with these rural people. My wife still worries about me a little bit in this respect – that I'm too much of a country boy. But life's too short to play a role or strike a pose.

> This will be fun. I'm really going to enjoy myself.

Not only is he clearly most "at home" in the rural part of the district, but it casts a strategically disproportionate 40 per cent of his party's primary vote. "If these city people (in my party) with their slick city ways think they could go out to Mrs. O'Leary's cow pasture and get the farmers to throw out the local boy, they're crazy." His presentation of self is skewed toward his primary constituency. During his 35 trips and 100 working days at home in 1973, his scheduled appearances were allocated as follows: 41 in the urban area, 38 in the suburban area and 70 (or about 50 per cent) in the rural one-third of the district. His accessibility – especially to his primary constituency – is the essence of his presentational style and, hence, he believes, the basis for constituent trust.

> Sometimes I do my talking to the same people over and over again. But they talk to others and they speak favorably about me. They tell others that 'old George' is always available and accessible. And I get a reputation in that way. That's how I succeed in this kind of district. People think of me as a nice guy, one of the boys, and they make presumptions in my favor because I'm a nice guy.

It is sometimes argued that representatives from heterogeneous districts enjoy a special degree of voting leeway because no single constituency interest controls their electoral future. Congressman E makes this argument. When asked if any single vote could defeat him in his district, he replied, "No, not in my district. It's too diverse, not all urban ghetto or Idaho potato farmers. That gives me a chance to balance interests in my votes. There really aren't any dominant interests." The crucial extra ingredient in the argument – one usually left out – is this: The congressman must be able to explain his voting pattern to his constituencies and have that explanation accepted. And this is especially true as regards his primary constituency. Congressman E commented,

> If I want to vote for an urban program, I can do it, and the people in the rural area will say, 'He does have an urban constituency and he has to help them, too.' And they will still vote for me so long as they think I'm a nice fella. But if I had no urban constituents – if I had all countryside – and I voted for an urban program, people in the rural areas would say, 'He's running for governor, he's forgotten who his friends are.' The same is true in the urban area. They know I'm a country boy and that I have a lot of rural area. So they say, 'He gives us a vote once in a while; he's probably all right.'

Congressman E is not unconstrained. He would, he admits, lose his primary constituency if he voted consistently for urban programs.[34] He will also lose his primary constituency if they stop trusting him as "a nice fella" and no longer make "presumptions in my favor" when he explains his few urban votes. If we are to understand a congressman's voting patterns in Washington, it seems that we must also understand his presentational and explanatory patterns at home.

As a final note, two general patterns of explanation deserve mention. One I had expected to find but have not, and one I had not expected to find but have. Both invite further research. In view of the commonly held notion that elective politicians "talk out of both sides of their mouths" (which Goffman discusses in terms of performances before "segregated audiences"),[35] I had expected to find members of Congress explaining their activity somewhat differently to their various constituencies. The likelihood seemed especially strong in heterogeneous districts, where the opportunity and temptation would be greatest. But I have found little trace of such explanatory chameleons in my travels. The House members I observed give the same explanations for their Washington activity before people who disagree with them as before people who agree with them – before nonsupporters as well as supporters, from one end to the other in the most segmented of districts. The lack of this kind of demagoguery, and the patient doggedness with which members explained their activities to unsympathetic audiences surprised me. I do not mean they went out of their way to find disagreement (though such a practice is of central importance to the presentation of self in some cases – such as Congressman C). I only mean that when disagreement was present, members offered the same explanation for their vote that they offered under all other conditions. Their presentation of self may vary from group to group in the sense that the basis for demonstrating identification or empathy will have to differ from group to group. As they reach out to each group in a manner appropriate to that group, they may take on some local coloration; and they may tailor their subject matter to fit the interests of their audience. However, they rarely alter their explanations of their Washington activity in the process.

---

[34] Kindgon's respondents told him that they could explain one vote that went contrary to constituent expectations, but not "a string of votes." Kingdon, pp. 41–42.

[35] Goffman, *Presentation of Self in Everyday Life,* pp. 136ff. A possible pattern of explanation requiring further research is that members explain their activities in different policy areas to distinctive groups in the constituency. See Clausen, *How Congressmen Decide,* especially his discussion of a special foreign policy constituency on pp. 225–226.

An explanatory pattern I had not expected to find was the degree to which the congressman at home explains his Washington activity by disassociating it from the activity of his colleagues and of Congress as a whole. I had assumed that home styles would be highly individualized. And I should not have been surprised, therefore, when I heard every one of my seventeen members introduced at home as "the best congressman in the United States." But I was not prepared to find each of them polishing this individual reputation at the expense of the institutional reputation of the Congress.[36] In explaining what he was doing in Washington, every one of my House members took the opportunity to portray himself as "different from the others" – the others being "the old chairmen," "the inexperienced newcomers," "the tools of organized labor," "the tools of big business," "the fiscally irresponsible liberals," "the short-sighted conservatives," "the ineffective leadership," "the obstructionist minority," "those who put selfish concerns before country," and so on. The diversity of the House provides every member with plenty of collegial villains to flay before supportive constituents at home. Individual members do not take responsibility for the performance of Congress; rather each portrays himself as a fighter against its manifest shortcomings. Their willingness, at some point, to stand and defend their votes contrasts sharply with their disposition to run and hide when a defense of Congress is called for. Congress is not "we"; it is "they." And members of Congress run *for* Congress by running *against* Congress. Thus, individual explanations carry with them a heavy dosage of critical commentary on Congress.

Conclusion: Political Support, Home Style, and Representation

"A congressman has two constituencies," Speaker Sam Rayburn once said. "He has his constituents at home and he has his colleagues here in the House. To serve his constituents at home, he must serve his colleagues here in the House." For over twenty years, political scientists have been researching the "two constituencies." Following the thrust of Rayburn's comment, we have given lopsided attention to the collegial constituency on Capitol Hill. And we have neglected the constituency at home. Knowing less than we might about one of the two constituencies, we cannot know all that we should about the linkage between them. This paper argues for opening up the home constituency to more political science investigation than it has received. It suggests that students of Congress pay more attention to "home" as a research focus and a research site. No one can say what we might learn if the suggestion were heeded. But a few speculations will serve as a conclusion to this exploratory effort.

For one thing, it appears that a congressman's constituency is more complicated than our normal treatment of it in our literature suggests. We have not, of course, obtained a constituent's-eye view of the constituency, having tried to keep the congressman's perceptions as our sole vantage point. But from that vantage point alone, it seems that we must be more precise about what we mean by "his (or her) constituents." If we are going to continue to talk in the language of role orientations – in which the "trustee's" votes are not determined by his constituents, and in which the "delegate" does follow the wishes of his constituents – we shall have to know just which "constituents" we (and he) are talking about. If we are going to continue to do survey research – in which we match the attitudes of the congressman with the attitudes of his constituents, or with his perceptions of the attitudes of his constituents – we shall, again, have to know just what "constituents" we (and he) are talking about. The abstraction "his (or her) constituents" is only slightly more useful as an analytical variable than the abstraction "the people." It may be that the distinctions attempted here are not the most useful ways of dividing up the home constituency for analytical purposes. But the days when our literature acknowledges the complexities of politics on Capitol Hill while accepting the most simplistic surrogates for political reality at home – those days ought to be numbered.

While this article has not dwelled directly on the topic of representation, our exploration has implications for the family of questions, both descriptive and normative, raised by studies of representation. For people studying the conditions of electoral accountability, this study has something to say about the electoral accountability of members of Congress. They are not among Kenneth Prewitt's "volunteerists."[37] House members work hard to get the job and work hard to keep it. In the course of that effort, they expend a great deal of time and effort keeping in touch with their various constituencies at home. Furthermore, they feel much more uncertain, insecure, and vulnerable electorally than our objective measures reveal. When a congressman describes his seat as "safe," he implicitly adds: "because, and so long as, I work actively to keep it so." And many a House retirement decision is made in anticipation of electoral difficulty. Surely, then, some of the necessary conditions for electoral accountability are present in Congress as they are not in certain other representative bodies in this country. But the existence of such conditions does not, by itself, tell us whether House members are as responsive to constituent desires as democratic theory may require. Our study can provide, however, some perspectives with which to pursue this normative inquiry.

---

[36] See also, Richard F. Fenno, "If, as Ralph Nader Says, Congress is 'The Broken Branch,' How Come We Love Our Congressmen So Much?", paper presented to Time, Inc. Symposium, Boston, Massachusetts, December, 1972, and reprinted in *Congress in Change*, ed. Norman Ornstein (New York: Praeger, 1975).

For example, it appears that members of Congress feel a good bit more accountable – and, hence, I would guess, are more responsible – to some constituents than to others. House members feel more accountable to some constituents than to others because the support of some constituents is more important to them than the support of others. Thus, the process by which people become and remain representatives is closely related to their activity while they are representatives. In a representative democracy, the process is electoral; and the central problem for the representative is that of winning and holding voter support. From the viewpoint of this paper, problems of representation and problems of support are inseparable. It is precisely because a congressman's right to represent depends on his cultivation of electoral support that he develops such a complex and discriminating set of perceptions about his constituents. In a representative democracy, the representative learns who his, or her, various constituencies are by campaigning for support among them. The one deficiency in Hanna Pitkin's splendid book is the undifferentiated, uncomplicated idea of the constituency which she employs in her study of representation.[38] And her monolithic view of "the constituency" remains viable only because she separates the process of running for office from the problems of representation. The traditional view of representation is somewhat more static and structural than the process-oriented view one gets by focusing on the cultivation of constituent support. To the congressman, the very word "political" connotes the ongoing problem of support. A "political vote" is one calculated to win (or to avoid losing) support. A "politically unwise move" is one that will lose support, etc. Focusing on the problem of support keeps the dynamism and the politics in the subject of representation. If politics is about "who gets what, when, and how," then the politics of representative democracy is about "who supports whom, when, and how – and how much." And, "what do various constituencies get in return for their support?"

A more inclusive, process-oriented view of representation has the effect of making it less exclusively a policy-centered subject. Traditionally, representation has been treated mostly as a structural relationship in which the congruence between the policy preferences of the represented and the policy decisions of the representative is the measure of good representation. The question we normally ask is: "How well does Representative X represent his or her district?" And we answer the question by matching and calibrating substantive policy agreement. But our view here is that there is an intertwining question: "How does Representative X carry his or her district?" To answer that question, we shall need to consider more than policy preferences and policy agreements. We shall need to consider the more encompassing subject of home style and the constituent trust generated by home style. We shall need to entertain the possibility that constituents may want good access as much as good policy from their representative. They may want "a good man" or "a good woman" whom they judge on the basis of home style and whom they trust to be a good representative in terms of policy. Indeed, the growing political science literature on voter behavior in congressional elections contains both evidence and speculation -- usually under the rubric of the rising pro-incumbent vote – that voters are looking increasingly to just such candidate-centered bases for tendering electoral support.[39] The point is not that policy preferences are not a crucial basis for the representational relationship. They are. The point is that we should not start our studies of representation by assuming they are the *only* basis for a representational relationship. They are not.

---

[37] Kenneth Prewitt, "Political Ambitions, Volunteerism and Electoral Accountability," *American Political Science Review*, 64 (March, 1970), 5–17.

[38] Hanna F. Pitkin, *The Concept of Representation* (Berkeley: University of California Press, 1972). Pitkin recognizes the complexity of constituencies – see p. 220 of her book – but she does not incorporate the idea into her analysis.

[39] Alan Abramowitz, "Name Familiarity, Reputation and the Incumbency Effect in a Congressional

This last comment may be as valid normatively as it is empirically. What reason is there to believe that a relationship based on policy is superior to one based on home style – of which policy is, at most, only a part – as a normative standard for representative democracy? It may be objected that a search for support that stresses stylistic compatibilities between the representative and the represented easily degenerates into pure image-selling. And, of course, it may. But the search for support that emphasizes policy compatibilities between the representative and the represented easily degenerates into pure position-taking. It is, perhaps, the signal contribution of David Mayhew's elegant essay to make exactly this point.[40] Position taking is just as misleading to constituents and as manipulative of their desires as image selling. Both representational bases, we conclude, may take a corrupt form. Appearing to do something about policy without a serious intention of, or demonstrable capacity for, doing so is no less a corruption of the representative relationship, no less an impediment to accountability and responsiveness than is the feigning of a personal relationship without a serious intention of establishing one. They are equally corrupt, equally demagogic. They are substitutes for any real effort to help make a viable public policy or to establish genuine two-way communication and trust. At the least, normative theory ought to take account of both policy and extra-policy standards of good representation, and acknowledge their respective corruptions.

Our concentration on and our preference for the policy aspects of representation carries a related implication. For a people who profess an attachment to representative democracy, we have always seemed curiously uncomfortable when our representatives devote themselves to contact with their constituents. We tend to denigrate the home part of the representational process as mere "errand-running" or "fence-mending" and to assume that it takes the representative away from that which he, or she, really should be doing, i.e., making public policy in Washington. As one small example, we have always criticized, out of hand, the "Tuesday to Thursday Club" of House members who go home for long weekends – on the assumption, presumably, that going home was *ipso facto* bad. But we never inquired into what they did at home or what the consequences (other than their obvious dereliction of duty) of their home activity might have been. Predictably, the home activities described in this paper will be regarded, by some, as further evidence that members of Congress spend too little of their time "on the job." But this paper asks that we entertain the alternate view that the Washington and the home activities can be mutually supportive. Time spent at home can be time spent developing leeway for activity undertaken in Washington. And it may be that leeway in Washington should be more valued than the sheer number of contact hours spent there. It may be, then, that the congressman's effectiveness in Washington is vitally influenced by the pattern of support he has developed at home and by the allocational, presentational, and explanatory styles he displays there. To put the point most strongly, perhaps we cannot understand his Washington activity without first understanding his perception of his constituencies and the home style he uses to cultivate their support.

No matter how supportive of one another their Washington and home activities may be, House members still face constant tension between them. Members cannot be in two places at once. They cannot achieve legislative competence and maintain constituency contact, both to an optimal degree. The tension is not likely to abate. The legislative workload and the demand for legislative expertise are growing. And the problems of maintaining meaningful contact with their several constituencies – which may make different demands upon them – are also growing. Years ago, House members returned home for months at a time to live with their supportive constituencies, soak up the

---

Election," *The Western Political Science Quarterly*, 28 (December, 1975), 668–684; Albert Cover, "One Good Term Deserves Another: The Advantage of Incumbency in Congressional Elections," paper delivered at American Political Science Association Convention, Chicago, September 1976; John Ferejohn, "On the Decline of Competition in Congressional Elections," *American Political Science Review*, 71 (March, 1977), 166–176; Morris Fiorina, "The Case of the Vanishing Marginals: The Bureaucracy Did It," *American Political Science Review*, 71 (March, 1977), 177–181; Raymond Wolfinger and Robert Arseneau, "Voting Behavior in Congressional Elections." See also Edward N. Muller, "The Representation of Citizens by Political Authorities: Consequences for Regime Support," *American Political Science Review*, 64 (December, 1970), 1149–1166, esp. p. 1157; Glenn R. Parker and Roger H. Davidson, "Bases of Public Assessments of Government Performance: The Content of Congressional Evaluations," unpublished paper, University of California at Santa Barbara.

[40] Mayhew, *The Electoral Connection*.

home atmosphere, absorb local problems at first hand. Today, they race home for a day, a weekend, a week at a time. The citizen demand for access, for communication, for the establishment of trust is as great as ever. The political necessity and the representational desirability of going home is as great as ever. So members of Congress go home. But the quality of their contact has deteriorated. It is harder to sustain a genuine two-way relationship – of a policy or an extra-policy sort – than it once was. They worry about it and as they do, the strain and frustration of the job increases. Many cope; others retire. Indeed, retirements from the House appear to be increasing. Political scientists do not know exactly why.[41] But the research reported in this article points to the possibility that an inability or an unwillingness to improve the quality of the home relationship may be a contributing factor. Those who cannot stand the heat of the home relationship may be getting out of the House kitchen. If so, people prepared to be more attentive to home are likely to replace them. Thus, our focus on home activity may help us understand some changing characteristics of House members.

Our professional neglect of the home relationship has probably contributed to a more general neglect of the representational side of Congress's institutional capabilities. At least it does seem to be the case that the more one focuses on the home activities of its members, the more one comes to appreciate the representative strengths and possibilities of Congress. Congress *is* the most representative of our national political institutions.[42] It mirrors much of our national diversity, and its members maintain contact with a variety of constituencies at home. While its representative strengths surely contribute to its deserved reputation as our slow institution, the same representative strengths give it the potential for acquiring a reputation as our fair institution. In a period of our national life when citizen sacrifice will be called for, what we shall be needing from our political institutions are not quick decisions, but fair decisions. Some of the recent internal congressional reforms have increased the potential for both representativeness and fairness. They have increased the equality of representation by distributing influence more broadly inside Congress. They have increased the visibility of representation by opening up congressional proceedings to public view. This is by no means all that is needed. But it is enough to place an added obligation on the members of Congress to educate their constituencies in the strengths as well as the weaknesses of the institution. Members should participate in consensus building in Washington; they should accept some responsibility for the collective performance of Congress; and they should explain to their constituents what an institution that is both collective and fair requires of its individual members.

The evidence of this study indicates that members have the leeway to educate their constituents if they have the will to do so. The evidence of this study also shows that they may not have that will. Instead, they often seek support and trust for themselves by encouraging a lack of support and trust in the Congress. By refusing to accept responsibility for an institutional performance, they simultaneously abdicate their responsibility to educate their constituents in the work of the institution. Instead of criticizing House members for going home so much, we should criticize them for any failure to put their leeway to a constructive purpose – during the legislative process in Washington and during the explanatory process at home. The trust of his, or her, supportive constituents should be viewed by a House member as his, or her, working capital – not just to be hoarded for individual benefit but to be drawn on, occasionally, for the benefit of the institution. So long as House members explain themselves but not the institution, they help sustain (wittingly or unwittingly) the gap between a 10 per cent approval level for Congress and a 90 per cent re-election record for themselves. If this imbalance seems unhealthy for both Congress and the Republic, we have yet another justification for an increased scholarly attentiveness to our House members at home.

---

[41] See Stephen Frantzich, "Congressional De-Recruitment: A New Look at Turnover in House Membership," unpublished paper, Hamilton College, 1975.

[42] The argument of the last paragraphs is more fully contained in Richard Fenno, "Strengthening a Congressional Strength," paper presented at a conference on "The Role of Congress" sponsored by Time, Inc., Washington, 1975, reprinted in *Congress Reconsidered*, ed. Lawrence Dodd and Bruce Oppenheimer (New York: Praeger, 1977).

Partisan Patterns of House Leadership Change, 1789—1977*

GARRISON NELSON
University of Vermont

> Of all these Formes of Government, the matter being mortall, so that not onely Monarchs, but also whole Assemblies dy, it is necessary for the conservation of the peace of men, that as there was order taken for an Artificiall Man [the state], so there be order also taken for an Artificial Eternity of life.... This Artificiall Eternity is that which men call the Right of Succession.
>
> – Thomas Hobbes, *Leviathan*

Introduction

Few of the world's legislative institutions have remained as unchanged over as long a period of time as the United States House of Representatives. Unlike the assemblies of France, Germany, and Italy, it has not had to contend with a number of regime changes and functional redefinitions. Unlike the British House of Commons, it has maintained its basic form of representation for almost two centuries. Extending the suffrage to the landless, the blacks, and women has not significantly changed the white, male middle-class character of its membership. The two-party system which exists within it today is very similar to the one which it spawned early in its existence.

The House has enjoyed the luxury of stability and continuity. When the few successful challenges to this continuity have occurred, they have become almost legendary. The "revolt" against Speaker Cannon in 1910–1911 occurred six decades ago, but so little has happened in the House since then, that this incident has taken on grandiose proportions. It has been recounted by congressional analysts in a dozen books and articles.[1] Yet it rates only a paragraph in most of the widely-read American history textbooks.

Perhaps the real story of the House is its continuity and not its changes. This may account for the popularity and widespread acceptance of Nelson Polsby's "institutionalization" thesis.[2] The "institutionalized House" which Polsby describes has become much like the "Artificiall Man" of the Hobbes quotation opening this paper. The House now appears to have a life of its own, separated from the parties, committees, and leaders who occupy space within it. The accumulated momentum of almost two centuries has left the House's legislative parties usually more in agreement than disagreement, most of its bipartisan legislative committees with institutional and not partisan loyalties, and a leadership shaped by the House not the shapers of it. The portrait of the House drawn by Polsby, Peabody, Huntington, Hinckley, Jones, Ripley, and others is that of a legislative assembly characterized by internal bureaucratization, electoral insulation, and

---

*I would like to extend my appreciation to Jerome Clubb, Samuel Patterson, James Sundquist, and David Vogler for their thoughtful critiques of earlier incarnations of this manuscript and to W. Ross Brewer, Kathleen Frankovic, Barbara Hinckley, Norman Ornstein, Robert Peabody, and John Wahlke for their helpful comments. An earlier version of this paper was presented to the 70th Annual Meeting of the American Political Science Association, Chicago, August 29–September 2, 1974.

[1] Among some of the better-known studies of the revolt are: Charles R. Atkinson, *The Committee on Rules and the Overthrow of Speaker Cannon* (New York: Columbia University Press, 1911); William Rea Gwinn, *Uncle Joe Cannon, Archfoe of Insurgency: A History of the Rise and Fall of Cannonism* (New York: Bookman Associates, 1957); Kenneth W. Hechler, *Insurgency: Personalities and Politics of the Taft Era* (New York: Columbia University Press, 1940), Chapter III; and James Holt, *Congressional Insurgents and the Party System, 1909–1916* (Cambridge, Mass.: Harvard University Press, 1967), Chapter 2. An effort to parallel the revolt against Cannon with the 1961 Rules Committee fight may be found in Charles O. Jones, "Joseph G. Cannon and Howard W. Smith: An Essay on the Limits of Leadership in the House of Representatives," *Journal of Politics,* 30 (August, 1968), 617–646.

[2] Nelson W. Polsby, "The Institutionalization of the U.S. House of Representatives," *American Political Science Review,* 62 (March, 1968), 144–168; and Nelson W. Polsby, Miriam Gallaher, and Barry Spencer Rundquist, "The Growth of the Seniority System in the U.S. House of Representatives," *American Political Science Review,* 63 (September, 1969), 787–807.

institutional autonomy marching most often to the regular beat of its own well-worn drum.[3]

The "Artificiall Eternity" which Hobbes bestows upon legislative assemblies manifests itself in the floor leadership succession system of the House. Analysts of congressional leadership have detected signs of institutional routinization in succession patterns which appear to transcend political, partisan, and personality considerations. One attains a post because one occupies the penultimate position which leads to it. Barbara Hinckley's statement addresses this phenomenon directly:

> Although formally elective, the actual selection bears all the marks of a much more automatic procedure. Leaders stay in their posts for a number of congresses. New leaders are recruited from apprenticeship posts. Whip leads to floor leader. Floor leader to Speaker.[4]

Hinckley goes on to suggest that both parties in both houses operate this way in the selection and retention of their leaders. Exceptions have occurred and are duly noted. The thrust of Hinckley's analysis, however, is toward the identification of institutionalized leadership selection norms which seem to triumph over party-related and chamber-related ones.

Hinckley relies upon Robert Peabody's 1967 analysis of leadership change to account for the "violations" of these congressional norms that occur. Peabody contended that change often represents the internal consequences of the external factor of election results: majority or minority status and the aggregate size of the net gain or loss of seats. As explained by Peabody:

> Strong victories promote good will and generally reflect to the benefit of party leaders. Conversely, defeat results in pessimism, hostility and a search for scapegoats. If the net losses are particularly severe, as many as thirty to fifty seats, then the possibilities of minority leadership change through revolt are greatly enhanced.[5]

Combining the Hinckley and Peabody analyses, two propositions emerge: Since House leadership succession patterns have been institutionalized for both parties, the only violations of these patterns will occur during periods of party frustration. The inference encompasses all of the House changes in the six Democratic Congresses which Peabody observed in his initial study (1955–1966) and all but three of the changes in the ten postwar Congresses which Hinckley examined (1947–1966). While this proposition may illuminate change in the contemporary House, it is less useful in explaining leadership change in previous Congresses for the simple reason that the present era in the House is very atypical. Democratic dominance of the House in twenty-two of the twenty-four Congresses since 1931 has produced an even more persistent era of one-party control than that of the Democratic-Republicans in the first twelve Congresses of the nineteenth century. Assertions about the impact of majority or minority status on party leadership in the contemporary House may mask genuine differences between the Democrats and Republicans.

Peabody's latest study, *Leadership in Congress*, recognizes the fact that the present-day uniqueness, "makes it impossible to prove or disprove any hypotheses relating leadership change to majority-minority status as over and against party differences, Democrat and Republican. For such interpretations longer range historical refinement will be required."[6]

---

[3] Certainly not all of the authors listed here would agree totally with my characterization of the House, but their studies provide sufficient data to enable one to gain this portrait. In addition to the Polsby articles cited above, some of the recent books and articles which have tried to assess the contemporary House within an institutionalized context are: Michael Abram and Joseph Cooper, "The Rise of Seniority in the House of Representatives," *Polity*, 1 (Fall, 1968), 52–85; Roger H. Davidson, *The Role of the Congressman* (New York: Pegasus, 1969), esp. Chap. 2; Samuel P. Huntington, "Congressional Responses to the Twentieth Century," in *The Congress and America's Future*, ed. David B. Truman (Englewood Cliffs, New Jersey: Prentice-Hall, 1965), 5–31; Charles O. Jones, *Every Second Year: Congressional Behavior and the Two-Year Term* (Washington, D.C.: The Brookings Institution, 1967); Charles O. Jones, *The Minority Party in Congress* (Boston: Little Brown & Co., 1970); Randall B. Ripley, *Party Leaders in the House of Representatives* (Washington, D.C.: The Brookings Institution, 1967); Randall B. Ripley, *Majority Party Leadership in Congress* (Boston: Little Brown & Co., 1969); Barbara Hinckley, *Stability and Change in Congress* (New York: Harper & Row, 1971); Robert L. Peabody, "Party Leadership Change in the United States House of Representatives," *American Political Science Review*, 61 (September, 1967), 675–693; and T. Richard Wittmer, "The Aging of the House," *Political Science Quarterly*, 79 (December, 1964), 526–541; and the articles by H. Douglas Price and Morris P. Fiorina, David W. Rohde, and Peter Wissel in *Congress in Change: Evolution and Reform*, ed. Norman J. Ornstein (New York: Praeger Publishers, 1975), 2–57.

[4] Hinckley, *Stability and Change in Congress*, p. 121. She also deals with this subject in greater detail in "Congressional Leadership Selection and Support: A Comparative Analysis," *Journal of Politics*, 32 (May, 1970), 268–287, esp. pp. 275–278.

[5] Peabody, "Party Leadership Change," pp. 687–688.

[6] Robert L. Peabody, *Leadership in Congress: Stability, Succession, and Change* (Boston: Little Brown & Co., 1976), p. 443. Peabody also indicates that the implication of his earlier research that party differen-

It is in the spirit of providing the historical dimension that this study was undertaken for it is my belief that the contemporary quest for both institutional norms of leadership succession and electoral system predictors of leadership change has led to an undervaluation of the impact of political party identity upon the selection of House leaders.

Party factors persist. For years, legislative analysts have contended that party identity is the single best predictor of congressional voting.[7] David Mayhew, for example, has pointed out that the House Democrats have become an "inclusive" party, attentive to the demands of many interests, while the House Republicans have become an "exclusive" party limited to a smaller range of interests.[8] Regarding the recruitment of House members, Joseph Schlesinger has observed that both parties are "office-based" but that the Democrats appear to be recruited in a more "open" system while the Republican system appears to be more "hierarchic."[9]

Since party members in the House have been recruited differently and seem to vote differently, these differences should also manifest themselves in each party's leadership selection system. In order to test this party-related proposition, the analysis was extended back to the First Congress. In this way, changes over time could be identified and periods could be analyzed in which a number of parties – not just the contemporary Democrats – held majority status.

For this analysis the voting records on the floor during speakership contests, votes in the various party caucuses, retention rates of House leaders in their positions, and the reasons for the leadership departing were examined. These four sets of data make it possible to discern both party-related and time-related impacts upon the leadership selection process in the House.

House Leaders: Positions and Periods

Of all the approaches to leadership identification, the simplest to understand is the "positional" or "formal-leadership" approach which selects as leaders the occupants of key posts.[10] Within the House of Representatives, the most important organizational positions are those of Speaker, floor leader, and whip. Not all holders of these posts have been prominent members of the House, nor have they even enjoyed the full confidence of the legislative parties which selected them, but there is no question that these offices are crucial, regardless of their occupants. For this reason, any member who held the office of Speaker, floor leader, or party whip must be categorized as a leader of the House.

Positions. Apart from the Speakership, no leadership position can be consistently defined over the entire course of House history. Different positions during different eras fulfilled the functions of floor leadership. The floor leadership of the majority party was first associated with the appointed chairmanship of the Ways and Means Committee shortly after its creation as a standing committee in 1795.[11] When the Appropriations Committee came into existence in 1865, the floor leadership was often shared by both chairmen. Movement between these two committees was not infrequent and their chairmen and ranking members often served with the Speaker on the Rules

---

ces were less important than status differences has been changed. He now sees the party identity factor in leadership change as "considerable" (p. 291).

[7] The existence of party-related voting patterns is generally acknowledged. How much of the variations in the patterns is attributable to party factors is the debatable issue. Compare two entries in the debate: Cleo H. Cherryholmes and Michael J. Shapiro, *Representatives and Roll Calls* (Indianapolis, Ind.: Bobbs-Merrill, 1969), pp. 106–110, on the importance of party influence; and Aage R. Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's Press, 1973), pp. 91–100, for a caveat.

[8] David R. Mayhew, *Party Loyalty among Congressmen: The Difference between Democrats and Republicans, 1947–1962* (Cambridge: Harvard University Press, 1966), pp. 148–160. Two nonacademic books make a similar argument: Dean Acheson, *A Democrat Looks at His Party* (New York: Harper and Brothers, 1955); and Milton Viorst, *Fall from Grace: The Republican Party and the Puritan Ethic,* rev. ed. (New York: Simon and Schuster, 1971), esp. Chap. 5 on "The Lost Constituencies."

[9] Joseph A. Schlesinger, "Political Careers and Party Leadership," in *Political Leadership in Industrialized Societies: Studies in Comparative Analysis,* ed. Lewis J. Edinger (New York: John Wiley & Sons, 1967), 266–293, esp. pp. 280–284.

[10] Wendell Bell, Richard J. Hill, and Charles R. Wright, *Public Leadership* (San Francisco: Chandler Publishing Co., 1961), p. 6.

[11] William Loughton Smith of South Carolina was the initial chairman of Ways and Means as a standing committee. He was replaced in the Third Session of that Congress by Robert Goodloe Harper, also of South Carolina. Harper is the first party floor leader mentioned by DeAlva Stanwood Alexander in his chapter on "Floor Leaders" in *History and Procedure of the House of Representatives* (Boston: Houghton, Mifflin, 1916), p. 107. For an assessment of how and when the various leadership posts came into being in the House, see Garrison Nelson, "Leadership Position-Holding in the United States House of Representatives," *Capitol Studies,* 4 (Fall, 1976), 11–36.

Committee during the peak of its influence as a vehicle for the House majority.[12] The most fundamental change in the majority leadership occurred in 1911 when the Democratic caucus asserted its authority and made the post elective.[13] Since then all majority leaders have been elected.

Minority leadership prior to 1863 was blurred by the multiplicity of parties in the House and the frequent manifestation of intraparty conflict on the floor during the balloting for Speaker.[14] After 1863 all but one of the sixty-one speakership contests were settled on the first ballot with the minority nominee receiving virtually all of the votes cast by his party.[15] Deciding to use this designation for the minority leadership represents a variation on the positional approach for the simple reason that the position did not exist until 1883 and did not receive official recognition until 1911.[16] For practical purposes I am willing to regard the minority's nominee for Speaker as the person to whom they would have entrusted the most important office in the House, hence their leader.

Party whips emerged first among the Republicans with Speaker Reed's designation of James A. Tawney in 1897.[17] The Republican leadership held the power of appointment of whips until 1919 when the caucus took control of the post.[18] In 1921 the Republican Committee on Committees elected the whip and held this responsibility until 1965 when the Republican membership reasserted its authority and made the post fully elective again.

The position of Democratic whip had a sporadic history following Oscar Underwood's initial appointment to the post in 1900.[19] Not until 1921 did the post develop clear continuity. It has been an appointive post since its inception. Both the reform-minded 94th and the newly-elected 95th Congresses defeated efforts to make the Democratic whip post elective.[20]

To summarize, the positions and relevant years used to identity the House leadership population are (1) the Speakers of the House (1789–1977); (2) the appointed chairmen of the Ways and Means Committee from its inception in the Fourth Congress to its separation from Speaker selection in the 62nd Congress (1795–1911); (3) the appointed chairmen of the Appropriations Committee from the 39th Congress through the 55th Congress when only the Ways and Means chairman was designated as majority leader (1865–1899) plus one six-year stint when James A. Tawney was its chairman (1905–1911);[21] (4) the caucus-elected majority leaders from the 62nd Congress to the present (*1911–1977*); (5) the minority's nominees for the speakership (1863–1911); (6) the caucus-elected and *Congressional Directory*-designated minority leaders (1911–1977); (7) the Republican party whips (1897–1977); and (8) the Democratic party whips (1899–1909, 1913–1915, and 1921–1977).

---

[12] Ripley indicates the presence of both positional leaders in his description of majority leadership patterns between 1861 and 1911, see his *Party Leaders in the House of Representatives*, p. 84. Thaddeus Stevens of Pennsylvania, Chairman of Ways and Means (1861–1865) was the first to chair the Appropriations Committee. Henry L. Dawes of Massachusetts chaired both Appropriations (1869–1871) and Ways and Means (1871–1875). In the years from 1880 through 1911 when the House Rules Committee functioned under the chairmanship of the Speaker, twenty-four of the other thirty-eight committee assignments held by the two other majority members were from Ways and Means (17) and Appropriations (7).

[13] Chang-Wei-Chiu, *The Speaker of the House of Representatives Since 1896* (New York: Columbia University Press, 1928), pp. 318–320. See also *The New York Times*, January 19, 1911, p. 1.

[14] Prior to 1863, there were thirteen multi-ballot speakership floor contests: the 3rd (1793–1795); 6th (1799–1801); 9th (1805–1807); 11th (1809–1811); Second Session of the 16th (1820–1821); 17th (1821–1823); 19th (1825–1827); Second Session of the 23rd (1834–1835); 26th (1839–1841); 30th (1847–1849); 31st (1849–1851); 34th (1855–1857); and the 36th (1859–1861) Congresses. See the *House Journals* for the relevant years and various issues of *Niles' Weekly Register* (Baltimore).

[15] The only exception involved Frederick Gillett of Massachusetts who was renominated by the Republican caucus in 1923 to be its candidate for Speaker, but it took nine ballots before he was able to gain the votes of twenty Republican members who cast their earlier ballots for Henry A. Cooper of Wisconsin and Martin B. Madden of Illinois. *House Journal*, 68th Congress, First Session, pp. 5–11.

[16] Ripley, *Party Leaders in the House of Representatives*, p. 28. James R. Mann of Illinois was the first to be designated "Chairman of the Minority Conference," in the *Congressional Directory*, 62nd Congress, 1st Session (May, 1911), p. 190.

[17] Randall B. Ripley, "The Party Whip Organizations in the United States House of Representatives," *American Political Science Review*, 58 (September, 1964), 561–576. Ripley, using Edward T. Taylor's *A History of the Committee on Appropriations*, House Document 299, 77th Congress, First Session (1941), p. 51, argues the case for Reed's appointment of Tawney in 1897.

[18] Ripley, *Party Leaders in the House of Representatives*, p. 33. See also *The New York Times*, March 12, 1919, p. 1.

[19] *Ibid.*, pp. 36–38.

[20] The Democratic Caucus for the 94th Congress voted down a proposal to make the whip post an elective one, 138–32, *Congressional Quarterly Weekly Report*, 33 (December 7, 1974), p. 3247. The organizing caucus for the 95th Congress "shouted down" the proposal without a vote, see the *Boston Globe* (December 9, 1976), p. 20.

[21] See "Tawney Republican Leader," in *The New York Times*, December 3, 1905, p. 3. An early comment on Tawney's aggressive role may be found in "House Chiefs in Panic at Tawney's War Howl," *The New York Times*, December 18, 1905, p. 9.

The positional approach identifies 128 men as holders of leadership posts in the House from 1789 through 1977.[22] These posts have been held by seventy-one members identified with the Democratic party or its predecessor, the Democratic-Republicans, and by fifty-four members associated with the major opponents of the Democrats. This latter figure includes six Federalists, five Whigs, one American and forty-two Republicans. Three leaders held their positions under different party labels.[23]

In Table 1, the positions are identified by relevant time frame and the number of selections and occupants for each post are enumerated. The figure of 364 selections constitutes the population of cases examined in this study.

Periods. The House's 189 year history can be divided into three eras based upon the elective-appointive dimension for the leadership posts. The first era, *1789–1863*, contains the Congresses in which only two leadership posts can be readily identified: the elected Speaker and the appointed Chairman of the Ways and Means Committee. The second era, *1863–1911*, begins with the 38th Congress, where an organized minority floor vote for Speaker can be detected. This era also includes the addition of the Appropriations Committee chairman as majority co-floor leader and the initial appearances of appointed party whips. It ends in

Table 1. House Leadership Positions and Selection Methods: Number of Selections and Occupants by Party, 1789–1977

	Number of					
	Selections			Occupants		
	Dem.	Non-Dem.	N	Dem.	Non-Dem.	N
Elective Posts						
Speaker of the House, 1789–1977	66	36	102	28	20	46[a]
Minority floor leader, 1863–1977	25	36	61	17	10	27
Majority floor leader, 1911–1977	28	8	36	11	4	15
Republican whip, 1919, 1965–1977	0	8	8	0	3	3
Semi-Elective Post						
Republican whip, 1921–1965	–	23	23	–	5	5
Appointive Post						
Chairman, Ways & Means, 1795–1911	36	27	63	23	16	38[a]
Chairman, Appropriations, 1865–1899, 1905–1911	9	13	22	4	7	11
Republican Whip, 1897–1919	–	11	11	–	5	5
Democratic Whip, 1900–1908, 1913, 1921–1977	38	–	38	16	–	16
Totals						
Elective posts	119	88	207	56	37	91[a]
Semi-elective posts	0	23	23	0	5	5
Appointive posts	83	51	134	43	28	70[a]
	202	162	364	99	70	166[a,b]

[a] Two Speakers (Muhlenberg and Taylor) and one Chairman of Ways and Means (McLane) held their posts during changes in party control.

[b] Also, two Republican Whips (Knutson and Arends) were selected by two differing methods. This further reduces the number of post occupants to 164.

---

[22] A list of the leaders selected for this study may be found in Garrison Nelson, "Appendix: Roster of Leaders of the United States House of Representatives, 1789–1976," *Capitol Studies*, 4 (Fall, 1976), 25–36.

[23] Frederick A. C. Muhlenberg served as Speaker for both the Federalist First Congress (1789–1791) and the Democratic-Republican Third Congress (1793–1795), see William Henry Smith, *Speakers of the House of Representatives of the United States* (Baltimore: Simon J. Gaeng, 1928), p. 20. The other two leaders to serve under differing party majorities were John W. Taylor and Louis McLane who served as Speaker and Ways and Means chairman respectively as National Republicans in the 19th Congress (1825–1827) after holding the same posts during the Democratic-Republican majorities earlier.

1911 with the first election of a majority floor leader by the caucus. The present era, *1911–1977,* is marked by an increase in the number of House party floor leaders directly elected by the membership.

Reducing the House's history to three time periods makes it more manageable but it does reduce the number of temporal observation points. Consequently, time-related changes can only be noted in broad outline and not as a steady progression towards some ultimate destination.

Continuity and Change in Leadership Position-Holding

In examining leadership change and continuity, one must differentiate between changes in leadership post and changes in leadership rank. The distinction is important because a leader may change his post, but not his rank. For example, leaving the speakership to become minority leader represents a change of institutional posts, but a continuity in party rank. Similarly, a leader may maintain his post but change ranks (e.g., a leader who remained in the post of whip when his party gained control of the House would be "moved" from the second-ranking post to the third-ranking one).

This particular distinction has increased in importance because of the high degree of interpositional mobility which has occurred since 1863. During the first period, only three of forty-seven leaders (6 per cent) moved between posts. Following 1863, mobility has increased with twenty-eight of the eighty-two leaders (34 per cent) having held at least two posts. The reason for this increase is that three more leadership posts were added as the minority became more recognizable and the whip system was formalized.

Both parties have moved leaders between positions in the last two eras. Since 1863, seventeen of the forty-four Democratic leaders (39 per cent) and eleven of the thirty-eight Republican leaders (29 per cent) held more than one post. One major difference occurs in recent years. The rate of interpositional mobility among Democratic leaders since 1931 is 50 per cent (9/18) while the Republican figure in the same era is only 22 per cent (2/9). There are two reasons for this party differential. First, the Democrats have become a semipermanent minority since 1931 and they have had continually one more leadership post to disburse at the opening of each Congress, namely, the speakership. Second, the emergence of the "leadership ladder" – the whip to leader to Speaker succession – has made holding the upper-level offices contingent upon holding the lower ones.

Interpositional mobility as manifested in the "leadership ladder" is deceptive as a guide to House leadership change. The first post in the sequence, that of whip, is a much more integral part of the House Democratic leadership structure than it is of the Republican one. Four Democratic majority leaders began their ascents as party whips: Oscar Underwood, Carl Albert, Hale Boggs, and Thomas P. O'Neill. In the cases of the latter three, their service as whip was a crucial element in their elections for the higher post.[24] Also, the Democrats have used the whip position as their second-ranking post in the last two Congresses in which they served in the minority. Sam Rayburn placed ex-Majority Leader John McCormack in that slot when he assumed the minority leadership in both 1947 and 1953.[25] This is not true of the Republicans. Only the first Republican whip, Tawney, moved into either of the two top leadership positions and that occurred when the floor leadership was also an appointive post. No Republican whip has been displaced by the former majority leader when the party lost control of the House.

The two parties have treated the position differently. For the House Democrats, the position of whip has become a "penultimate" one, a step away from majority leader; but for the Republicans, it is a "career office," to be surrendered only upon departure from the

---

[24] On Albert's election in 1962, see Nelson W. Polsby, "Two Strategies of Influence: Choosing a Majority Leader, 1962," in *New Perspectives on the House of Representatives,* ed. Robert L. Peabody and Nelson W. Polsby, 2nd ed. (Chicago: Rand McNally, 1969), pp. 325–358. Specific references to Albert's service as whip are made on pp. 335 and 342. Hale Boggs's victory is described in Larry L. King, "The Road to Power in Congress," *Harper's Magazine,* 252 (June, 1971), 39–63, esp. pp. 48 and 62 on the "leadership ladder." See also John F. Bibby and Roger H. Davidson, *On Capitol Hill: Studies in the Legislative Process,* 2nd ed. (Hinsdale, Ill.: The Dryden Press, 1972), pp. 124–144. O'Neill's position as whip was mentioned in all of the news accounts covering his contests for leader and in these accounts, the pattern of ordered succession was often described, see *The New York Times,* November 11, 1972, p. 20, and the editorial, "Potential Speaker," *The New York Times,* November 24, 1972, p. 36. See also, Peabody, *Leadership in Congress,* Chapter 8.

[25] Following the Democratic defeat in the 1946 congressional elections, Sam Rayburn purportedly intended to leave the House leadership to John McCormack. Opposition to McCormack from a number of southern members was strong enough to dissuade Rayburn and he reluctantly accepted the nomination as minority leader. He then appointed McCormack to be whip, *The New York Times,* January 3, 1947, pp. 1 and 4; and January 8, 1947, p. 14.

House.[26] Eight of the ten Republican whips had no House career following their last term in that post, and one other served less than six months after relinquishing it. Leslie Arends of Illinois, who held the post from 1943 through 1974, is the latest to follow in that pattern.

This difference between the parties has been overlooked by some House analysts and has led them to conclude that the Republicans are violating a House "norm" when their whip retains his post regardless of the majority or minority status of the party.[27] Unlike the Democratic whip, the Republican whip is not part of the rank structure.

One curious feature of this party differential is that it appears to contradict some of the assumptions of Joseph Schlesinger's "ambition theory." Republican whips with constituencies almost congruent (and identical since 1965) with the floor leaders should be expected to move up when the floor leadership falls vacant; and the Democratic whips, the appointees of the incumbent leadership, should be expected to return to the ranks of the ordinary members once their patrons had left their positions of influence. This has not occurred. For this reason it is important to maintain the party variable in discussions of "ambition theory" as well as "institutionalization."

The leadership contests of 1976 reaffirmed in part the continued reliance of the Democrats upon the "leadership ladder" to control the succession process and limit internecine warfare among the members. Majority Leader O'Neill was nominated for Speaker without a dissenting vote and Majority Whip John McFall felt obliged to move up as well.[28] McFall's failure to gain the floor leadership is an indication that the Democratic membership's tolerance for automatic elevations had declined. It is important to note, however, that McFall was O'Neill's personal choice to succeed him in the post and had McFall not been so badly tarnished by the "Korean payoff" scandal, then the "ladder" may have made him the fourth consecutive Democratic whip to become majority leader. O'Neill seems to recognize that the heterogeneity of the party's membership would spawn a number of bitter leadership contests if the succession were less tightly controlled. While he may have been unable to help McFall, the Speaker was able to defeat the man he least wanted in the position, Representative Philip Burton, and also to retain the power of appointment over the party whip.

The Republicans, with their more homogeneous composition seem to have no need for a routinized succession system. Leadership changes in that party, particularly in more recent years, appear to have more to do with style than substance.[29]

Leadership Selections

All of the 364 selections since 1789 have been classified. Operationally, leadership continuity in any given Congress occurs when the leader remains in the same post as in the previous Congress. Change, therefore, occurs when a leader vacates his post.

Defined in this manner, change and continuity appear to occur at an identical rate. Of the 364 selections, 177 represented post continuations, while 181 represented post changes. There were six cases which involved the first selections in new posts. Focusing solely upon the 358 selections which involved post continuations or post changes, Table 2 presents the number of instances which occurred in each party by period.

The least surprising aspect of Table 2 is the steady increase in the proportions of post continuations relative to changes over the three periods. One reason for the increase is the stabilization of the leadership posts. A second reason relates to institutionalization; as more leaders choose to remain in the House, they receive more selections, as may be seen in Table 3.

The only period in which the Democratic leadership received more selections and experienced fewer post changes than their opponents was in the first one. This was due largely to the fact that the Democrats (and Democratic-Republicans) organized twenty-six of the period's thirty-seven Congresses and held this control for lengthy spans of time.

---

[26] These terms come from Joseph A. Schlesinger, *Ambition and Politics: Political Careers in the United States* (Chicago: Rand McNally & Co., 1966), p. 90 for "penultimate office," and p. 46 for "career office." There have been only three Republican whips since 1933 – Harry Englebright of California, 1933–1943; Leslie Arends of Illinois, 1943–1975; and Robert Michel of Illinois, 1975–present.

[27] Barbara Hinckley, "Congressional Leadership Selection and Support: A Comparative Analysis," pp. 275–277.

[28] O'Neill was nominated without a contest, but McFall finished fourth in a four-man field and was eliminated on the first ballot, see "House Democrats Elect Leaders, Slow Reforms," *Congressional Quarterly Weekly Report*, 34 (December 11, 1976), pp. 3291–3295. The eventual winner, Jim Wright of Texas, defeated Philip Burton on the third ballot, 148 to 147.

[29] In both the 1959 Martin-Halleck and 1965 Halleck-Ford contests, commentators noted the presence of style considerations and the absence of philosophical ones. See Charles O. Jones, *Party and Policy-Making: The House Republican Policy Committee* (New Brunswick, N.J.: Rutgers University Press, 1964), pp. 37–38; and Robert L. Peabody, *The Ford-Halleck Minority Leadership Contest* (New York: McGraw-Hill, 1966), p. 32.

The House Democrats had more leadership changes than the Republicans did in both the second period (in which they organized only eight of the twenty-four Congresses) and the third period (in which they organized twenty-six of thirty-four). Despite the consistency of the interparty differences in Table 2, the reasons for the higher rates of Democratic change vary. In the second period, the major problem was the inability of the Democrats to organize themselves effectively as waves of new southern congressmen rejoined their ranks. As a result, ten of the sixteen caucus nominations for the Democratic speakership candidate were contested in the years between 1871 and 1899.[30] Only four Republican caucuses in the same period showed dissension.[31] Not until 1899 under the successive leadership stints of James Richardson, John Sharp Williams, and Champ Clark did the Democrats manifest any significant degree of leadership continuity.

Period III's interparty difference indicates more post changes for the Democrats, but with a higher number of selections per leader. The "leadership ladder" accounts for this anomaly

---

[30] Caucus nominations in the Democratic party were contested in 1871, 1873, 1875, 1876, 1877, 1879, 1883, 1891, 1897, and 1899, see various editions of *The New York Times* and *The Washington Post.*

[31] Caucus nominations in the Republican party were contested in 1881, 1883, 1885, and 1889.

Table 2. Post Continuations and Changes by Party and Period

	Continuation		Change		Totals	
Era and Party	N	%	N	%	N	%
1789–1863						
Democratic	20	36.4	35	63.6	55	100.0
Opponents	4	17.4	19	82.6	23	100.0
	24	30.8	54	69.2	78	100.0
χ^2 = 2.7398, Insignificant; *gamma* = +.462						
1863–1911						
Democratic	16	34.8	30	65.2	46	100.0
Republican	31	52.5	28	47.5	59	100.0
	47	44.8	58	55.2	105	100.0
χ^2 = 3.3109, Insignificant; *gamma* = –.350						
1911–1977						
Democratic	53	54.6	44	45.4	97	100.0
Republican	53	67.9	25	32.1	78	100.0
	106	60.6	69	39.4	175	100.0
χ^2 = 3.2021; Insignificant; *gamma* = –.275						
1789–1977						
Democratic	89	44.9	109	55.1	198	100.0
Opponents	88	55.0	72	45.0	160	100.0
	177	49.4	181	50.6	358	100.0
χ^2 = 3.5809; Insignificant; *gamma* = –.199						

Table 3. Party Differences in Mean Selection Frequencies

	Democratic		Non-Democratic			All Leaders	
Periods	Mean	N	Mean	N	Dem.-Opp. Differences	Mean	N
I, 1789–1863	1.87	30	1.15	20	+.72	1.68	47
II, 1863–1911	2.18	22	3.05	20	–.87	2.59	42
III, 1911–1977	4.08	24	4.11	19	–.03	4.09	43
Totals	2.73	74[a]	2.84	57[a]	–.11	2.87	128[a]

[a] Adjusted for period overlapping and party switching.

as Democratic leaders move between posts but not out of the leadership. Republicans in this era appear to be more conscious of post continuity and leave their leaders in place until either the leader's career ends or the caucus arises and removes him from office.

This variety in reasons for leadership change needs more refinement. In the next section, types of leadership change will be examined for each period.

Types of Leadership Change

In the most thorough study of leadership in the contemporary Congress, Robert Peabody identified three forms of change in the House: interparty turnover, intraparty change, and institutional reform.[32] The last appears in the redefinition of leadership posts, such as a change from an appointive to an elective majority leader or the emergence of a new post among the leadership hierarchy. Comparatively speaking, institutional reform has not been an important agent of leadership change. Interparty and intraparty factors have had a far greater impact upon the identity of the House leadership.

Peabody's identification of change agents allows for but does not deal with the disappearance of leaders because of resignation, death, or electoral defeat. So few leaders have resigned from either the leadership or the House itself in recent times that this factor may not seem relevant to analysts of the contemporary Congress. Similarly, recent House leaders, like the vast majority of the members, have become so immune from the vicissitudes of the American electorate that it is difficult to believe that leadership careers have ended because the leader failed to obtain renomination or re-election in his own district. As presently interpreted by students of Congress, the electorate's impact upon the House leaders is indirect. Party failure at the polls presumably angers the House caucus which thereupon garrots its leaders.

In Table 4, the various reasons for changes in House posts are catalogued by period. This table includes Peabody's three types of leadership change and adds another category which I call "career-related changes."

Career-Related Changes. Included in this category are events which affect the leader's own career regardless of what happens to his party at the polls or to his position within it. This category has two subdivisions: personal and political. Personal career-related events which affect change include retirement from the House, death in office, or resignation from a leadership post. Retirement refers to a leader's leaving the House and not directly occupying another political office. A number of the retirees in early periods returned to public life after an absence of a year or more. Retirees in this present era are often too old to do much more than contemplate their past achievements. This age factor is reflected in the sizeable number of deaths among House leaders in this century. For example, between 1789 and 1931, only one of thirty-six Speakers died in office, but since then, five of the last nine Speakers to leave the office died holding the chair.

Comparing the two career-related subdivisions in the present period with the two previous ones indicates that personal factors now outweigh political ones in causing leadership change. Thus, Period III's retirements and deaths easily outnumber leadership departures arising from electoral defeats. Only two of Period III's forty-three leaders were defeated in re-election bids: Champ Clark in 1920 and Carl Bachmann in 1932. Prior to 1911, re-election defeats ended eighteen leadership careers. The party identity of the leader had no bearing on the rates of re-election defeat; all parties suffered equally.

Leaving the House in quest of other political offices was a very common occurrence in the first period, but less so in the later ones. Most often it was the lure of the Senate which took leaders out of the House. Seventeen of the twenty-four leaders who left the House for other elective offices contested for Senate seats. Thirteen were successful. The last one was John Sparkman of Alabama who relinquished the whip post in 1947. Two were elected governor (two were defeated), two were elected Vice President (Colfax in 1868 and Garner in 1932), and one, James A. Garfield, left the House to become president. Two leaders became Secretary of State (Henry Clay and Elihu Washburne). Since 1869, only one leader has left the House directly for an appointive post: Gerald Ford in October, 1973.

Polsby's contention that the House has developed "boundaries" from the other branches and levels of government is supported by the decline in the number and proportion of House leadership changes due to career-related political events.[33] The leadership appears insulated from both their constituents and from

---

[32] Peabody, "Party Leadership Change," *op. cit.*, pp. 676–678.

[33] Polsby, "The Institutionalization of the House," pp. 145–153.

Table 4. Reasons for House Leadership Changes by Period

	1789–1863			1863–1911			1911–1977			1789–1977		
Types of Changes	**Dem.**	**Opp.**	**Tot.**	**Dem.**	**Rep.**	**Tot.**	**Dem.**	**Rep.**	**Tot.**	**Dem.**	**Opp.**	**Tot.**
Career-Related Changes	*51%*	*42%*	*48%*	*40%*	*57%*	*48%*	*36%*	*52%*	*42%*	*42%*	*51%*	*46%*
Personal	*14%*	*0%*	*9%*	*10%*	*14%*	*12%*	*25%*	*32%*	*28%*	*17%*	*17%*	*17%*
Retired from House	14	0	9	3	7	5	7	16	10	8	8	8
Died in office	0	0	0	3	7	5	18	8	15	8	6	7
Resigned post	0	0	0	3	0	2	0	8	3	1	3	2
Political	*37%*	*42%*	*39%*	*30%*	*43%*	*36%*	*11%*	*20%*	*14%*	*25%*	*35%*	*29%*
Not re-elected	11	16	13	20	18	19	2	4	3	10	12	11
Elected: other office	11	11	11	10	18	14	7	4	6	9	11	10
Appointed: other office	11	11	11	0	4	2	0	4	1	4	6	4
Defeated: other office	3	5	4	0	4	2	2	8	4	2	6	3
Inter-Party Turnover	*17%*	*47%*	*28%*	*27%*	*29%*	*28%*	*32%*	*24%*	*29%*	*26%*	*32%*	*28%*
Loses elective post	6	32	15	0	0	0	2	8	4	3	11	6
Loses appointive post	11	16	13	17	11	14	5	0	3	10	8	9
Keeps rank; new post	0	0	0	10	18	14	25	16	22	13	21	13
Intra-Party Change	*31%*	*11%*	*24%*	*30%*	*11%*	*21%*	*30%*	*24%*	*28%*	*30%*	*15%*	*24%*
Changes post and rank	6	0	4	3	11	7	23	4	16	12	6	9
Defeated on floor	6	0	4	0	0	0	0	0	0	2	0	1
Defeated in caucus	0	0	0	3	0	2	0	16	6	1	6	3
Replaced as elected leader	3	5	4	10	0	5	0	4	1	4	3	3
Replaced as appointed leader	17	5	13	13	0	7	7	0	4	12	1	8
Institutional Reform	*0%*	*0%*	*0%*	*3%*	*4%*	*3%*	*2%*	*0%*	*1%*	*2%*	*1%*	*2%*
Post eliminated	0	0	0	3	4	3	2	0	1	2	1	2
TOTALS												
Percentages	99%	100%	100%	100%	101%	100%	100%	100%	100%	100%	99%	100%
N of cases	35	19	54	30	28	58	44	25	69	109	72	181

Note: Italicized percentages represent category subtotals.

whatever "progressive ambitions" they may have once had.

Interparty Turnover. This has been a constant factor in House leadership change. The mean deviation from the 29 per cent recorded for all three eras is a meager one per cent. However, the number of leaders who lose their party ranks during changes in party control of the House has dwindled greatly, from 28 per cent in the 1789–1863 era to 7 per cent in the post-1911 one. There are two major reasons for this change. First, the inability of the opponents of the Democrats to maintain control of the House during Period I insured a high turnover of their elective and appointive leaders as a result of election defeats. The second reason involves the development of a coherent minority following 1863. Leaders whose parties had lost control of the House now had a place to relocate their influence. In the present era, only two elected floor leaders (Kitchin in 1919 and Halleck in 1949 and 1955) lost their posts when loss of House control resulted in a constriction of the number of leadership positions available to their parties. The two appointed leaders in this era to lose their posts because of interparty turnover were Sereno Payne in 1911 and J. Percy Priest in 1953. Priest was removed from the whip post by Minority Leader Rayburn to make room for ex-Majority Leader McCormack. This move preserved McCormack's second-place ranking. One advantage of having appointed whips is that they cannot loudly protest such moves.

Institutional Reform. Concentrating the majority leadership in Ways and Means, Chairman Sereno Payne in 1899 eliminated Joe Cannon and the Appropriations Committee from the leadership for a short time. The other two instances involved the Democratic whip post which disappeared as a leadership position in 1909 and 1913. Since both disappearances of this post were accompanied by changes in the elective leadership of the Democratic party, it would not be unreasonable to consider these to be intraparty changes.

Intraparty Changes. Of the various types of leadership change, none is more fascinating than intraparty change when a leader remains in the House and is moved within or removed from the hierarchy by the members of his own party. Often filled with intrigue and interpersonal machinations, these intraparty changes provide insight into the factions which exist within legislative parties. Changes within this context have shaped much of the writing about the House.

This category has five different manifestations: (1) interpositional mobility when a leader holding one post moves to another within the hierarchy; (2) defeat on the floor for the speakership when disaffected members of the leader's own party join with the opposition to deny a re-election; (3) a defeat in the caucus for any of the party's elective leaders; (4) replacement of an elective leader as a nominee without a formal denial of support; and (5) demotion to the ranks of the ordinary members of an appointed leader by an elected one. To these might be added the elimination by one leader of a post held by another, such as happened twice to Democratic whips.

Of the forty-three interparty changes in these top posts, seventeen were interpositional moves. The six pre-1911 changes included the moving of three Democratic appointed leaders into the speakership and of three Republican appointed leaders into other appointive posts.

The eleven interpositional cases in the present era involved one move by a Republican and ten by Democrats. The lone Republican to move was Majority Leader Nicholas Longworth who became Speaker in 1925. His succession was bitterly contested in the caucus, but he won on the first ballot with 62 per cent of the votes.[34] John Rhodes's uncontested elevation to the minority leadership in 1973 from the chairmanship of the Republican Policy Committee may portend a new development in the Republican patterns of leadership succession.[35] From the historical standpoint of this study, however, the chairmanships of the Republican Policy Committee and Conference as well as their counterparts in the Democratic Caucus and Steering Committees have not been clearly identified as leadership posts for a sufficient length of time to warrant their inclusion here.[36] At some future time they may well qualify for this category.

Interpositional mobility in the highest posts generally meets with less opposition in Democratic caucuses. Only four of these moves were greeted with caucus opposition. Henry Rainey captured 60 per cent of the vote in his successful move from the floor leadership to the chair in 1933,[37] and in 1971 Carl Albert received 92 per cent of the vote in his identical move.[38] The most serious challenge to an interpositional move was that faced by Hale Boggs in his successful contest to move from whip to floor leader in 1971. He won 57 per cent of the votes on the second ballot.[39] Although challenges were hurled at Byrns's 1934 succession to the speakership,[40] and Albert's and O'Neill's successions to the floor leadership in 1962 and 1973 respectively, they were withdrawn before the caucus vote. Four Democratic interpositional moves between the floor leadership and the chair were uncontested and unchallenged: Bankhead in 1936, Rayburn in 1940, McCormack in 1962, and O'Neill in 1976. The "leadership ladder" as it pertains to the floor leader-Speaker rung has enormous legitimacy in the Democratic caucus.

---

[34] *The New York Times,* February 28, 1925, p. 1. Longworth defeated Martin Madden of Illinois, 140 to 85 on the first ballot. Madden had been one of the two Republican candidates challenging Speaker Gillet's re-election on the floor during the previous Congress.

[35] *Congressional Quarterly Weekly Report,* 31 (December 5, 1973), p. 3171.

[36] Ripley asserts that "Chairmen of various party committees (for example, Committee on Committees, Steering Committee, Policy Committee) or of the party caucus or conference have *occasionally* been prominent figures in the majority party in the House," in *Majority Party Leadership in Congress,* pp. 3–4 (emphasis added). Jones states that "Although they have existed on occasion in both parties, until the 1960s policy or steering committees were not heavily relied on by the minority party," in *The Minority Party in Congress,* p. 38.

[37] Rainey defeated John McDuffie of Alabama who had served as whip under Garner. The vote was decided on the first ballot: Rainey's 166 to McDuffie's 112, *The New York Times,* March 3, 1933, p. 1. A fuller account of this contest may be found in Robert A. Waller, "The Selection of Henry T. Rainey as Speaker of the House," *Capitol Studies,* 2 (Spring, 1973), pp. 37–47.

[38] Albert defeated John Conyers of Michigan 220–20, *Congressional Quarterly Weekly Report,* 29 (January 22, 1971), p. 176.

[39] The final tally on the second ballot was Boggs, 140; Udall, 88; and Sisk, 17; in *Congressional Quarterly Weekly Report,* 29 (January 22, 1971), p. 176.

[40] The most serious threat to Byrns came from Sam Rayburn, but Rayburn withdrew after Pennsylvania announced for Byrns, C. Dwight Dorough, *Mr. Sam* (New York: Random House, 1962), p. 253. See also *The New York Times,* December 13, 1934, p. 17.

John McFall's fourth place showing in the 1976 floor leadership contest indicates that the whip-floor leader rung has less legitimacy. McFall's Korean troubles aside, a counter-trend has emerged among a segment of the Democratic membership which opposes the ladder concept. If caucus opposition to the "ladder" continues to grow, it is quite possible that interpositional mobility may decline as a major form of intraparty change.

Voted Floor Defeats. The most dramatic cases of intraparty leadership change occur when the sitting Speaker of the incumbent party is defeated in an open floor fight. Both cases occurred in the first period.[41] Since 1835 only Speaker Frederick Gillett faced defeat on the floor from fellow Republicans. It took nine ballots in 1923 to overcome this challenge.[42]

Intraparty floor contests for the speakership disappeared with the institutionalization of party caucuses for both majorities and minorities following the Civil War.[43] Conflict was moved from the floor into the confines of the caucus room. Here conflict could be contained and a party which had arrived in Washington with a majority of the seats would not have to face the prospect of seeing its victory snatched away by renegades on the floor.

Voted Caucus Defeats. Newspaper accounts of party caucuses from 1863 to 1977 revealed that the sixty Republican caucuses held seventy-seven elections for these major posts, of which sixteen (21 per cent) were contested.[44] The Democrats in the same period held sixty-one caucuses with ninety-one elections. Twenty were contested (22 per cent).

Only one Democratic floor leader was defeated in the caucus: Samuel Randall, the minority nominee in the 47th Congress, was defeated on the first ballot by John Carlisle in his bid to become Speaker of the 48th Congress in 1883.[45] As a conciliatory gesture, Carlisle appointed Randall to chair the Appropriations Committee. Ironically, it was this Congress that Woodrow Wilson used to illustrate his observation that "all [House chairmen] are subordinate to the Chairman on Appropriations."[46] So even the lone case of direct removal of a Democratic leader by the caucus had little discernible impact upon the party's power relationships.

Since then the only challenges to incumbent Democratic floor leaders occurred in 1969 and 1973. Speaker McCormack beat back Morris Udall's frontal assault 178 to 58 in the 91st Congress[47] and Speaker Albert thwarted a challenge from John Conyers and the Black Caucus in the 93rd.[48]

Leadership challenges in the Republican caucuses are more successful. In 1919 Minority Leader Mann's bid for the speakership was stopped by Frederick Gillett who received twice as many votes from his fellow partisans (138–69).[49] Twelve years later following another change in House party control, John Q. Tilson, who had been elected three times to be the party's majority leader, had his floor leadership endorsement revoked by the caucus when the party organized itself as a minority rather than as the majority which it had anticipated earlier. Eight ballots transpired before Bertrand Snell of New York was victorious.[50]

---

[41] Both cases involved Philip P. Barbour of Virginia. He defeated Speaker John W. Taylor in 1821 and was later defeated for re-election as Speaker by Henry Clay two years later. See *Niles' Weekly Register,* 21 (December 8, 1821), p. 234 and 25 (December 6, 1823), p. 22, respectively. One unusual case which does not technically qualify for this category occurred in 1834 when John Bell, a Whig, was elected to preside over the 2nd Session of the Democratically-controlled 23rd Congress when the Democrats could not agree on a candidate. In the next Congress, the united Democrats elected James K. Polk over Bell. See *Niles' Weekly Register,* 46 (June 7, 1834), pp. 248 and 49 *Niles' Weekly Register* (December 12, 1835), p. 248 for the two contests.

[42] This contest has been described earlier, see note 15, *supra.* See also, Paul D. Hasbrouck, *Party Government in the House of Representatives* (New York: Macmillan, 1927), p. 19 and *The New York Times,* December 6, 1923, pp. 1–2.

[43] This may be seen in the decline of multiballot floor contests and the fact that the mean percentage of votes received by the top two speakership candidates since 1865 in fifty-eight regular and two special speakership elections has been 98.86 per cent. The lowest percentage was 93.4 in the 55th Congress (1899). The first ballot percentage in 1923 was 94.4 for Gillett and Garrett combined.

[44] This information is derived from the accounts of two newspapers, *The New York Times* and *The Washington Post.* The first contest reported by the *Post* occurred in 1879. A number of caucus nominations were simply reported with no mention of the number of members present or the votes cast. When this happened, I recorded it as "no reported contest" and assumed that the nomination had met with no opposition.

[45] Randall was defeated by Carlisle, 106–52. Samuel Cox received 30 votes, *The New York Times,* December 3, 1883, p. 1.

[46] Woodrow Wilson, *Congressional Government* (New York: Meridian Books, 1956), pp. 82–83. This book was originally published in Boston by Houghton, Mifflin and Co., in 1885.

[47] *Congressional Quarterly Weekly Report,* 27 (January 3, 1969), p. 2. Wilbur Mills received four votes.

[48] Albert defeated Conyers by 202–25 in the 93rd Congress, *Congressional Quarterly Weekly Report,* 31 (January 6, 1973), p. 5. Conyers's opposition to Albert in 1971 was due to the Speaker's lack of support for an effort to strip three Mississippi members of their seniority. In 1973 he tried for support beyond the Black Congressional Caucus.

[49] *The New York Times,* February 28, 1919, p. 1.

The two best-known recent cases of leadership change in the Republican caucus were the defeats of Joe Martin by Charles Halleck in 1959 and of Halleck by Gerald Ford in 1965.[51] Both contests were close, but both were decided early: two ballots in 1959 and one in 1965.

Nonvoted Removals. Similar to defeat in the caucus is removal as the nominee in the subsequent Congress. The leader is back in the House, but his party has nominated someone else to lead it without a formal vote on the floor or in a party caucus. A case in point is the replacement of Speaker Nathaniel Macon by Joseph Varnum, a fellow Democratic-Republican, in 1807. Macon had fallen out with President Jefferson over Macon's continuance of John Randolph as Chairman of the Ways and Means Committee during his three terms as Speaker. Students of the period suggest that Macon stayed away from the balloting on the opening day of the Tenth Congress to avoid an outright rebuff on the floor.[52] A less clear example of this bypassing occurred in 1791 when Speaker Frederick Muhlenberg was not in contention to succeed himself.[53] However he was elected in the subsequent Congress to preside over the first Democratic-Republican majority. The other three cases involve Democratic minority nominees who were not renominated by the party caucus in the following Congress (Marshall in 1869, Kerr in 1871, and Wood in 1875). Wood wanted to be renominated. but he withdrew when he saw that his strength in the caucus was slim.[54] Kerr handled credentials contests in the 1871 caucus, but his name was never offered for a formal vote.[55] Marshall's leadership career disappeared without a trace.

The lone post-1911 case of an elected leader being replaced without a vote involved a Republican leader, Harold Knutson. He had been elected whip in 1919 by the caucus and in 1921 by the Committee on Committees, but he had broken with the leadership and voted for the insurgent speakership candidate in the 1923 floor fight.[56] Needless to say, he was replaced as an elected leader. Including this nonvoted defeat with the voted ones of Mann, Tilson, Martin, and Halleck brings to five the number of "removals from below" of leaders by members of the Republican caucus in the 1911–1977 era. No such removals occurred in the Democratic caucus.

Demotions

Demotion of appointed leaders represents "removal from above." In these cases, the top-ranking party leader simply replaces an appointive leader. There generally is little fanfare surrounding these post departures because the power to hire (i.e., appoint) carries with it the inherent power to fire. The first demotion occurred in 1800 when Federalist Speaker Theodore Sedgwick removed Robert Goodlow Harper from the Ways and Means chairmanship. Harper had been initially appointed by Speaker Dayton and had been continued by Sedgwick, but before the end of the session, Sedgwick concluded that he was a man "whom no one can control," and removed him.[57] This is the only case of a non-Democratic leader being demoted.

---

[50] Snell's nomination was made unanimous on the eighth ballot. His tally over Tilson on the seventh ballot was 96 to 64, *The New York Times,* December 1, 1931, pp. 1 and 4.

[51] Accounts of the Halleck-Martin contest may be found in Martin's own book, *My First Fifty Years in Politics* (New York: McGraw-Hill, 1960), pp. 12–19; and in Charles O. Jones, *Party and Policy-Making,* pp. 33–42. The Ford-Halleck struggle is covered by Robert L. Peabody in *The Ford-Halleck Minority Leadership Contest,* and in "Political Parties: House Republican Leadership," in *American Political Institutions and Public Policy: Five Contemporary Studies,* ed. Allan P. Sindler (Boston: Little Brown & Co., 1969), pp. 181–229.

[52] Noble E. Cunningham, Jr., *The Jeffersonian Republicans in Power: Party Operations, 1801–1809* (Chapel Hill: University of North Carolina Press, 1963), p. 88. Macon was a candidate for Speaker in the 11th Congress and lost to Varnum on the second ballot, 45 to 65, *Annals of Congress,* 11th Congress, First Session, p. 56. Macon's relationship with Randolph is described by Cunningham on pp. 77, 86–88, and 230.

[53] Muhlenberg was apparently not a contestant in the Second Congress. According to a letter from Elbridge Gerry to his wife, the two leading candidates were Jonathan Trumbull of Connecticut who had lost to Muhlenberg in the previous Congress and John Laurence of New York. The letter was dated October 24, 1791 and may be found in the Russell W. Knight Collection of Elbridge Gerry Papers in the Massachusetts Historical Society. Special thanks are extended to Professor Patrick J. Furlong of the Department of History of Indiana University at South Bend for this reference.

[54] Wood withdrew a week before the balloting amidst speculation that he was trying to arrange an appointment to the chairmanship of Ways and Means, which was not forthcoming, *The New York Times,* December 1, 1875, p. 1. A fuller account of the 1875 election may be found in Albert V. House, "The Speakership Contest of 1875: Democratic Response to Power," *Journal of American History,* 52 (September, 1965), pp. 252–274.

[55] *The New York Times,* March 3, 1871, p. 1.

[56] Knutson was described as a "Cannon pupil" at the time of the 1919 whip contest, *The New York Times,* March 12, 1919, p. 1. Four years later he voted for the leading insurgent candidate in the caucus, Henry A. Cooper of Wisconsin, *The New York Times,* December 2, 1923, p. 1. The Republican Committee on Committees gave the post to Albert Vestal of Indiana, see *The Washington Post,* December 13, 1923, p. 5.

In the 1789–1863 era, Democratic Speakers demoted six chairmen of Ways and Means.[58] In the next era, 1863–1911, four Democratic appointed leaders were removed by elected ones.[59]

The pattern of Democratic demotion has continued into this present period. Speaker Rainey demoted John McDuffie from the whip post after the latter had failed in his bid to defeat Rainey for the speakership nomination in 1933.[60] Two years later, Speaker Byrns removed Rainey's whip, Arthur Greenwood, and replaced him with Patrick Boland of Pennsylvania. Boland held his office during two speakership changes, Bankhead's succession in 1936 and Rayburn's in 1940. He was the last Democratic whip to remain in his post when the elected leaders above him changed.

If one includes the cases of the two Democratic whips whose posts disappeared in 1909 and 1913, and of J. Percy Priest who was not reappointed whip by Speaker Rayburn when the Democrats regained control of the House in 1955,[61] as well as O'Neill's non-reappointment of John McFall, then there is a total of sixteen demotions of appointed Democratic leaders by the party's elected leaders.

Clearly, this is a power which Republican leaders do not possess. The independently elected Republican whips may lack opportunities to gain the top slot, but they have been protected from those "above" them. A ready example of this appeared in 1965 when Gerald Ford, then a newly elected minority leader, backed Peter Frelinghuysen as his candidate for whip against Leslie Arends, who had held the post for twenty-two years. Frelinghuysen was soundly defeated in the caucus, and Minority Leader Ford quickly became aware of the limits of power in the Republican leadership.[62]

Even when the elected Republican leaders had appointive posts under their control (1859–1917) they were reluctant to exercise their authority. None of the eighteen appointed Republican floor leaders was removed. This reluctance extended to other appointed officials as well. In the years from 1881 through 1911, the Republican speakers "violated" seniority in chairmanship selections much less often than the Democratic speakers did (43 per cent to 60 per cent).[63] Greater unity and organization combined to minimize the number of rolling heads following each change in the elected officers of the Republican party.

Summary. The patterns of leadership change and continuity reveal little variation over time in the proportions of House leaders leaving their posts under the three broad categories of: career-related departures, interparty turnovers, and intraparty changes. Within these categories, however, there are important trends toward institutionalization as personal career-related departures have increased and politically related departures have decreased. The House leadership career is now an entity unto itself and has declined as a springboard to other offices. In addition, House leaders have more to fear from illness and old age than they do from their constituents. The ratio of electoral defeats to deaths in the pre-1911 periods was 6:1, but the ratio has been reversed since then with a 1:5 relationship between defeats and deaths.

Post continuations in the years since 1911 outnumber post changes for both parties, another indication of institutionalization. Also the development of the minority leadership and whip positions has made it possible for leaders to retain their party ranks regardless of their party's success (or lack of it) at the polls. This has led to greater professionalization within these posts.

---

[57] Richard E. Welch, Jr., *Theodore Sedgwick, Federalist: A Political Portrait* (Middletown, Connecticut: Wesleyan University Press, 1965), p. 211n.

[58] John Randolph in 1807, Ezekiel Bacon in 1812, William Lowndes in 1819, George McDuffie in 1831, Thomas Bayly in 1851, and J. Glancy Jones in 1858. Both Bacon and Jones were removed between sessions of a Congress. Serious questions existed about their fitness for floor leadership, Alexander, *History and Procedure of the House of Representatives,* pp. 124–125.

[59] Two chairmen of Ways and Means, William R. Morrison in 1877 and William Springer in 1893; one chairman of Appropriations, William S. Holman in 1893; and one whip, Oscar Underwood in 1901 were removed from their appointive posts.

[60] Waller, "The Selection of Henry T. Rainey," p. 45.

[61] Ripley reports that Priest "decided not to continue as whip in 1955 because he had become Chairman of the Committee on Interstate and Foreign Commerce," in "Party Whip Organizations" in Peabody and Polsby, 2nd ed., p. 208n.

[62] See Arthur Krock's article, "Rep. Ford's Defeat," in *The New York Times,* January 17, 1965, IV, 13. Arends defeated Frelinghuysen 70–59. *The New York Times,* January 15, 1965, p. 1.

[63] These figures are based upon a recomputation of Table 10, "Violations of Seniority by Speaker, 1881–1910," in Polsby, Gallaher, and Rundquist, p. 799.

As important as the time-related institutionalizing trends may be, there appear to be more important interparty differences in the patterns of leadership change and continuity. Three of these relate directly to intraparty change. Democratic leaders change posts more frequently and do so with little opposition from the caucus. Republican leaders change posts infrequently and the only interpositional move by a Republican in this century was met with caucus opposition. Democratic leaders face infrequent and impotent opposition in their caucuses, while Republicans face similarly infrequent, but highly potent opposition in theirs. And finally, because Democratic leaders retain the power to appoint leaders under them, they can also remove them with relative impunity. Elected Republican leaders did not remove appointive leaders when they possessed the authority to do so and now they possess neither the power of appointment nor the power of removal. They must content themselves with whomever the caucus chooses to assist them in the leadership.

The House parties do differ in their patterns of intraparty leadership change. Combining their separate patterns to generate a picture of House "norms" in the furtherance of the institutionalization thesis may lead to valuable conclusions but it may also lead one to overlook party differences.

Establishing these important party differences indicates that institutionalization theories should maintain the party variable in future explorations of leadership change, but it does not bear on the important contemporary statement regarding the impact of majority or minority statuses upon leadership change. In the next section of this paper, these variables will be directly examined.

The Impact of Majority and Minority Statuses upon Leadership Contests

In focusing wholly upon majority vs. minority statuses, it becomes necessary to confine the analysis to the later two periods. No discernible minority exists before 1863, and since thirteen of the forty reported speakership contests went beyond a single ballot, it is also hard to discern a majority. In many of the speakership contests in that era the major opponent to the eventual victor was a fellow member of his own party. Consequently, the terms "majority" and "minority" do not have as much meaning as they do in the two most recent ones.

The literature on leadership change suggests that there should be greater turnover among minority leaders because the frustration of losing often takes the form of "scapegoating" the leadership.[64] Regardless of the leaders' inability to affect the outcome of congressional elections throughout the nation, it is presumed in the literature that the leaders must face the consequences of an electorate dissatisfied with the party's House candidates. This contention is examined in Table 5 for Periods II and III.

Note that the mean frequencies in Table 5 are generally lower by period than those in Table 3, which dealt with frequencies by party. The reason is that majority or minority leadership is not the mutually exclusive phenemonon which party leadership has been since 1827. Eighteen of the eighty-two post-1863 leaders (22 per cent) served their parties under both conditions.

The general expectation that leadership selection frequencies would be lower for minorities than for majorities is borne out, but the sizes of the differences are similar to those presented in the interparty comparisons. The lack of mutual exclusivity is one reason, but not the sole reason for this similarity.

Table 5. Majority-Minority Differences in Mean Selection Frequencies

Periods	Majority Mean	Majority N	Minority Mean	Minority N	Maj.-Min. Difference	All Leaders Mean	All Leaders N
II, 1863–1911	2.44	32	1.63	19	+.81	2.59	42
III, 1911–1977	3.45	31	3.29	21	+.16	4.09	43
Totals	2.98	62[a]	2.56	39[a]	+.42	3.48	82[a]

[a] Adjusted for overlapping periods.

---

[64] Peabody, "Party Leadership Change," p. 688. Joe Martin certainly felt that the size of the 1958 defeat had cost him the minority leadership, see his book, *My First Fifty Years in Politics*, pp. 4–5.

The greatest difference in this table appears among the leaders during Period II. A closer examination of this difference reveals an important party factor. None of the ten Democratic minority nominees from 1863 through 1901 was renominated. This contrasts directly with the nine minority nominations made by the Republican party in this same span. The Republican minority caucuses nominated Reed four times and Garfield three times. The other two Republican nominees, Blaine and Keifer, left the House after their single selections in the minority. This strengthens the assertion that the Republican party had an effective House organization before the post-Civil War Democrats did. Even when they were in the minority, the Republicans had a clearer sense of which of their members they wished to nominate as Speaker. Only two of their nine minority nominations for Speaker in Period II were contested in the caucus.[65] Once again a party contrast emerges. The Democrats had caucus contests for four of their seven minority nominations from 1871 through 1901, one of which took six ballots to decide.[66]

Focusing only upon the elective leaders reveals a sizeable difference in Period II's majorities and minorities (2.50 – 1.53 = +.97), but an inconsequential one between Period III's elective leaders (3.68 – 3.86 = –.18). The lack of any meaningful difference in this category represents a serious challenge to much of the contemporary wisdom concerning the impact of party status upon leadership change. It is possible, however, that the use of averages to examine this proposition may be insufficient.

A more direct test of the minority frustration hypothesis would be to examine only the leadership selections voted on by the membership and to see if caucus dissension appears more often among minorities than among majorities. The first step in this test involves eliminating ninety-five appointive and twenty-three semi-elective selections from the total of 285 recorded in the two later periods. This leaves 167 leadership selections made by the party membership. To this may be added the second majority leadership election in the 1919 Republican caucus.[67] Ninety-one of these elections involved Democrats, and seventy-seven involved Republicans; 100 occurred when a party was in the majority and sixty-eight when the party was not. This set of data seems sufficiently large to test the impact of party status upon the House leadership.

The dependent variable to be used in this analysis will be the frequency of caucus contests as reported in contemporaneous newspaper accounts. The newspapers report contests in thirty-six of the 168 elections (21 per cent). Often the news account the following day makes no mention of either a contest or a nomination by acclamation. Since every contest referred to in the House literature may be found in the news accounts plus a number which have eluded previous analysis, I am willing to assume that the absence of a reported contest means that none occurred. The pre-caucus barrage of news stories surrounding genuine contests makes it seem unlikely that an account would overlook any manifestation of conflict.

In three cases, selections were made without caucuses. Two occurred at the opening of the 40th Congress, which convened a day after the 39th adjourned. Each party rallied around a candidate, and the two men received every vote cast in the House for speaker between them.[68] The other case occurred in 1863 when the Democrats held a caucus, but made no nominations because of their internal divisions over the Civil War. Samuel Cox, the leading Democrat in the speakership balloting on the floor, received forty-two votes from the seventy-five Democrats who had been elected to that Congress.[69] This degree of obvious dissension within the party justifies adding this event to the caucus contests, bringing the total to thirty-seven.

The 168 intraparty elections are compared in Table 6 by party and era for the relative frequency of contests within differing party statuses. Correlations are also presented to determine if the generally accepted proposition concerning the greater frequency of leadership contests in the minority is borne out.

In every case but one in Table 6, the data disconfirm the proposition that minority status engenders leadership contests. In three of the four period and party comparisons, contests are more prevalent in the majority party. The only case which supports the proposition – i.e., the

---

[65] The two Republican minority contests occurred in 1883 and 1885.

[66] The four Democratic minority contests occurred in 1871, 1873, 1897, and 1899. The last contest took six ballots before James D. Richardson of Tennessee defeated David DeArmond of Missouri, 90 to 47, *The New York Times*, December 3, 1899, p. 2.

[67] James R. Mann was elected majority leader by the caucus after his defeat for the speakership nomination. He refused the post and Frank Mondell of Wyoming was elected. *The New York Times*, March 12, 1919, p. 1.

[68] *The New York Times*, March 5, 1867, p. 1.

[69] *House Journal*, 38th Congress, First Session, p. 11.

Table 6. Frequency of Intraparty Contests for House Elective Posts: By Period and Party Status, 1863–1977

	1863–1911		1911–1977		1863–1977	
Party and Status	**Contests/ Caucuses**	**%**	**Contests/ Caucuses**	**%**	**Contests/ Caucuses**	**%**
Democratic Majority	6/9	67	10/57	18	16/66	24
Democratic Minority	5/17	29	0/8	0	5/25	20
Democratic Totals	11/26	42	10/65	15	21/91	23
Gamma	–.655[a]		–1.000		–.123	
Republican Majority	3/16	19	6/18	33	9/34	26
Republican Minority	2/9	22	5/34	15	7/43	16
Republican Totals	5/25	20	11/52	21	16/77	21
Gamma	+.106		–.487		–.299	
All Majorities	9/25	36	16/75	21	25/100	25
All Minorities	7/26	27	5/42	12	12/68	18
Totals	16/51	31	21/117	18	37/168	22
Gamma	–.208		–.335		–.217	

Note: In none of the comparisons does statistical significance emerge at the .05 level.

[a] The direction of the signs tests the proposition that contests will be less frequent in majority status situations.

case of the Republicans from 1863 to 1911 – represents the smallest percentage differential in the table. And when all majorities are compared to all minorities, regardless of party, the incidence of majority contests is higher in each period.

One explanation for this finding is that the stakes are higher in the majority party than in the minority. As Peabody describes the difference:

> There are more committee assignments and appointments to prestige boards and commissions to be distributed. It is the majority which receives most of the credit when legislation is passed. Their projects receive higher priority. Majority members chair the committees and subcommittees. With position comes staff, superior access to executive officialdom and greater influence on legislative outcomes.[70]

Also in Table 6, an intriguing interparty difference appears with regard to the incidence of these contests over time. Republican caucuses had a slight increase in dissension in Period III, while the Democratic caucuses have shown a marked decrease between the two eras. This decline in contested Democratic leadership elections is most likely due to the conscious efforts of the leadership itself to minimize conflict and to institutionalize the party's succession system. It appears as if the initial selections for the Democratic leadership ladder are designed to appeal to the widest possible group of members.[71]

There is support for this inference in the congressional voting literature. David Truman's study of the 81st Congress (1949–1951) indicated that the floor leaders in both House parties tended to be ideological "middlemen."[72] But Barbara Hinckley's analysis of more recent Congresses reveals that only the Democratic leaders continue to occupy this location.[73] In these Congresses, the Democratic leadership stood astride the gulf between a liberal-moderate membership and a brace of conservative committee chairmen, while the Republican leadership was more conservative than its membership and ranking committeemen. Whether this was true in the pre-World

---

[70] Peabody, "Party Leadership Change," p. 687.

[71] A case in point is that of "Tip" O'Neill whose widespread popularity made his move from appointed whip to elected floor leader without opposition. Peabody reports that Congressman Sam Gibbons of Florida, who had hoped to challenge O'Neill, told him, " 'Tip, I can tell you something that nobody else in this room can. You haven't got an enemy in the place.' " *Leadership in Congress*, p. 258.

[72] David B. Truman, *The Congressional Party: A Case Study* (New York: John Wiley & Sons, 1959), pp. 205–208. A recent study suggests that the "middleman" role is adopted following the selection as leader rather than being a reason for the selection itself. See William E. Sullivan, "Criteria for Selecting Party Leadership in Congress: An Empirical Test," *American Politics Quarterly*, 3 (January, 1975), 25–44.

[73] Hinckley, "Congressional Leadership Selection and Support," pp. 281–284.

War II Congresses is an important question beyond the scope of this study.

Leadership Change Typology. In order to add the dimension of incumbency to the study, the data have been placed into Robert Peabody's typology of "intraparty leadership change."[74] Peabody's typology identifies six possible change situations: (1) status quo; (2) routine advancement; (3) appointment or emergence of a consensus choice; (4) open competition; (5) challenge to an heir apparent; and (6) revolt or its aftermath. Two of Peabody's situations – (2) and (5) – depend upon the existence of established succession patterns for their meaning. Since the Democrats appear to have an orderly succession system and the Republicans do not, these two situations lack interparty comparability and will not be included in this analysis.

Using Peabody's other two criteria – vacancy or no vacancy in the leadership post, and a contest or no contest in the caucus – a simple four-fold typology can be created from Peabody's original one. This typology is used in Table 7 to classify the 168 floor leadership elections in the House since 1863.

Operationally, *status quo* refers to the unopposed continuance of a leader in his party leadership rank, regardless of party control

Table 7. Frequencies of Intraparty Leadership Situations by Period, Party Status, and Post Occupancy, 1863–1977

	Occupied Post		Vacant Post		Totals	
	Status Quo	Revolt	Consensus	Competition	N	%
1863–1911						
Democratic majority	33%	44%	0%	22%	9	99
Democratic minority	41	0	20	29	17	100
Democratic totals	38	15	19	27	26	99
Republican majority	56	12	25	6	16	99
Republican minority	67	11	11	11	9	100
Republican totals	60	12	20	8	25	100
Both majority	48	24	16	12	25	100
Both minority	50	4	23	23	26	100
Both totals	49	14	20	18	51	101
1911–1977						
Democratic majority	61%	4%	21%	14%	57	100
Democratic minority	62	0	38	0	8	100
Democratic totals	62	3	23	12	65	100
Republican majority	44	17	22	17	18	100
Republican minority	76	9	9	6	34	100
Republican totals	65	12	13	10	52	100
Both majority	57	7	21	15	75	100
Both minority	74	7	14	5	42	100
Both totals	63	7	19	11	117	100
1863–1977						
Democratic majority	58%	9%	18%	15%	66	100
Democratic minority	48	0	32	20	25	100
Democratic totals	55	7	22	16	91	100
Republican majority	50	15	24	12	34	101
Republican minority	74	9	9	7	43	99
Republican totals	64	12	16	9	77	101
All majority	55	11	20	14	100	100
All minority	65	6	18	12	68	101
All totals	59	9	19	13	168	100

---

[74] Peabody, "Party Leadership Change," pp. 681–686. The fullest presentation of this typology may be found in *Leadership in Congress,* pp. 266–294.

changes. A Speaker becoming minority leader after his party has lost the House represents a continuation of his party's leadership status quo. *Consensus* choices represent new moves into ranks which are unopposed by the caucus. Also included in this category are unopposed moves by appointed Democratic whips into the elected majority leadership post following changes in party control (e.g., McCormack in 1949 and 1955). The rank remains the same, but dependence upon caucus approval has increased.

Contests occur in two cases: *competition* when there is a vacancy in the leadership rankings, and *revolt* when there is no vacancy and when opposition to the incumbent leader manifests itself in actual caucus votes against him.

The three instances in Period II in which Democratic minority speakership candidates remained in the House but were replaced as nominees without voted defeats have been classified on the basis of each leader's caucus activity. Since neither Marshall in the 41st Congress nor Kerr in the 42nd Congress figured in the leadership contests, these cases were treated as vacancies. In the 44th Congress (1875–1877), however, Fernando Wood, the previous speakership nominee, contended until his support evaporated in the final week before the vote, that election is considered a revolt.

Focusing first upon the 1863–1911 data in Table 7, we see that contests occurred at a higher rate in Democratic caucuses regardless of the status of the party or the incumbency of its leaders. Democratic disarray was very pronounced throughout most of this era, particularly during the party's majorities from 1875 through 1885. All four majorities in that period opened with leadership revolts. Two were successful – the replacement of Wood in 1875 and the voted defeat of Randall in 1883; two were unsuccessful – Randall's two first-ballot victories in 1877 and 1879.[75] This contrasts with the failures of the three Republican revolts against Keifer in 1883, Reed in 1889, and Cannon in 1909.[76]

In both parties, contests for occupied offices were more frequent in majority situations. Six of the seven revolts occurred during majorities. This lends further support to the contention that when the stakes are higher competition will be intensified. Such a finding is not unexpected in an era when the Speaker of the House had sole control over the naming of sixty-plus committee chairmen.[77] Many House careers prior to 1911 were affected by members' decisions about which speakership candidate to back.

Regarding minorities, Republican ones were slightly less competitive than Democratic ones (22 per cent to 29 per cent). As mentioned earlier, however, stability in the Democratic minority was an outgrowth of the consolidation of the post-1899 leadership of Richardson, Williams, and Clark. These three leaders accounted for six of the twelve uncontested minority nominations in this era. Prior to that time, Democratic minority contests were almost as frequent as majority ones.

The post-1911 data indicate a marked decline in the frequency of Democratic contests regardless once again of the party's status or its leaders' incumbency. Only two revolts occurred during forty-two incumbency situations (5 per cent): Udall's in 1969 and Conyers's in 1973. No contests occurred during Democratic minorities in this era. Even the three successions of Claude Kitchin in 1921, Finis Garrett in 1923, and John Garner in 1929 were uncontested.

Of the eight instances of competition in the Democratic caucus for open posts, all occurred during majorities, and five were decided on the first ballot. The 1935 Bankhead-O'Connor and 1971 Boggs-Udall contests went to a second ballot, while the Wright-Burton election of 1976 had three ballots. Competition within Democratic caucuses in this era has not been protracted.

Among the post-1911 Republicans, revolts occurred more frequently and were more potent. Three of the six resulted in leadership overthrows – Mann in 1919, Martin in 1959, and Halleck in 1965; while two of the others shortened leadership careers. Mann refused the majority leadership after another contest in 1919 and Gillett left the House upon completing his third (and vigorously contested) speakership in 1925. Only Leslie Arends withstood the revolt against him in the caucus and remained in office during subsequent Congresses. Republican leaders apparently heed the messages of their caucuses.

---

[75] Randall was renominated in 1877 with 75 per cent of the votes on the first ballot, *The New York Tribune*, October 15, 1877, p. 1; and in 1879 with 53 per cent on the first ballot, *Washington Post*, March 18, 1879, p. 1.

[76] Keifer defeated George D. Robinson of Massachusetts, 44 to 15, *The New York Times*, December 2, 1883, p. 1. Six years later Reed received 85 votes of 166 cast in a second ballot victory, *Washington Post*, December 1, 1889, p. 1. Cannon received 162 votes of 187 cast on the first ballot in 1909, *The New York Times*, March 14, 1909, p. 1.

[77] During the second period the number of committees increased from 38 in 1863 to 61 by 1905, the highest figure ever recorded for the House. See Lauros G. McConachie, *Congressional Committees* (New York: Thomas Y. Crowell and Co., 1898), pp. 349–358 and various issues of the *Congressional Directory*. As a measure of the care with which committees were selected, the Speakers in the twenty-five congresses from 1857–1907 averaged 42.2 days in making their final committee appointments, *Hinds Precedents of the House of Representatives of the United States* (Washington, D.C.: U.S. Government Printing Office, 1907), IV, p. 891.

In cases of a vacancy, Republicans are more willing to have a contest than are Democrats (42 per cent to 35 per cent). As in the case of the Democrats, however, most open Republican contests were settled on the first ballot with only the eight-ballot Snell-Tilson fight an exception.

Contests have declined in the most recent era regardless of the occupancy of the leadership post. Contests for occupied posts decreased from 22 per cent in Period II to 10 per cent in Period III, while those for vacant posts decreased from 47 per cent to 37 per cent. The decrease, however, has been confined to the leadership of the Democratic party. Contests for occupied Democratic posts have declined by 24 percentage points (from 29 to 5 per cent) and contests for vacant ones have dropped by 23 points (from 58 to 35 per cent). Republican leaders have not been as fortunate; the rate of contests for occupied Republican posts has declined slightly (from 17 to 15 per cent) but the number of contests for vacant ones has *increased* sharply (from 29 to 42 per cent) in the post-1911 era.

Two party-minimizing explanations account for the general decline in contests. The first would be the institutionalization of the House leadership role. Greater acceptance of the authority of the leaders would presumably explain an increased reluctance of members to challenge either the sitting leadership or its anointed successors. The other explanation would stress the shift in power from the floor leaders to the committee chairmen in the years following the revolt against Speaker Cannon. The post-1911 diffusion of power has lowered the stakes involved in floor leadership contests and has thus led to a minimization of conflict. As plausible as each of these explanations may be, the fact that the decline in contests has been limited to Democratic caucuses limits their utility.

Types of Majorities and Minorities. One very important interperiod difference emerges from the data in both tables. The Democrats had a higher proportion of contests in Period II regardless of their party status or office occupancy. In the subsequent period, the Republicans had a greater frequency of contests in these categories. One factor which characterizes both the Period II Democrats and the Period III Republicans is that both had difficulty holding control of the House. The Period II Democrats held the House for three spans totaling sixteen years (an average of 5.3 years per span). The Period II Republicans were more successful. They held control of the House during four spans totaling thirty-two years (an average of 8.0 years). In Period III, the Republicans averaged 5.3 years per span (16/3) while the Democrats averaged 13.0 years (52/4). Thus, it is possible that the certainty (or lack of it) of gaining and keeping control of the House may have an impact upon the frequency of leadership contests.

Some support for this proposition appears when the Democratic party leaders are analyzed. From 1863 through 1899, the Democrats had eleven contests for the leadership. During this time, they emerged from near-extinction as a party to become a very competitive force in the House. Despite their frequent majorities in the House, however, only once did they control the Senate and the presidency at the same time. Certainly, this was a continual reminder of their tentative status even when they held the House. They were an uncertain minority at this time, and leadership contests were one way of gaining access to the perquisites of power. Following their sizeable defeats in the congressional elections of the late 1890s they became a secure minority. Even their consecutive victories in the House elections of 1910–1916 were marred by the fact that their presidential standard-bearer had gained the White House twice with less than half of the votes. Their majority was insecure. In keeping with the expectation that contests will be infrequent for such insecure majorities, as for secure minorities, there were no contests for the twenty-two elected Democratic leaders from 1901 through 1931.

Following Roosevelt's landslide victory in 1932 and the ever-mounting number of House Democrats in subsequent elections, contests for the leadership reappeared. Five of the eleven elected Democratic leaders were challenged in the caucus between 1933 and 1940. The next quarter-century, 1942–1967, was free of contests, largely because of the Rayburn-McCormack power combine. The Democratic party's majority throughout segments of this period, however, was not as secure as it had been in the 'thirties nor as it has been in the past decade. Five of the ten leadership selections since 1969 have been contested and the caucus defeats for committee chairmanships[78] lend substance to the "secure majority" thesis as an explanation.

The Republican pattern is not as clear. Four of their contests occurred in one eight-year span between 1881 and 1889 during which they alternated with the Democrats in controlling the House. Whether or not they were an insecure minority at the time is difficult to determine. Their next spate of contests appeared in the 1919–1925 period when they contested six leadership elections as a majority. One can speculate that having two presidential landslides behind them, they had become a secure majority which could afford to allow intraparty contests. But the proposition fails when the 1863–1875 and the 1895–1911 spans of Republican control are examined. No contests occurred in the first period, and only one (a minor challenge in 1909) occurred in the second. Secure Republican majorities do not always unleash a multitude of leadership contenders.

In one further effort to test the impact of party status upon House leadership change, the "declining minority" hypothesis will be explored. This proposition, advanced by Peabody, holds that severe losses in congressional elections will enhance the likelihood of minority party challenges.[79] Peabody's initial assessment was based upon his analysis of Republican revolts in the 84th–89th Congresses (1955–1966). When pushed back in time this proposition has some validity for the Republican party. In addition to the Martin and Halleck overthrows, there is the replacement of ex-Majority Leader John Q. Tilson by Bertrand Snell in 1931 after the Republicans lost control of the House and the challenge to ex-Speaker J. Warren Keifer in 1883 after a similar loss. Keifer was the only Speaker to be challenged for renomination in the caucus following his party's defeat at the polls.[80]

Looking at the Democrats, one sees a different picture. There have been ten instances since 1863 when the Democrats have convened as a minority after having lost either twenty-plus seats or 10 per cent of their membership from the previous Congress.[81] No Democratic leader who remained in the House and stood for renomination following these defeats was either overthrown or challenged in the caucus. Electoral success may have an impact upon the amount of discomfort which Democratic leaders face but electoral defeat apparently has none.

From this interpretation of the data, it appears that party status – be it majority or minority, secure, insecure, or declining – does not consistently explain the frequencies of House leadership contests and changes. The failure of party status as an explanatory variable forces a return to the concept of party distinctiveness: Democrats select their leaders differently than Republicans do.

Summary and Implications

The most direct finding in this study is that the two parties differ significantly in their patterns of House leadership change. Consequently, theories of House development which minimize the party variable in quest of institutional norms and electoral status predictors may slow our understanding of the dynamics of leadership change.

It is important to note however that institutionalized norms have appeared. Over time the number of House organizational posts has steadily increased as the leadership has become more bureaucratized. The increased importance of minor elective posts in the Republican organization (e.g., the chairmanships of the Republican Conference and Policy Committee) and the emergence of new appointive posts in the Democratic party (e.g., the deputy whips) indicates that this trend will continue.

---

[78] The Democratic caucus in 1975 ended the chairmanship career of Representatives Wright Patman of Banking and Currency, W. R. Poage of Agriculture, and F. Edward Hebert of Armed Services. Two subcommittee chairmanships were also voided. *Congressional Quarterly Weekly Report,* 33 (January 18, 1975), pp. 114–118; (January 25, 1975), pp. 210–212; and (February 1, 1975), p. 275. The most recent victim was Robert L. Sikes, who was stripped of his chairmanship of the Military Construction Subcommittee on January 26, 1977, *Congressional Quarterly Weekly Report,* 35 (January 29, 1977), p. 159.

[79] Peabody, "Party Leadership Change," pp. 692–693.

[80] Keifer had become a serious embarrassment to many of his fellow House Republicans because of his intemperate attacks on the press and reports of nepotism in the staffing of his office. (*The New York Times,* March 7, 1883, p. 1). On the eve of the vote for Speaker of the 48th Congress he rebuffed suggestions that he not run for the Republican nomination. He won the caucus nomination, but only 59 of the Congress's 118 Republicans attended, and 15 of them voted against him. *The New York Times,* December 2, 1883, p. 1.

[81] Congresses in this category include: the 39th (1865–1867), 43rd (1873–1875), 54th (1895–1897), 59th (1905–1907), 66th (1919–1921, 67th (1921–1933), 69th (1925–1927), 71st (1929–1931), 80th (1947–1949), and the 83rd (1953–1955).

Other signs of institutionalization may be found in the increased selection frequencies for House leaders and the greater incidence of continuations either in specific posts or party ranks during this latest House era. These developments contribute to the greater professionalization of the House leadership career, as does the decline in the number of departures arising from the leaders' own extra-House ambitions and the vagaries of their constituents.

It is the interparty differences, however, which hold the most meaning, for these differences appear to be related to fundamental questions of each party's social composition and governing philosophy. In this analysis of leadership change over time, the following observations were made for the Democrats: they have a higher proportion of appointed leaders than Republicans; their leaders moved between posts within the party hierarchy in an ordered succession; their appointed leaders are often "removed from above" by their elected ones; and their elected leaders are subjected to infrequent and unsuccessful contests in their caucuses. Republicans, on the other hand, were found to rely more upon election to select their leaders; their rate of interpositional mobility among leaders is very low; their appointed leaders were never removed by the elected ones; and while their leaders face contests at the same rate that the Democrats do, the incidence of successful challenges is much greater in their caucuses. Republican leaders, in short, are often "removed from below."

Regarding the impact of majority vs. minority status upon leadership challenges, contests appeared to occur more frequently in the majority party than in the minority one. Since the stakes are higher and the rewards are greater in the majority, the high incidence of contests for posts which contain real power should not be surprising. The last three intraparty contests which toppled leaders occurred during minority situations, but all three involved Republicans. The Democratic leadership's general invulnerability to caucus challenges, regardless of party circumstances, reduces the power of this variable in explaining change.

These interparty differences reveal two distinct leadership succession systems. The Democratic House organization is hierarchically arranged in a tightly controlled system which is designed to minimize internal conflict. The end result is a leadership with wide ranging "inclusive" attitudes toward issues that face the House. In contrast, the House Republicans appear to be relatively egalitarian in their leadership succession system, with open competition and a dominant role for the membership. The Republican leaders who emerge from this system appear to have "exclusive" attitudes toward House issues.

At this point, a possible reason for the difference in the functioning of the two parties may lie in the difference in their composition: the House Democrats are a large and heterogeneous mix of contending social groups, regions, and ideologies, while the House Republicans are a smaller and more homogeneous band of predominantly conservative small-town and small-city white males. Conflict within a homogeneous setting will not result in one social or ideological group's triumphing over another. Only the names in the offices will change, and in such a context, change of style will be more common than change of substance.

When a party is divided as the Democrats are, leadership change can result in a complete reshuffling of power. Formerly excluded groups can gain access to key posts and deny others this privilege. The legislative agenda may undergo an enormous transformation following an upheaval in the leadership. Any observer of the last few Democratic nominating conventions can readily see the consequences of such leadership change upon the party's presidential wing. It certainly would appear as if the controlled succession system which the House Democrats have carefully constructed is designed to avoid a similar outbreak of internecine warfare in the House.

Projections are always tentative, but it seems as if the Republican leadership succession system will remain relatively open and competitive. The party has undergone no fundamental reorientation in recent years. Hence, it is likely that its operating procedures will remain intact. The Democratic succession system has entered a transitional period as younger and more assertive members have joined the party's congressional ranks. Congressman Jim Wright is the first newly elected Democratic floor leader in thirty-six years who did not previously hold the position of whip. His election against a backdrop of deposed committee and subcommittee chairpersons indicates change for the floor leadership of the party. As the Democratic party becomes more philosophically united, and as the ancient regional conflicts recede, it is likely that the Democratic leadership will no longer need their controlled succession system and that the membership will no longer tolerate it.

Party Realignment and the Transformation of the Political Agenda: The House of Representatives, 1925—1938*

BARBARA DECKARD SINCLAIR
University of California, Riverside

Perhaps because of the political upheavals of the 1960s, party realignment has recently become a major area of research in political science.[1] The scholars who have dealt with the phenomenon have concentrated on its electoral aspects – on what Burnham calls the "sharp reorganizations of the mass coalitional bases of the major parties."[2] Another important characteristic ascribed to realignments has, to this point, received much less attention. According to Burnham, party realignments "result in significant transformations in the general shape of policy"[3] or, in Ladd's terms, in a new political agenda.[4]

From the realignment literature, the following theoretical sketch of the link between realignment in the electorate and policy transformation emerges. A party realignment is the result of the mass electorate's response to a new and highly salient issue which cuts across old party lines. Such massive changes in voting behavior will only occur if a considerable proportion of the electorate feels that the government should respond to or in some sense handle the new issue. That is, the catalytic event must be perceived as being amenable to government action. The new alignment will be lasting only if the party which benefits from the new issue does respond in a way that the new majority considers satisfactory. A realignment thus has policy implications: a new element is added to the political agenda.

Because the issue which precipitated the realignment is highly salient, the majority party must quickly attempt to satisfy its new coalition by responding to the issue. Because the issue is new, a satisfactory response is likely to require fairly radical departures from past policies. If the problem had been amenable to incremental, politics-as-usual solutions, realignment would have been unlikely. Through its responses to the issue now at the center of controversy, the majority party may create a new political agenda. Not only are certain pieces of innovative legislation passed, but the centers around which controversy revolves are permanently changed. Thus, in the United States, realignments are an important mechanism through which policy changes come about. They may be the major mechanism through which rapid and nonincremental change occurs.

It, therefore, seems important to scrutinize the policy process during periods of realignment. This study will examine the transformation of policy at the national level during the New Deal realignment. The above theoretical sketch suggests that a relatively abrupt and nonincremental transformation in policy should have occurred in direct response to the Depression and that the coalition supporting the new agenda should be a partisan one. Thus, the questions to be asked are: Was there a major transformation, in what areas of policy, when

---

*I would like to thank the Academic Senate of the University of California, Riverside, for an intramural grant which facilitated this research and the Inter-University Consortium for Political Research for the roll-call data.

[1] See Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: W. W. Norton and Co., Inc., 1970); James Sundquist, *Dynamics of the Party System* (Washington: The Brookings Institution, 1973); Gerald M. Pomper, *Elections in America* (New York: Dodd, Mead and Co., 1970); Everett Carl Ladd, Jr., *American Political Parties: Social Change and Political Response* (New York: W. W. Norton and Co., Inc., 1970); Kevin P. Phillips, *The Emerging Republican Majority* (Garden City: Doubleday and Co., Inc., 1970); Robert H. Blank, "Party Realignment and Congressional Elections," paper presented at the 1975 Meeting of the Western Political Science Association.

[2] Burnham, p. 10.

[3] *Ibid.*, p. 10. Sundquist and Ladd do discuss the transformation of policy, but neither undertakes a detailed analysis.

[4] Ladd, p. 2.

and how abruptly did it take place, and what were the coalitional bases for the new agenda?

Data and Framework of Analysis

The data used are roll-call votes in the House of Representatives, 1925 through 1938 (69th–75th Congresses). The House is used since biennial elections should make it particularly sensitive to the winds of change. The study begins with 1925 so as to provide a baseline against which change can be measured.

In order to examine transformation in policy outputs, some sort of categorization of policy types is needed. Aage Clausen has developed a useful categorization of policy in his work on the Congresses of the 1950s and early 1960s.[5] From an examination of the content of roll calls taken during that period, Clausen delineated five major policy domains – government management of the economy, social welfare, agricultural assistance, civil liberties, and foreign affairs. In each of the Congresses and in each issue domain, Clausen isolated a preponderant issue cluster which met the criteria for unidimensionality. Clausen further showed that, from the 83rd through the 88th Congresses, voting alignments on each issue dimension were highly stable.

Since no critical realignment occurred between the New Deal era and the period Clausen studied, one would expect that at least some of his policy dimensions took their present shape during the New Deal realignment. This should be the case if realignments are the major source of basic agenda change and if a new agenda did come out of the New Deal realignment. Clausen's categorization will thus be used. The questions to be asked are: Do any of the dimensions exist in the Congresses before realignment? Which dimensions, if any, develop in response to the realignment? Are there other stable dimensions during this period which have since disappeared, and if so, was their disappearance due to the realignment? Within the broad definition of each policy area, what is the policy content of the dimensions found, and does it change during the period of realignment? Did voting alignments change and, if so, how?

Procedure

The procedure used to establish the existence of unidimensional clusters of roll calls is similar to that used by Clausen. Roll calls on which the majority was greater than 90 per cent were excluded from the analysis. The remaining roll calls were categorized by issue content. The Johnson hierarchical clustering technique was applied to matrices of Yule's Q-coefficients within each congress.[6] A dimension was defined as consisting of a group of roll calls with common content for which the minimum Yule's Q-intercorrelation was .60. This criterion produces clusters with mean intercorrelations of approximately .80.

Since this procedure establishes unidimensionality, a simple scoring procedure can be used. In each cluster, one roll call was arbitrarily chosen to establish direction; the direction of the other roll calls was then determined by the sign of their correlation with the chosen roll call. For each congressman who had voted on one half or more of the roll calls in a given cluster, the percentage of the roll calls on which he voted in the chosen direction was computed, and this became his score.

Realignment and the Changing Regional Bases of the Major Parties

The data on the landslide Democratic victories during the 1930s are well known and will not be belabored here. The presentation of data on the regional composition of the major parties in the House shows how much the Democratic party changed not only in size but in composition during the period under study. Table 1 shows the percentage of the seats in each of eight regions held by Democrats over the seven Congresses and the percentage of all House seats held by Democrats.[7] During the 1920s, the Democrats were a regional party. In the 69th, 70th and 71st Congresses (1925–1930), almost two-thirds of Democratic House members were from the South, which then held about 30 per cent of all House seats. In no other region did the Democrats consistently win as much as one-third of the seats.

The magnitude of the change can be seen by comparing these three Congresses with the first three New Deal Congresses. In the 73rd (1933–1934), Democrats held over half the

---

[5] Aage Clausen, *How Congressmen Decide: A Policy Focus* (New York: St. Martin's Press, 1973). See also Clausen, "Measurement Identity in the Longitudinal Analysis of Legislative Voting," *American Political Science Review*, 60 (December, 1967), 1020–1035; and Clausen and Richard B. Cheney, "A Comparative Analysis of Senate-House Voting on Economic and Welfare Policy, 1953–1964," *American Political Science Review*, 64 (March, 1970), 138–152.

[6] For a description of Clausen's method see Clausen and Cheney. For the Johnson technique see Steve Johnson, "Hierarchical Clustering Schemes," *Psychometrika*, 32 (September, 1967), 241–254.

[7] The regional categorization is that used by the Survey Research Center.

Table 1. The Changing Regional Composition of the Democratic Party in the House 1925–1938 (Democratic % of Seats by Region)

	Congress						
Region	**69**	**70**	**71**	**72**	**73**	**74**	**75**
New England	12.5	10.3	12.5	25.0	41.4	55.2	44.8
Middle Atlantic	26.1	32.6	28.3	33.7	47.9	59.6	68.1
East North Central	16.3	18.6	14.0	37.2	71.1	65.6	72.2
West North Central	24.6	29.8	15.8	31.6	63.8	59.6	53.2
Solid South	98.9	98.9	94.7	97.9	100.0	100.0	100.0
Border South	65.9	70.7	51.2	78.0	94.9	92.3	92.3
Mountain	28.6	35.7	21.4	28.6	92.9	100.0	100.0
Pacific	15.8	10.5	10.5	15.8	65.5	69.0	79.3
Democrats as % of House members	42.1	44.8	38.2	50.6	72.0	74.0	76.6

seats in all regions except New England and the middle Atlantic. In the other nonsouthern regions, the Democratic percentage varied from 63.8 to 92.9. More than half the seats in every region were held by Democrats in the 74th Congress (1935–1936); and in the 75th (1937–1938), Democrats held a majority in all but New England and more than two-thirds in all but one of the other regions.

The New Deal realignment changed the Democratic party in the House from a southern and thus heavily rural party to a national party.[8] The southerners' proportion of the Democratic House membership dropped from almost two-thirds to about 40 per cent.

The Government Management Dimension

Clausen's government management issue domain centers on legislation dealing with the economy and the nation's resources.[9] Since tax policy and questions about the overall level of government spending are included, roll calls which fall into this issue category have existed since the nation began. By the 1920s, government regulation of business and involvement in public works had also had a long history. The fact that roll calls fall into the domain does not, however, guarantee the existence of a government management dimension. The requisite unidimensionality will result only if congressmen see the roll calls as similar and thus use similar voting cues.

In each of the Congresses under study a predominant government management dimension does emerge. Table 2 shows the stability of the dimension over time. The mean intercorrelation of scores is .93, and throughout the period, the relationship between government management scores and party is high.

This seeming stability hides considerable change in the content of the roll calls over time. In the pre-New Deal Congresses, federal spending was not seen as a tool for government

Table 2. The Government Management Dimension – Intercorrelations between Congresses

	Congress						
Congress	**69**	**70**	**71**	**72**	**73**	**74**	**75**
69		.93	.92	.96	.95	.92	.94
70			.95	.93	.94	.92	.91
71				.94	.93	.92	.90
72					.95	.92	.92
73						.91	.95
74							.87
Correlation with party (Dem=1) (Rep=0)	.95	.94	.90	.94	.96	.90	.94

---

[8] For a more detailed analysis of the change in the Democrats' constituency base, see David W. Brady, "Critical Elections, Congressional Parties and Clusters of Policy Changes: A Comparison of the 1896 and 1932 Realignment Eras," paper delivered at the 1975 annual meeting of the American Political Science Association.

[9] Clausen, *How Congressmen Decide,* pp. 49–50.

management of the economy. The Democrats favored additional government spending when it would directly help their constituents. Money for additional rural letter carriers[10] and for flood control projects on the lower Mississippi,[11] for example, were strongly supported by the heavily rural Democratic membership. When the proposed spending did not benefit their constituents, Democrats tended to interpret it as milking the taxpayer for the benefit of selfish private interests or for the benefit of the Republican party and voted in opposition. Thus the Democrats opposed spending several hundred thousand dollars to improve a road in northern Alaska, claiming it would benefit only a few gold prospectors.[12] They attempted to reduce the appropriation for ocean mail service, saying it was simply an unwarranted subsidy for certain steamship companies.[13]

On regulatory legislation, varying tinges of populist-progressive ideology are evident. Democrats consistently opposed any legislation which would directly or indirectly weaken the anti-trust laws. Thus a proposal to allow pools or combinations for the purchase of raw rubber abroad[14] and another to lift the prohibition on interlocking directorates in banks not in substantial competition with each other brought forth classic populist statements on the House floor.[15] The Democrats opposed some regulatory legislation, arguing that the legislation did not contain sufficient safeguards against monopoly.[16] State's rights and Jeffersonian minimal government arguments, however, also appear. Democrats, for example, opposed additional employees for the Federal Power Commission claiming it was just the first step towards the creation of a giant bureaucracy.[17]

On tax policy, elements of populist-progressive ideology are also evident. While no longer united in opposition to Secretary of the Treasury Mellon's proposals to lower taxes on high incomes as they had been in the early 1920s,[18] Democrats did oppose Mellon's proposals much more consistently than Republicans did. Democrats also favored a graduated corporate tax and attempts to close loopholes in corporate tax law.[19] Tariff legislation, which was important in the 71st Congress (1929–1930), saw Democrats tending to take the free trade position on manufactured goods but favoring some protection for farm products and raw materials.

Several of the minor government management dimensions show a stronger populist-progressive thrust. In the 69th (1925–1926), a number of roll calls on allowing branch banking and on radio regulation form a unidimensional cluster. In the 72nd (1931–1932), roll calls on limiting the size of loans under the Reconstruction Finance Corporation, for imposing a surtax on high incomes, for congressional review of any tax refund over $20,000, and for government operation of Muscle Shoals cluster. These dimensions bring forth moderately similar voting patterns (r = .75). Neither is as highly related to party as are the major government management dimensions (69th, r = .61; 72nd, r = .71).

During this period, the Republicans are a pro-business party – as shown by their support for the Mellon tax bills, their attempt to turn over Muscle Shoals to private business and the sort of regulatory legislation they sponsored. Thus, in the pre-New Deal Congress, the extent of pro- or anti-big business sentiment does to some extent distinguish the parties, but neither party advocated an activist federal role in regulating or managing the economy.

With the 73rd Congress (1933–1934), the major government management dimension becomes a New Deal dimension. In the 73rd, a high score indicated the congressman voted for the creation of the TVA, for the invalidation of the gold standards clause,[20] for Roosevelt's tax bill,[21] for the Reciprocal Trade Agreements Act, and for the Securities Exchange Act. In the 74th and the 75th Congresses (1935–1938), the major government management dimension included roll calls on Roosevelt's "soak the rich" tax bill,[22] on a controversial corporate tax bill,[23] on the regulation of public utility holding companies (recommital and final passage), on extending the time in which the President could further devalue the dollar,[24] and on the Banking Act of 1935. By and large, the bills included in the dimension were proposed or at least strongly supported by the administration and represent an activist federal government philosophy. Congressmen's voting behavior on these measures, as shown before, was highly partisan. Thus, while the gross voting response remained stable, the content of the measures included in the government management dimension changed with the coming of the New Deal. The result was a much greater ideological distance between the parties, with the Democrats now clearly supporting an activist position. The average Democrat's support for the party position on the major government management dimension is actually higher after 1930 than in the less ideologically charged preceding years. (See first column of Table 3.)

---

[10] Amendment to H.R. 5959. 69th Congress, Jan. 4, 1926, CR–67–2–1439.

[11] S.3740. 70th Congress, April 24, 1928, CR–69–7–7124.

[12] H. J. Res. 73. 69th Congress, February 2, 1927, CR–68–3–2845.

[13] H.R. 853. 71st Congress, January 18, 1930, CR–72–2–1920.

[14] H.R. 8927. 70th Congress, April, 1928, CR–69–6–6047.

[15] H.R. 6491. 70th Congress, February 1, 1928, CR–69–3–2339.

[16] See, for example, debate on H.R. 10288, a bill regulating buses in interstate transportation, 71st Congress, February 24, 1930, CR–72–6–6028.

[17] H.R. 8141. 70th Congress, March 20–21, 1928, CR–69–5–5066, 5112.

[18] David Burner, *The Politics of Provincialism: The Democratic Party in Transition 1918–1932* (New York: Alfred A. Knopf, 1968), pp. 162–164.

[19] See roll calls on H.R. 1, 70th Congress, December 15, 1927, CR–69–1–715, 715.

[20] H.J. Res. 192.

[21] H.R. 7835.

[22] H.R. 8974, 74th Congress.

[23] H.R. 12395, 74th Congress.

Not all major New Deal measures cluster in the main dimension. In the 73rd (1933–1934), a number of roll calls on the National Industrial Recovery Act form a dimension of their own. The same is true of roll calls on the death sentence provision of the Public Utility Holding Company bill in the 74th (1935–1936). This bill would, in effect, have outlawed holding companies in the public utilities area.[25] Both were administration proposals, and voting on each is influenced by party, but on neither is the level of party voting as high as on the major government management dimensions. (The correlation with party is .68 for the NIRA; .72 for the death sentence dimension.) These lower correlations are due primarily to the Democrats' lower support for these measures as compared with their support for those included in the main government management dimension. The mean Democratic support on the major dimensions for the 73rd through the 75th Congresses is 90.1 per cent; on the minor dimensions, 74.2 per cent. While Democrats did give substantial support on these measures, the figures clearly indicate that Roosevelt by no means had a blank check even from these overwhelmingly Democratic Congresses. The party position had shifted toward favoring activist government, but many Congressional Democrats were leery of carrying the activist thrust too far.

Throughout the period, party affiliation is highly related to voting on the major government management dimensions and substantially related on the minor ones. But, since the relationship is not perfect, other factors which might affect the vote must be examined. In the contemporary Congresses region is an important predictor of vote; and, as was shown earlier, the realignment significantly changed the regional composition of the Democratic party. It thus seems appropriate to ask: Did regional subgroupings display distinctive voting patterns in the 1920s, and what effect did the realignment have?

The procedure used to answer this question

Table 3. Government Management Voting Scores by Party and Region

	Democrats				Republicans		
Congress	All	NE&MA[a]	SS&BS[a]	WNC[a]	All	NE&MA[a]	WNC[a]
69	86.4	93.4	85.8	86.7	5.7	3.8	2.6
70	83.4	62.4	90.6	81.5	14.2	6.8	18.6
71	84.7	67.9	88.9	93.2	9.9	1.5	23.2
72	94.8	95.9	94.6	93.3	11.6	4.8	21.0
73	91.6	88.8	96.4	91.4	10.9	6.0	24.7
74	89.3	86.5	90.0	91.5	11.8	4.5	35.4
75	89.4	89.8	94.7	80.7	6.2	3.2	12.1
69 Populist-Progressive	49.9	3.8	55.0	73.4	6.8	1.6	11.8
72 Populist-Progressive	71.4	37.4	79.6	90.9	20.3	4.3	38.9
73 NIRA	78.9	94.5	77.5	62.2	16.2	10.2	18.2
74 Public Utility Holding Co.'s	69.4	60.0	72.4	83.0	8.4	2.5	26.2

[a] NE&MA = New England and Middle Atlantic; SS&BS = Solid South and Border South; WNC = West North Central.

---

[24] S416, 75th Congress.

[25] For a good description of the controversy, see Arthur M. Schlesinger, Jr., *The Politics of Upheaval* (Boston: Houghton Mifflin Co., 1960), pp. 302–325.

is stepwise multiple regression analysis.[26] The dependent variables are the scores on the various government management dimensions, the independent variables, party and the eight regions defined by the SRC. For each dimension, an equation is estimated for all House members using party and region as the independent variables; then an equation for each party is estimated with the region dummy variables only as independent variables. Because regression equations with this many variables are difficult to interpret at a glance, tables showing mean scores for the most distinctive regional groupings are given.

Despite the strong relationship between party and vote on the major government management dimensions, some regional differences within party do appear. Within the Republican party, regional differences account for substantially more than 10 per cent of the variance in scores in all but one Congress (69th, 1925–1926). In the pre-New Deal Congresses, the greatest differentiation in voting behavior along regional lines occurs in the 70th Congress. Of the major government management dimensions, this one has the strongest populist-progressive content.[27] The progressive heritage of many congressmen from the West North Central region shows up in their being consistently the most deviant regional grouping. (See Table 3). Prior to the 73rd Congress (1933–1934), east north central congressmen also show a greater tendency than other Republicans to vote with the Democrats but, in the New Deal Congresses, this deviance ceases. Throughout the whole period, northeastern Republicans are most loyal to party.

From the 69th through the 71st Congress (1925–1930), Democrats show somewhat greater splits along regional lines than do Republicans. After the Democrats become the majority party in the 72nd (1931–1932), the positions of the two parties are reversed. The strong populist-progressive thrust of the government management dimension in the 70th Congress resulted in distinct regional voting patterns with the South and the West North Central region strongly supportive and the northeast most opposed. Through the 73rd Congress (1933–1934), when regional splits occur, the same pattern emerges. Only in the 75th Congress (1937–1938), do Congressmen from the middle Atlantic states become somewhat more loyal than the average Democrat and, even then, they are less supportive of the party position than are southern Democrats.

The two minor government management dimensions with a strong populist-progressive thrust which appear in the pre-New Deal Congresses elicited similar regional voting patterns. The West North Central and to a lesser extent the southern Congressmen were especially supportive of the populist-progressive pole, the northeastern congressmen especially opposed. Not surprisingly, given its antimonopoly thrust, the public utilities holding companies dimension also produced similar voting patterns. Scores on this dimension show a correlation of .72 with those on the minor dimension in the 69th and of .80 with that in the 72nd. On the National Industrial Recovery Act dimension, in contrast, regional voting patterns are quite different. The governmental philosophy underlying this legislation significantly conflicts with a major tenet in populist-progressive thought, and many members were concerned that the NIRA, in effect, sanctioned monopoly. On this dimension, Democratic congressmen from the West North Central region and from the solid South, who on most government management legislation were most supportive of the Democratic party position, were most likely to vote in opposition.

This discussion of region should not obscure the predominance of party as a predictor of vote on the government management dimensions. The findings do show that the realignment did not immediately change the regional voting patterns of the 1920s into those that prevail today. In the 1920s, southerners were the most loyal segment of the Democratic party while the relatively few northeastern Democrats were the most deviant.[28] Within the Republican party, northeasterners were the most loyal; Midwesterners, especially those from the West North Central states, were the most likely to defect from the party position. The immediate effect of the realignment was to alter massively the party balance and, through the infusion of large numbers of new Democrats, to change policy outputs drastically. These new Democrats voted very differently from the Republicans they had replaced. The realignment did not, however, result in any drastic shift in relative party loyalty within either party. On the government management dimensions, which include much of the significant New Deal legislation, the regional patterns of the 1920s persisted through the 1930s.

---

[26] The SPSS stepwise regression program was used. See Norman Nie, Dale Bent, and C. Hadlai Hull, *Statistical Package for the Social Sciences* (New York: McGraw-Hill, 1970), Chapter 15. Tables presenting the regression results in full are available from the author.

[27] Included are a number of roll calls on tax measures and several on bills which would indirectly relax antimonopoly laws in certain cases. See notes 14, 15 and 19.

[28] These findings confirm those of Turner. See Julius Turner, *Party and Constituency: Pressures on Congress* (Baltimore: Johns Hopkins Press, 1951), p. 136.

Agricultural Policy

In the government management area, one major dimension persists throughout the period under study, even though, with the coming of the New Deal, the content of the roll calls included changes significantly. In the area of agricultural policy, on the other hand, voting alignments change drastically within the period under study. As Table 4 shows, the dimension in existence at the beginning of the period is completely transformed. In the 69th and 70th Congresses (1925–1928), voting on agricultural policy is unrelated to party; from the 71st (1929–1930) on, it is highly influenced by party.

This shift is the result of a change in the content of the farm bills being considered and of a change in the environment. Farmers did not participate in the prosperity of the 1920s. With the end of World War I, farm prices dropped drastically, while the prices of farm supplies and other manufactured goods remained high. Whether and how farmers should be helped was one of the major sources of controversy during the 1920s.[29] In Congress, the controversy centered on the McNary-Haugen plan, the brainchild of George Peek of the American Farm Bureau Federation. The McNary-Haugen bill proposed the setting up of a two-price system for agricultural products. Domestic prices were to be kept high by restricting the domestic supply. This would be done, not by cutting back production, but by dumping abroad whatever could not be sold at the high domestic price. The plan appealed to farmers who sold most of their output in the domestic market; it had little to offer those, such as cotton growers, who sold a large proportion of their crop abroad. All the roll calls included in the agricultural dimension in the 69th and 70th (1925–1928) were taken on various versions of the McNary-Haugen plan. A version was defeated in the House in the first session of the 69th, as had been the case in the previous Congress. Both houses passed the bill in the second session of the 69th and in the 70th; but in each case, it was vetoed by President Hoover. Its supporters' inability to muster the votes to override the vetoes killed the McNary-Haugen plan as a viable answer to the farmer's plight.

The Depression, which brought a further drastic drop in cotton and wheat prices, and the great drought of 1930 made it clear even to Hoover that something had to be done and aided in uniting the Democrats. The 71st Congress's agricultural dimension consists of roll calls on Democratic attempts to strengthen and liberalize Hoover's proposals. To the rather weak Agricultural Marketing Act of 1929 the Democrats attempted to add the debenture plan. An exceedingly complex piece of legislation, this plan would essentially have used tariff receipts to aid farmers wherever they sold their crops. The Democrats also attempted to increase the amount of money to be spent on drought relief over the amount desired by Hoover. Since, at this time drought was especially severe in the lower Mississippi Valley, this legislation was of great interest to southerners.

In the 72nd Congress (1931–1932), no agricultural dimension appears.[30] With the House narrowly controlled by the Democrats and the presidency still Republican, stalemate on agricultural policy had been reached. From

Table 4. The Agricultural Policy Dimension – Intercorrelations between Congresses

Congress	69	70	71	73	74	75
69		.85	.14	.48	.41	.33
70			.21	.54	.47	.43
71				.94	.92	.89
73					.88	.82
74						.80
Correlation with party	.09	.13	.93	.90	.89	.76

---

[29] For a good brief discussion of the farm situation and the various legislative remedies proposed, see John D. Hicks, *Republican Ascendancy 1921–1933* (New York: Harper Torchbooks, 1960), pp. 193–200.

[30] H.R. 13991, a fairly comprehensive agriculture bill, was passed in the House on January 12, 1933, but died in the Senate. There was only one roll call vote on the bill.

the 73rd Congress on, the agricultural dimension centers on the New Deal farm program. Roll calls on the Agricultural Assistance Act, on the Emergency Farm Mortgage Act, on the Cotton Control Act, the Soil Conservation Act, and other similar bills are included.[31] The severity of the farm situation and the wide coverage of the legislation which provided benefits to producers of all farm products assured generally high Democratic party support for this legislation.

This brief overview of the changing farm situation and the resulting changes in policy provide a plausible explanation for the change in voting patterns described above. As one would expect, during the McNary-Haugen period, voting was largely sectional (see Table 5). Congressmen from the northeast and from the solid South opposed the legislation; West North Central congressmen were its most fervent supporters. The split was most intense within the Republican party; in both the 69th and the 70th Congresses, the regional dummy variables account for almost half of the variance in Republican scores. During the New Deal Congresses, the Republican party continued to split on farm legislation, with the West North Central congressmen consistently most supportive of the Democratic position. Among Democrats, regional splits were much less deep. The solid South had the highest levels of support, but all sections of the party supported the administration program.

In the agricultural policy area, regional voting alignments gave way to party voting under the pressure of political and economic events. The change took place before the 1932 election and the precipitant was the worsening of the farmers' position in 1929 and 30. The great Democratic victories of 1932, 1934, and 1936 were, however, required to solidify the new alignment behind a comprehensive and, at the time, radical program.

Social Welfare

The origins of the welfare state are popularly associated with the New Deal. Prior to the Depression, legislation designed to give relatively direct help to the individual[32] was not completely without precedent in American history. Some labor legislation was on the books;[33] veterans' pensions and other social services for certain classes of veterans had a relatively long history; and, in the early 1920s, legislation concerning maternal and infant health needs was passed.[34] Nevertheless, direct help to individuals was generally not considered a legitimate federal function.

During the period under study, the social welfare dimension first appears in the 71st Congress (1929–1930). Roll calls on a veterans' pension bill and on providing additional hospital facilities for veterans are included. More clearly related to the Depression are the roll calls on taking a census of unemployment and on the use of federal funds for relief. President Hoover favored relying upon private charity to alleviate the misery caused by widespread unemployment and was firmly opposed to a federal "dole."[35] The Democrats read their gains in the 1930 midterm election as a mandate, and in the 72nd Congress, Speaker John Nance Garner proposed a several billion-

Table 5. Agricultural Policy Voting Scores by Party and Region

	Democrats				Republicans		
Congress	All	NE&MA	SS	WNC	All	NE&MA	WNC
69	55.6	41.1	47.6	92.9	50.1	11.3	90.0
70	64.2	38.7	59.6	93.8	52.3	9.8	91.0
71	93.9	81.7	97.2	95.6	8.5	1.6	12.6
73	88.3	82.9	94.8	76.1	11.6	3.2	37.2
74	90.0	79.1	99.0	90.0	7.5	.7	27.0
75	76.8	76.6	88.6	65.8	9.1	.9	28.9

---

[31] For a discussion of this legislation, see Lawrence Henry Chamberlain, *The President, Congress and Legislation* (New York: Columbia University Press, 1946), pp. 252–268.

[32] For Clausen's definition of the social welfare domain see *How Congressmen Decide*, p. 46.

[33] For an overview of pre-New Deal labor legislation, see Chamberlain, *President, Congress and Legislation*, pp. 138–164.

[34] William Chafe, *The American Woman: Her Changing Social, Economic, and Political Roles, 1920–1970* (New York: Oxford University Press, 1972), p. 27.

[35] William E. Leuchtenburg, *The Perils of Prosperity 1914–1932* (Chicago: University of Chicago Press, 1958), p. 252.

Table 6. The Social Welfare Dimension – Intercorrelations between Congresses

Congress	71	72	73	74	75
71		.91	.89	.93	.89
72			.87	.88	.80
73				.89	.82
74					.80
Correlation with party	.89	.89	.90	.92	.83

dollar relief bill.[36] Democratic control of the House and Hoover's statement that the bill was "the most gigantic pork barrel ever proposed . . . an unexampled raid on the public treasury"[37] helped ensure that the measure would be perceived in partisan terms. The struggle over relief spending dominates the major social welfare dimension during the New Deal Congresses also. Debate in the 71st and 72nd Congresses (1929–1932) centered around whether the federal government should provide direct relief; in the later Congresses, the controversy concerned the dollar level.

As Table 6 shows, voting on the major social welfare dimension was partisan at its inception and remains so during this period. In neither party are major regional variations in voting response evident. (See Table 7.) Midwestern and Pacific Republicans do show some tendency to support the Democratic position more than their party colleagues. Among Democrats, no consistent regional splits occur; there is certainly no tendency for the South to be less loyal than other segments of the party.

Two minor social welfare dimensions which appear in the 75th Congress (1937–1938) produced quite different alignments. One includes roll calls on the National Housing Act; the other consists entirely of roll calls on the Fair Labor Standards Act. On neither is voting as partisan as on the major social welfare dimensions. The correlations with party for the first is .67; for the second, .38. Both of these dimensions produced a North-South split within the Democratic party, with southern Democrats much less supportive of the party position than their northern colleagues. On the fair labor standards dimension, which produced the deeper split, the two southern regional dummy variables account for almost half the variance in Democratic scores.

The regional split on the Fair Labor Standards Act had an economic foundation. Southerners feared that a nationwide minimum wage would nullify their region's advantage in attracting industry. For the same reason, some usually very conservative northeastern Republicans supported the bill. Still, since this split within the Democratic party was to become routine rather than exceptional and since the split also appears, though less strongly, on the other minor dimension, the voting alignment which appeared on the Fair Labor Standards Act cannot be interpreted simply as an anomaly. Rather, the alignments on both of the minor social welfare dimensions seem to mark the beginning of the pattern Clausen finds in the 1950s and early 1960s.

Why, then, did the southern Democrats go along on the major social welfare dimensions, and why did the regional split begin at this

Table 7. Social Welfare Voting Scores by Party and Region

	Democrats			Republicans		
Congress	All	NE&MA	SS	All	NE&MA	WNC
71	97.3	96.9	97.9	20.9	11.9	26.6
72	96.3	97.4	99.7	11.5	5.0	13.5
73	86.8	93.8	87.4	2.6	.7	7.6
74	91.1	93.3	95.0	2.0	.6	4.8
75	83.6	85.0	84.3	7.7	3.4	15.8
75–Housing	82.6	96.8	62.2	25.3	16.3	38.9
75–Fair Labor Standards Act	77.8	99.0	39.2	43.5	46.1	39.8

---

[36] E. Pendleton Herring, "First Session of the Seventy-second Congress," *American Political Science Review*, 26 (October, 1932), 869–872 for a discussion of the bill and the political controversy surrounding relief spending in that Congress.

[37] Quoted in *ibid.*, p. 869.

time? Certainly the severity of the national emergency played an important role in solidifying the support of all Democrats behind Roosevelt's programs. The South as the poorest section of the country was hit especially hard by the Depression and southern congressmen were quick to demand federal relief. The emergency itself and most of the social welfare programs designed to alleviate it were seen as temporary. Almost all of the roll calls included in the major social welfare dimensions concern such temporary relief programs. A congressman's vote for work relief appropriations was simply a pragmatic response to a temporary emergency. It did not necessarily imply a commitment to a new philosophy of government predicated on ongoing government responsibility for the welfare of the individual.

In contrast to the major social welfare dimensions, the two minor ones which appear in the 75th Congress (1937–1938) are predominantly composed of roll calls on permanent programs and took place at a time when many believed the emergency to be over.[38] A positive vote on them does imply a commitment to continued government activism in the social welfare area. It is on such programs that the North-South split in the Democratic party begins to appear.

Thus, under the stimulus of economic emergency, a new dimension appears. Neither in content nor in the voting alignments it produced is it identical with the modern social welfare dimension. Only at the end of the period under study does one begin to see outlines of the modern dimension taking shape.

Civil Liberties

In addition to the three issue dimensions already discussed, Clausen identifies two others in the modern Congress. During the period under study, no stable civil liberties or international involvement dimension exists. Roll calls on issues with civil liberties implications do appear and, in a number of Congresses, one or more clusters of such roll calls meet the criterion of unidimensionality. These clusters do not, however, correlate at a high level across Congresses.

If the civil liberties domain is defined broadly, seven such clusters appear. One, in the 69th Congress (1925–1926), consists of roll calls on immigration legislation; another, in the 74th (1935–1936), concerns the deportation of aliens; two, in the 71st and 72nd (1929–1932), concern prohibition; another in the 71st, congressional reapportionment. The two which appear in the 75th (1937–1938) most clearly fall into the civil liberties domain as defined by Clausen. One consists of roll calls on anti-lynching legislation; the other includes votes on withholding funds for the salaries of District of Columbia teachers who advocate communism and on instituting an investigation of sit-down strikes. In general thrust if not in specific content, these two clusters are similar to the modern civil liberties domain. Yet their correlation is –.22. The only really high correlation (.91) between any pair of these clusters is between the immigration cluster in the 69th Congress and the anti-lynching cluster in the 75th.

The scoring procedure on these clusters is such that a high score always indicates pro-civil liberties voting. A number of the correlations are negative, indicating that a congressman who took a pro-civil liberties position on one was likely to take an "anti" position on the other. The signs of the correlations with party also vary. Voting alignments on these clusters are thus unstable.

As Table 8 shows, Republicans sometimes took the pro- and sometimes the anti-civil liberties position. Regional differences are seldom great. The variation in the Republicans' voting behavior on these clusters indicates that they did not perceive the issues involved as philosophically similar. Among Democrats, the situation is somewhat different. All the clusters produce splits along regional lines and, while the depth of the split varies from cluster to cluster, its form is remarkably stable. In every case, the South – especially the solid South – takes the anti-civil liberties position and the northeast takes the "pro" position. West North Central and mountain congressmen tend to take an intermediate position. For Democrats, the mean intercorrelation of scores on these clusters is .55, compared with a mean of .01 for all members (.37 when the signs of the correlations are ignored). The correlations for Democrats only are not high enough to label these clusters a dimension; still they indicate that, among Democrats, voting alignments vary much less from cluster to cluster than they do among Republicans.

The heterogeneity of the Democratic party made party unity impossible on the sort of issues included in these clusters. During the 1920s the Democratic party was primarily composed of two quite distinct and often conflicting elements. On the one side was the rural, mostly southern element which tended to

---

[38] James T. Patterson, *Congressional Conservatism and the New Deal* (Lexington, Ky.: University of Kentucky Press, 1967), especially pp. 169, 175.

Table 8. Civil Liberties Voting Scores by Party and Region

	Democrats			Republicans		
Congress–Legislation	All	NE&MA	SS	All	NE&MA	WNC
69–Immigration	31.1	100.0	8.1	95.9	99.6	96.4
71–Prohibition	39.1	97.6	18.0	11.6	16.7	7.1
71–Reapportionment	43.5	100.0	25.7	76.0	93.3	34.8
72–Prohibition	65.6	96.7	46.7	50.2	62.1	31.6
74–Alien Deportation	68.9	98.5	40.6	39.5	49.2	47.2
75–Anti-Lynching	59.4	96.0	1.1	94.7	93.5	96.1
75–Civil Liberties	53.0	69.1	27.8	10.9	3.2	26.0

be Protestant, prohibitionist, and usually nativist; on the other was the big city element which was usually wet, frequently Catholic, and often newly immigrant.[39] The 1928 nomination and election campaign was the most dramatic but not the only manifestation of this split. The influx of Democrats from previously Republican districts during the 1930s shifted the balance away from the South but did not alter the underlying division. In the House, this split had little effect on most issues during the period under study, but, on the moral, style-of-life issues included in the civil liberties clusters, the differing outlooks of the elements became predominant. Basically, then, the split within the Democratic party on such issues predates the existence of a stable civil liberties dimension.

No other stable issue dimension appears during the Congresses under study. While a number of other scales were found in various Congresses, the voting alignments they call forth are ephemeral. There is no stability across Congresses.

Mechanisms of Change

In response to economic crisis, a new agenda was developed. Even before the 1932 election, congressional Democrats had responded to the crisis in a more innovative way than the ruling Republicans had. The election of a president and of a heavy congressional majority in 1932 gave the Democratic party the chance to build a majority coalition in the country by offering solutions to the economic problems, and Roosevelt was quick to seize this opportunity. The support the congressional Democrats gave Roosevelt's programs made the Democrats the party of the new agenda.

Quite clearly the legislative success of the new agenda was due to the large number of new Democrats elected in 1932 and re-elected, with reinforcements, in 1934 and 1936. As was shown above, voting behavior during the New Deal was heavily partisan. Several scholars have pointed out that membership replacement is a major mechanism of change in Congress.[40] Certainly, in the case under study, partisan turnover had massive policy effects. The hypothesis on the importance of turnover as a mechanism of change is not, however, restricted to partisan change. It is argued that new members, elected under different circumstances, will differ in their voting responses from their senior party colleagues. In a period of realignment, new members of the majority party will be more supportive of the new agenda than will senior members whose political behavior was shaped in an earlier period and who have less reason to interpret their election as a mandate for the new agenda.

To test the hypothesis, support scores on the major government management, social welfare and agricultural policy dimensions and on several of the important minor dimensions have been computed for veterans and newcomers within each party. Veterans are defined as congressmen first elected in 1928 or before; newcomers are members first elected in 1932 or later. Because the 1930 election is transitional, taking place after the Depression had begun and, under that stimulus, resulting in significant Democratic gains, members first elected to the 72nd Congress are excluded.

For Democrats, the hypothesis predicts that newcomers will have higher support scores for New Deal legislation than veterans do. The prediction for Republicans is less clear. On the one hand, because new Republicans were first elected during the New Deal, one might hypothesize that they would be less set in their ways, less committed to the older party positions, and thus more sympathetic to the new agenda. Conversely, one might argue that since they won despite a strong adverse electoral tide, they would see themselves as mandated to oppose the new agenda and thus should have even lower support scores than their senior party colleagues.

---

[39] Burner, *Politics of Provincialism*.

[40] See David W. Brady and Naomi B. Lynn, "Switched-Seat Congressional Districts: Their Effect on Party Voting and Public Policy," *American Journal of Political Science*, 17 (August, 1973), 528–543; Clausen, *How Congressmen Decide*, pp. 231–232. Marvin G. Weinbaum and Dennis R. Judd, "In Search of a Mandated Congress," *Midwestern Journal of Political Science*, 14 (May, 1970), 302; Barbara Hinckley, "Interpreting House Midterm Elections: Toward a Measurement of the In-Party's 'Expected' Loss of Seats," *American Political Science Review*, 61 (September, 1967), 697–699.

With respect to Republicans, Table 9 offers little support for either form of the hypothesis. There are few consistent differences between Republican veterans and newcomers. Only on agricultural policy do newcomers consistently support the New Deal position more than veterans do, but even here the differences are not impressive in magnitude.

More important for the status of the hypothesis is the result of the test on the Democrats. Here again little support is found. On the major dimensions, the differences which do occur run counter to the prediction. Veterans are more supportive of the new agenda than newcomers are. Only on the Fair Labor Standards Act dimension do newcomers give significantly more support than veterans. The difference is largely due to the overrepresentation of members from the solid South among veterans. This is not, however, sufficient reason for dismissing the difference. Newcomers from the solid South were more supportive on this dimension than were veterans from the same region. Thus, while the hypothesis in general is not supported, it does hold on this one particularly innovative piece of legislation.[41]

Why generally were veterans somewhat more supportive than newcomers? A part of the explanation may lie in the newcomers' lack of the hindsight we possess. They easily could have interpreted their election as a mandate for the new agenda; nevertheless, many were elected from previously safe Republican districts and this may have resulted in a modicum of caution. After all they could not have known that many of these districts would become safely Democratic. The scores in Table 9 indicate that such responses, if they did occur, played only a minor role in determining newcomers' voting behavior. Newcomers may have been slightly less supportive than veterans, but their absolute level of support was still high.

The problem then resolves itself into explaining why the *veterans* were highly supportive. Why did congressmen whose ideological positions and political behavior patterns were formed in a previous era accept the new agenda so readily? Certainly the extent of the economic disaster is one answer. Our notions about the resistance of human beings to change are based on observations during periods of stability or slow change in the environment. Furthermore, the veterans participated in the development of the new agenda both before the 1932 election and afterward.[42] With some notable exceptions, this agenda was not simply thrust upon them by the administration. The data certainly support the notion that periods

Table 9. Veterans' and Newcomers' Support for the New Deal

	Democrats		Republicans	
Congress	Veterans	Newcomers	Veterans	Newcomers
Government Management				
73	95.1	90.3	10.5	10.7
74	90.8	90.1	7.5	18.0
75	95.9	87.1	8.1	4.7
Agricultural Policy				
73	90.6	84.8	8.6	14.9
74	92.5	89.3	5.5	11.2
75	87.2	72.4	7.3	10.3
Social Welfare				
73	88.1	85.5	2.4	2.9
74	94.7	89.8	1.3	2.6
75	85.2	83.8	7.5	8.9
Minor Dimensions				
NIRA	86.7	72.3	15.0	15.3
Utility Holding Companies	69.1	70.9	5.4	12.0
Fair Labor Standards Act	60.2	82.0	45.8	42.8

---

[41] But see Brady, "Critical Elections," especially Table 23. On a general measure of party support, Brady finds that, in the 73rd Congress, switched-seat Democrats are more loyal than nonswitched-seat Democrats.

[42] Chamberlain, *President, Congress and Legislation.* See especially his summary, pp. 448–464.

of radical change in the political-economic environment may bring about rapid ideological change. Depending upon whether circumstances are reinforcing or not, the ideological change may be thorough and permanent or superficial and temporary. But under the felt need to do something to alleviate a new and frightening situation, it does inform action and, in the case of policy makers, leads to major, nonincremental changes in policy outputs.

Many southern Democrats were increasingly to defect from major elements of the new agenda in later years. Northern Democrats, who in the era before realignment had shown even less evidence that they would be receptive to such radical departures in policy, were to remain firm supporters. But, for everyone, the basepoint around which controversy about the role of the government in American society would revolve had shifted in the activist direction.

Conclusion

The 1930s realignment and the new agenda were clearly the products of the great Depression. Key argues that 1928 was a realigning election in New England.[43] Nationally, however, the 1928 election saw the Democratic presidential candidate badly defeated and Democratic House strength decline in almost all areas. Even in New England, the Democrats picked up only one House seat, increasing their seats in that region from three to four, the same number they had held in the 69th Congress. One could argue that the potential for a realignment along class lines had existed since the 1890s. The rapid and massive industrialization following the Civil War produced some big winners and many victims. In 1896, the dominant populist element in the Democratic party did attempt to build a coalition of small farmers and urban workers. The attempt did not succeed and, since party identification hardens with the passage of time, some cataclysmic event was necessary to set off the realignment process.

The Great Depression was such an event. Electorally it produced immense national party turnover. From 1933 through 1938, the new majority party had very large majorities in the Congress and it passed a large amount of significant and innovative legislation. In the process, the centers around which political controversy would revolve were permanently changed. That the federal government should play a positive role in managing the economy and in protecting the individual from economic forces beyond his control became, during this period, a major tenet of the majority party's ideology.

The beginnings of and the Democratic party's identification with the new agenda predates the election of Roosevelt. In the years between the beginning of the Depression and the election of a Democratic president, congressional Democrats did attempt to represent the interests of the Depression's victims and, in so doing, were much more willing than the Republicans under Hoover to try new approaches and expand the federal government's responsibilities. One may speculate that during the Hoover years, the Democrats' status as the minority party and the greater prevalence among Democrats of remnants of populist-progress ideology made the Democratic party more likely to respond to the emergency in an innovative way.

These activities by congressional Democrats probably did not have a major effect on the elections of 1930 and 1932. In 1930, the Democratic congressional gains primarily reflected an anti-Hoover vote. Seemingly, in our system, only a president with his "bully pulpit" can produce in the voters' minds an identification between certain policies and his party. In the case of the New Deal much of the major legislation was developed in the executive branch. The congressional Democrats' legislative response to the emergency had taken the form primarily of short-run measures which would alleviate some of the suffering without making any structural changes. The Roosevelt administration developed a number of permanent programs intended to produce structural change. Thus, it was Roosevelt who made the Democratic party the party of the new agenda in the eyes of the electorate. In that process, the congressional party was a willing partner.

The agenda, because it was truly new and proposed nonincremental remedies for the nation's problems, increased the ideological content of American politics. Since the response in the House to the new agenda was partisan, the result was a much clearer ideological distinction between the congressional parties.

In each of the three major issue areas, the process of change took a distinct form. Voting on government management issues was heavily partisan before as well as after the Depression and realignment. With the advent of the New Deal, however, the issues involved became much more ideologically charged. During the New Deal, voting on the dimension constituted an endorsement or repudiation of an activist governmental role in managing the economy. On agricultural policy, a regional alignment was transformed into a partisan one. The social welfare dimension first appeared in the 71st Congress (1929–1930) and was quite clearly a response to the Depression. From the beginning, it evoked a partisan voting response. As the newest of the issue areas, it was, during the late 1920s and through most of the 1930s, least similar to its modern counterpart. In terms of content and voting alignments, the government management and agricultural assistance dimensions took their modern form during the early New Deal. This was not true of the social welfare dimension. Under the pressure of the economic emergency, the need for temporary federal relief was fairly quickly accepted. Permanent programs which implied a continuing government role in protecting – or, depending upon one's perspective, interfering with – the individual were less readily accepted. Some permanent social welfare programs did pass easily without provoking a split in the Democratic party. But by the 75th Congress (1937–1938), many people had concluded that the worst of the Depression was over, and some Democrats began to have second thoughts. It was then that the regional split within the Democratic party in the House began to take its modern shape.

---

[43] V. O. Key, "A Theory of Critical Elections," *Journal of Politics*, 17 (February, 1955), 3–18.

The very real changes which took place should not, however, lead us to ignore the continuities between the pre- and post-realignment periods. Within each party, voting patterns were not immediately transformed. West North Central Republicans were the most progressive element in their party during the 1920s and remained so in the 1930s. Northeastern Republicans maintained their position as the most conservative section. The North/South split within the Democratic party was just beginning to take shape at the end of the period under study. During most of the period, and on most issues, southern Democrats were at least as loyal to the party position as their northern colleagues were.

Sundquist talks of the aftershocks of the New Deal realignment.[44] Such a massive change reverberates through the polity; it takes time before all its effects become manifest. In the Midwest, the rebirth of the Democratic party and the sorting out of the electorate into the two parties along ideological lines eventually led to the extinction of the progressive element in the Republican party. The logic implicit in the New Deal's attempts to provide some security for the individual through governmental action led, in the late 1930s, to a regional split within the Democratic party that was to deepen with the years.

While all the effects of the realignment on the electorate and on the House took many years to become fully manifest, the major outlines of the political agenda which dominated politics at least till the mid-1960s took their shape in the period under study. Thus it is clear that a sufficiently intense environmental stimulus can produce rapid and radical change even in a political system seemingly designed to further only incremental change.

---

[44] Sundquist, *Dynamics,* Chapters 11 and 12.

Corrupt Politicians and Their Electoral Support: Some Experimental Observations*

BARRY S. RUNDQUIST
GERALD S. STROM
University of Illinois at Chicago Circle
JOHN G. PETERS
University of Nebraska-Lincoln

A venerable tradition of scholarship in American politics is concerned with the nature of "political corruption."[1] The consensus in this work seems to be that governmental officials can be expected to engage in corrupt or illegal activities for a variety of reasons. James Q. Wilson summarizes three: (1) the needs and values of particular ethnic groups lead officials identifying with those groups to use their offices illegally to benefit group members with lucrative contracts, jobs, and the like;[2] (2) "Because each branch can and sometimes does paralyze the other, American government is so constituted that it cannot be carried on without corruption. The boss, the machine, the political party, the bagmen – all these operate . . . to concert the action of legally independent branches of government through the exchange of favors";[3] and (3)

---

*An earlier version of this paper was presented at the Annual Meeting of the American Political Science Association, Chicago, Illinois, September, 1974. The authors would like to thank John Ferejohn, John Gardiner, Susan Hansen, Lyman Kellstedt, Benjamin Page, Harry Scoble, Lester Seligman, Donley Studlar, Beatrice Villar, and Susan Welch for critical comments and suggestions relevant to drafts of this paper.

[1] For a useful collection of some of this work, see Arnold J. Heidenheimer, ed., *Political Corruption: Readings in Comparative Analysis* (New York: Holt, Rinehart and Winston, 1970), especially Chapters 6 and 9, and John A. Gardiner and David J. Olson, eds., *Theft of the City* (Bloomington, Indiana: Indiana University Press, 1974).

There is no consensus in this literature on the standards appropriate to determining which political acts are "corrupt." For some, political acts which violate the *public interest* for private interest or gain are corrupt. See, for instance, Arnold A. Rogow and Harold D. Lasswell, *Power, Corruption, and Rectitude* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1963), pp. 132–134. In a similar manner, Berg, Hahn, and Schmidhauser define political corruption as a process which "violates and undermines the norms of the system of public order which is deemed indispensable for the maintenance of political democracy." See Larry L. Berg, Harlan Hahn, and John R. Schmidhauser, *Corruption in the American Political System* (Morristown, New Jersey: General Learning Press, 1976), p. 3. Other authors hold that a political act is corrupt if the public deems it so. See, for example, Arnold J. Heidenheimer's typology of corrupt acts based on this "public opinion" criterion, *Political Corruption*, pp. 26–28. A third standard for assessing corrupt acts relies predominantly on legal norms: those acts are corrupt which violate the rules or norms of public-office holding for personal or private gain. See J. S. Nye, "Corruption and Political Development: A Cost-Benefit Analysis," *American Political Science Review*, 61 (June, 1967), 417–427, and James C. Scott, *Comparative Political Corruption* (Englewood Cliffs, New Jersey: Prentice-Hall, Inc., 1972), pp. 3–5 for an excellent discussion of the definitions of political corruption. In this paper we have chosen this latter criterion and define political corruption as engaging in illegal activity for personal or special interest gain.

[2] James Q. Wilson, "Corruption: The Shame of the States," *The Public Interest*, 2 (Winter, 1966), 28–38, at p. 30, also reprinted in Heidenheimer, pp. 298–306.

[3] Wilson, p. 31. Here Wilson is referring to an argument made in 1904 by Henry Jones Ford in his "Municipal Corruption," *Political Science Quarterly*, 19 (December, 1904), pp. 673–686. This same theme is developed by Edward M. Sait in "Machine, Political," *Encyclopedia of the Social Sciences*, IX (New York: The Macmillan Company, 1933), 657–661; by Arthur F. Bentley, *The Process of Government*, ed. Peter H. Odegard (Cambridge, Massachusetts: The Belknap Press of Harvard University Press, 1967), Chapter II; and by Robert K. Merton, *Social Structure and Social Theory*, 2nd ed. (New York: The Free Press, 1968), p. 73.

"men steal when there is a lot of money lying around loose and no one is watching."[4]

Scholars also seem to agree that officials can often engage in illegal activities without being reprimanded at the polls. Two basic explanations are offered for this. The first claims that public officials can usually conceal their involvement in illegal activities.[5] Moreover, even when they cannot, the contentiousness of American elections is such that voters discount reports that one or more of the candidates are corrupt, assuming instead that allegations of corruption are merely partisan tricks.[6] The other explanation is that officials can trade material benefits (e.g., patronage jobs, the opportunity to gamble) or even appeals to group identification in return for votes.[7]

What these explanations ignore, and in fact direct attention away from, is whether people would support corrupt politicians if they knew they were corrupt or if they were not themselves benefited materially. How, in other words, would voters weight a candidate's corruption along with other things they know about the candidates in an election? In this paper we identify several conditions under which people will knowingly vote for a corrupt candidate, report some experimental results regarding their operation, and suggest some avenues for additional research. This research does not address the frequency of official corruption, nor does it contest the conclusion that politicians are under no electoral constraint to avoid illegal activities except in the sense that a felony conviction deprives them of office.[8] Rather, our focus is on electoral support for corrupt politicians.
nomenon of electoral support for corrupt politicians.

The Ignorant Voter Explanation

There are, of course, reasons why voters would be ignorant or at least uncertain about a candidate's illegal activities. The corrupt candidate usually seeks to conceal this datum from his constituents. Moreover, to the extent that citizens are repelled by political corruption, both candidates in a two-candidate race have similar incentives to accuse each other of being corrupt – even if the charge is false.[9] Because this situation is understood by voters, the credibility of corruption messages during an election – even if true – will normally be quite low. Only when the corruption message comes from a source outside the electoral setting is it likely to be given credibility. For example, Gardiner found that reports by federal law-enforcement agencies about local corruption in one eastern city had a clear impact on three city elections, but similar reporting by local newspapers was discounted in other elections.[10]

This explanation implies that people would oppose corrupt candidates if they knew they were corrupt. Without this assumption, any case in which a voter supports a corrupt politician could be explained in one of two ways. Either the voter is in fact ignorant, or he knows the candidate is corrupt and votes for him anyway. Given no *a priori* reason for assuming that voters always oppose corrupt candidates, and in the absence of independent evidence to that effect, the voter ignorance explanation cannot be accepted as the only possible explanation for voting for a corrupt candidate.

---

[4] Wilson, "Corruption," p. 31.

[5] James Bryce, in analyzing the incidence of corruption in the nineteenth-century Congress, claimed that the "opportunities for private gain are large, the chances of detection small," and because of the relatively short tenure of most members, "the temptation to make hay while the sun shines is all the stronger." See James Bryce, *The American Commonwealth,* Louis M. Hacker, ed. (New York: G. P. Putnam's Sons, 1959), Vol. I, p. 221.

[6] *Ibid.*, p. 197.

[7] For the typology of incentives alluded to here, see Peter B. Clark and James Q. Wilson, "Incentive Systems: A Theory of Organizations," *Administrative Science Quarterly,* 6 (September, 1961), 129–166. Solidary inducements are intangible rewards which accrue from socializing or group identification. Banfield and Wilson, for instance, refer to a precinct captain who offers "personal friendship" in return for a vote. See Edward C. Banfield and James Q. Wilson, *City Politics* (Cambridge, Massachusetts: Harvard University Press, 1963), p. 117. On the use of material incentives to induce electoral support, see Harold F. Gosnell, *Machine Politics: Chicago Model,* 2nd ed. (Chicago: The University of Chicago Press, 1967), chapter IV.

[8] Of course, in principle, politicians, like other citizens, are subject to prosecution for breaking laws, and this threat would seem to constrain their participation in illegal activities. This is not an issue here, since our focus is on the electoral rather than the legal constraint. For a journalistic treatment of the former point, see Jethro K. Liberman, *How the Government Breaks the Law* (New York: Stein and Day, 1974).

[9] This argument follows from the familiar logic of rational party behavior. See Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957), chapter 3.

[10] John A. Gardiner, *The Politics of Corruption: Organized Crime in an American City* (New York: Russell Sage, 1970).

The Material Inducement Explanation

The material inducement explanation suggests an incentive that seems sufficient to produce corruption voting. The problem is that in numerous real-world situations, people seem to support corrupt politicians even when material inducements are absent. A voter may be willing to trade his vote to improve his material well being, but whether or not he does so is contingent upon an explicit exchange between the candidate (or his associates) and the voter. For example, a precinct worker would have to arrange the *quid pro quo* with the voter, or at least the voter would have to understand that he would be rewarded if he voted for the corrupt candidate. Moreover, the inducement would have to be sufficient to change the voter's behavior. The numerous explicit exchanges that would be necessary to control elections would require the presence of large and complex organizations with quite centralized authority structures centered on the corrupt candidates. In this century, such organizations are increasingly rare, not only because people are less willing to respond to material inducements, but because it is expensive for an organization to provide material inducements that are sufficient to change their voting behavior.[11] Inducements based on appeals to common interests are also less effective in providing votes for corrupt politicians than they once were. Banfield and Wilson maintain that television and the changing ethnic character of the inner city have made "friendship" harder to give to prospective voters.[12] For example, precinct captains have to compete with the evening television viewing habits of citizens for their attention, while white representatives of the political machine find it difficult to become "friends" with the increasing proportion of nonwhites in their precincts.[13]

One should not construe from what has been said that voters do not support corrupt politicians – rather, in the absence of sufficient organization the material inducement explanation is insufficient to account for why they do so.

Implicit Trading

If the above explanations do not account for corruption voting, what does? The literature on rational voting in two-candidate elections suggests that whether someone votes for one or the other of the candidates is a function of what he cares about, the intensity of his concern, and whether the candidates take different stands on the things he cares about. Voters may be thought of as evaluating the candidates' positions on items of concern to them, weighting their evaluations according to the strength of their preferences, adding up these weighted evaluations, and then voting for the candidate who receives the highest score.[14]

If candidate corruption is treated like any other component in the voter's choice between two candidates, it follows that there are conditions under which a rational voter would knowingly support corrupt candidates. He would do so if he perceives both candidates as corrupt, or if he decides that a corrupt candidate who is closer to his own preferences on other issues is preferable to a "clean" candidate who is not. In either case, he would vote for the corrupt candidate because the candidate shares or is closer to his own political preferences. This explanation differs from the material inducement theory in that the inducement (the candidate's positions) need not be material, and the exchange between the candidate and the voter may be implicit in the sense that no interpersonal negotiations are necessary. The candidate simply takes positions on things and the voter decides those positions weigh more heavily in the candidate's favor than the knowledge that the candidate is corrupt weighs against him.[15] A corrupt candidate's strategy, therefore, would be to take distinct positions on things many voters care about and thereby arrange as many implicit trades as possible. Hence we refer to this theory as the *trading theory* of corruption voting.[16]

---

[11] Fred I. Greenstein, *The American Party System and the American People* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), pp. 47–49.

[12] Banfield and Wilson, p. 122.

[13] *Ibid.*

[14] This formulation of rational choice is based on similar models developed by economists and psychologists. An early version of the economic model can be found in Otto A. Davis and Melvin J. Hinich, "A Mathematical Model of Policy Formation in a Democratic Society," in *Mathematical Applications in Political Science II*, ed. J. L. Bernd (Dallas: Arnold Foundation, Southern Methodist University Press, 1966), 175–205, see also, Otto A. Davis, Melvin J. Hinich and Peter C. Ordeshook, "An Expository Development of a Mathematical Model of the Electoral Process," *American Political Science Review*, 64 (June, 1970), 426–448. For the psychological formulation, see Martin Fishbein, "A Behavior Theory Approach to the Relations between Beliefs About an Object and the Attitude toward the Object," in *Readings in Attitude Theory and Measurement*, ed. Martin Fishbein (New York: John Wiley and Sons, Inc., 1967), pp. 389–400. This approach is applied to voting behavior in Martin Fishbein and Fred S. Coombs, "Basis for Decision: An Attitudinal Approach toward an Understanding of Voting Behavior," paper presented at the sixty-seventh Annual Meeting of the American Political Science Association, Chicago, Illinois, 1971. A useful synthesis of the economic and psychological choice models is presented in Michael J. Shapiro, "Rational Political Man: A Synthesis of Economic and Social-Psychological Perspectives," *American Political Science Review*, 63 (December, 1969), 1106–1119.

Some Experimental Findings

In order to assess the validity of the above reasoning, we constructed a computer-based experiment to determine whether people always oppose corrupt politicians when they know they are corrupt, and if they do not, whether this reflects implicit trading between candidates and voters. Accomplishing these ends required controlling for the credibility of the corruption message and insuring that only one candidate was viewed as corrupt. We also had to insure the absence of material inducements and the presence of differences between the candidates on items of most concern to the subjects. Our sample was composed of undergraduate students enrolled in introductory political science courses at the University of Illinois during 1972–1973.

In our experiment, subjects received different kinds of information about two candidates in a hypothetical congressional election.[17] On each piece of information, the candidates held opposite positions. The information concerned the candidate's party (Democrat vs. Republican), position on local domestic issues (pro vs. antibusiness), position on President Nixon's Vietnamization policy (hawkish vs. dovish), and relative standing in the polls (ahead, behind). Our subjects were asked to choose between the candidates on the basis of this information in a simulated "opinion poll" (this was not the final vote). Then they were informed that a prominent member of their candidate's own party had withdrawn his endorsement from the subject's most preferred candidate because of alleged illegal activities by the candidate while holding a previous elected position. The message read: "While in the state assembly [their preferred candidate] is alleged to have used his position as chairman of the Public Roads Committee to authorize construction on the new State Route 54 through property his family owned downstate. The sale of the land to the state reportedly brought $1.3 million." Finally, all subjects were asked to "vote" for one or the other of the two candidates. The purpose of the experiment, conducted during the fall and winter of 1972–1973, was to determine whether or not our subjects would switch from the corrupt candidate. (A detailed description of the experiment is included in the Appendix.)

To determine the extent to which our subjects remained with the corrupt candidate, we utilized generalized least squares regression analysis to estimate the following equation:

$$(1)\quad Y = A + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5(X_1X_2) + b_6(X_1X_3) + b_7(X_1X_2X_4) + b_8(X_1X_3X_4) + U,$$

where:

Y = 1 if subject remained with the corrupt candidate, 0 otherwise;

X_1 = 1 if subject received the party affiliation of the candidates, 0 otherwise;

X_2 = 1 if subject received the Vietnam positions of the candidates, 0 otherwise;

---

[15] At a more general level, James C. Scott's theory of the relationship between changing loyalty patterns and the type of inducements political parties must offer for support fit as well with our own notions of individual candidate and voter choice. For instance, Scott suggests that, as the process of economic growth forges new occupational and class loyalties among former supporters, the political party must change the nature of its inducements "to stress policy concerns or ideology." See James C. Scott, "Corruption, Machine Politics, and Political Change," *American Political Science Review*, 63 (December, 1969), 1142–1158, at p. 1146.

[16] Our distinction between implicit and explicit trading is analogous to that between implicit and explicit logrolling suggested by Buchanan and Tullock. Regarding implicit logrolling, they say:

> Here there is *no* formal trading of votes, but an analogous process takes place. The political "entrepreneurs" who offer candidates or programs to the voters make up a complex mixture of policies designed to attract support. In so doing, they keep firmly in mind the fact that the single voter may be so interested in the outcome of a particular issue that he will vote for the one party that supports their issue, although he may be opposed to the party stand on all other issues.

James M. Buchanan and Gordon Tullock, *The Calculus of Consent: Logical Foundations of Constitutional Democracy* (Ann Arbor: The University of Michigan Press, 1962), pp. 134–135.

[17] The experiment was conducted on the PLATO III computer-based education system at the University of Illinois at Urbana-Champaign. A description of this system and the experiment is contained in the Appendix. For a more detailed description of the PLATO system, see D. Alpert and D. Bitzer, "Advances in Computer-Based Education," *Science*, 167 (March, 1970), 1582–1590.

$X_3 = 1$ if subject received the domestic policy positions of the candidates,
0 otherwise;

$X_4 = 1$ if subject received the public opinion polls,
0 otherwise;

and where $X_2X_3 = 0$ and where U is an error term with $E(U) = 0$ by assumption.[18] Estimation of this equation yields a conditional probability model in which, for example, the probability of remaining with the corrupt candidate for subjects who received only the party affiliations of the candidates is A + b (i.e., $X_1 = 1; X_2 = X_3 = X_4 = 0$).

The estimated coefficients are presented in Table 1 with the conditional probabilities of remaining with the corrupt candidate derived from these coefficients presented in Table 2.[19] In examining Table 2, two results are apparent: (1) information about the candidates does make a difference in the probability of remaining with the corrupt candidate, and (2) all types of information are not equal; certain kinds of information induce more voting for the corrupt candidate than do other kinds of information. In particular, we note that the single most powerful piece of information was that on the candidates' Vietnam positions; subjects who received this information had a probability of .44 of voting for the corrupt candidate whereas subjects who received no information had a zero probability of voting for the corrupt candidate. Also note the minimal effect of party information. For subjects who received only the party affiliations of the candidates, the probability of voting for the corrupt candidate was .18, even lower than the probability for those who only received poll information. Moreover, neither party nor domestic policy information seems to make much difference when the two are combined; subjects receiving

---

[18] For descriptions of generalized least squares regression, especially with dummy variables and limited dependent variables, see J. Johnston, *Econometric Methods*, 2nd ed. (New York: McGraw-Hill, 1972), pp. 208–242 and 176–186; and Arthur S. Goldberger, *Econometric Theory* (New York: John Wiley & Sons, Inc., 1964), pp. 251–255.

[19] Initially there were 217 subjects in our experiment but nine were dropped because of missing data on one or more of the questionnaire items. Also, note that the F-levels reported in Table 1 as well as those in Tables 3 and 5 indicate that each variable makes a significant contribution to the explanatory power of the estimated equations. However, as is evident, from an examination of the standard errors reported in these tables, several of the estimated coefficients are not significantly different from zero. Hence, care must be taken not to attribute too much to the exact values of the coefficients.

Table 1. Estimated Coefficients for Initial Conditional Probability Model

Term		Coefficient	Standard Error	F Level
A	(Constant)	.00	.09	
b_1	(Party)	.18	.13	4.51
b_2	(Vietnam)	.44	.14	16.00
b_3	(Domestic)	.37	.13	11.08
b_4	(Poll)	.20	.14	8.02
b_5	(Party-Vietnam)	–.12	.19	3.43
b_6	(Party-Domestic)	–.37	.18	6.35
b_7	(Party-Vietnam-Poll)	–.17	.18	3.92
b_8	(Party-Domestic-Poll)	.03	.19	5.28
(N = 208)				

Note: Multiple R = .37

Table 2. Conditional Probabilities of Remaining with the Corrupt Candidate Estimated from Table 1

Item	
P (Remain/No Information)	$= .00 = A$
P (Remain/Party Information)	$= .18 = A + b_1$
P (Remain/Party, Domestic Information)	$= .18 = A + b_1 + b_3 + b_6$
P (Remain/Poll Information)	$= .20 = A + b_4$
P (Remain/Party, Domestic, Poll Information)	$= .35 = A + b_1 + b_3 + b_4 + b_6 + b_8$
P (Remain/Domestic Information)	$= .37 = A + b_3$
P (Remain/Vietnam Information)	$= .44 = A + b_2$
p (Remain/Party, Vietnam Information)	$= .50 = A + b_1 + b_2 + b_5$
P (Remain/Party, Vietnam, Poll Information)	$= .53 = A = b_1 + b_2 + b_4 + b_5 + b_7$

the combination package voted similarly to subjects who received only party information. Interestingly, however, when poll information is added to this combination, our subjects, for some reason, acted much like those who received only domestic policy information.

In constructing Tables 1 and 2 and in estimating the previous regression equation, we have implicitly assumed that all our subjects would attach identical importance to each of the pieces of information they encountered in the experiment. This assumption is inappropriate, however, since the implicit trading theory assumes that people are likely to feel more strongly about some things than about others. We thus ask: "Does the intensity with which our subjects hold their positions affect their tendency to vote for corrupt politicians?" To answer this question, we asked our subjects to indicate, on a paper-and-pencil questionnaire administered about two weeks before they participated in the experiment, how strongly they felt about President Nixon's Vietnamization policy, about their own party identification, and about local zoning policies. In each case their preferences were registered on a seven-point scale ranging from "strongly favor" to "indifferent" to "strongly oppose." (The party identification item asked whether they were Democrats, Republicans or Independents; and if they were partisan identifiers, how strongly they identified with their party. Independents were asked if they were closer to the Republican or Democratic party.)[20] Consistent with the implicit trading theory, we hypothesized that subjects who felt strongly about party, Vietnam, and local zoning policies (i.e., those in scale positions "1," "2," "6," and "7" on the seven-point scales) would be more likely to stick with the candidate who was closer to their position even if he was corrupt.[21]

To test this hypothesis, we estimated the following equation:

$$(2) \quad Y = A + b_1X_1 + b_2X_2 + b_3X_3 + b_4X_4 + b_5(X_1X_2) + b_6(X_1X_3) + b_7(X_1X_2X_4) + b_8(X_1X_3X_4) + b_9(X_1X_5) + b_{10}(X_2X_6) + b_{11}(X_3X_7) + b_{12}(X_1X_2X_5X_6) + b_{13}(X_1X_3X_5X_7) + U,$$

where Y, X_1, X_2, X_3, X_4, and U are identical to those in equation (1), and where:

X_5 = 1 if subject had an intense party preference,
0 otherwise;

X_6 = 1 if subject had an intense position on Vietnam,
0 otherwise;

X_7 = 1 if subject had an intense position on domestic policy (local zoning),
0 otherwise.

The estimated parameters for this equation, again using the generalized least squares procedure, are presented in Table 3, and the conditional probabilities derived from these parameters are presented in Table 4. Unfortunately, none of our subjects was intensely concerned about local zoning (our domestic policy information), and hence no b_{11} or b_{13} appear in Table 3 and no conditional probabilities are given in Table 4.

As hypothesized, the conditional probabilities of voting for the corrupt candidate were much larger for those who felt intensely about Vietnam than for those who were indifferent on this issue dimension. In particular, those who were intense about Vietnam had a conditional probability of voting for the corrupt candidate that was .34 larger than that for those indifferent on Vietnam. Interestingly, however, intense party identifiers had a *lower* probability of voting for the corrupt candidate than did those without a strong party identification. Party identifiers were more likely to switch and vote for the "clean" candidate when they were informed that their initial choice was corrupt, while independents tended to remain with the corrupt candidate.

Finally, it follows from the implicit trading theory that, if voters have an intense preference for honesty in government, they will be more likely to switch from the corrupt candidate than if this is not the case. Accordingly, we asked our subjects to indicate how intensely they valued the concept of "honesty in government" on a seven-point scale from "strongly for" to "strongly opposed."[22] We then con-

---

[20] This is the standard party identification item used by the Survey Research Center, University of Michigan, for their Presidential Election Studies.

[21] For the purposes of our experiment, we have called respondents "intense" when they recorded their feelings in position 1, 2, 6, or 7 on our seven-point scales:

Strongly favor Strongly oppose
1 : 2 : 3 : 4 : 5 : 6 : 7
"Intense" "Indifferent" "Intense"

This measurement technique is very similar to the one employed by Martin Fishbein to measure the "evaluative aspects of belief statements." See, Fishbein and Coombs, "Basis for Decision," p. 12.

[22] For the "Honesty in Government" question, a respondent was called "intense" if a 6 or 7 was marked on the item.

Table 3. Estimated Coefficients for the Preference Intensity Model

Term		Coefficient	Standard Error	F Level
A	(Constant)	.00	.09	
b_1	(Party)	.21	.14	5.83
b_2	(Vietnam)	.29	.15	11.43
b_3	(Domestic)	.37	.12	13.94
b_4	(Poll)	.20	.13	9.12
b_5	(Praty-Vietnam)	−.14	.19	4.30
b_6	(Party-Domestic)	−.37	.18	7.60
b_7	(Party-Vietnam-Poll)	−.14	.18	5.24
b_8	(Party-Domestic-Poll)	.04	.18	3.89
b_9	(Party Identifier)	−.08	.09	4.71
b_{10}	(Vietnam Intense)	.34	.12	24.34
b_{11}	(Party Identifier and Vietnam Intense)	−.10	.18	6.59

Note: Multiple R = .43

Table 4. Conditional Probability of Voting for a Corrupt Candidate[a]

	Nonintense on Either Party or Vietnam	Intense on Party	Intense on Vietnam	Intense on Both Party and Vietnam
P (Remain/No Information)	.00 (.00)[b]			
P (Remain/Party, Domestic, Poll)	.17 (.18)	.09		
P (Remain/Poll)	.20 (.20)			
P (Remain/Party)	.21 (.18)	.13		
P (Remain/Party, Domestic)	.21 (.18)	.13		
P (Remain/Vietnam)	.29 (.44)		.63	
P (Remain/Party, Vietnam)	.36 (.50)	.38	.70	.63
P (Remain/Domestic)	.37 (.37)			
P (Remain/Party, Vietnam, Poll)	.42 (.52)	.34	.76	.68

[a] Subjects who did not receive a particular piece of information are considered nonintense on that information on the assumption that what they did not know could not affect their behavior.

[b] For comparative purposes, the conditional probabilities of Table 2 are repeated here in parentheses.

structed the following variable:

$X_8 = 1$ if subject is strongly for honesty in government,
0 otherwise;

and re-estimated equation 2 (with b_{14} being the coefficient for X_8). The estimated coefficients are presented in Table 5.

Table 5 shows that, under all of the various information conditions, subjects who intensely preferred honesty in government were less likely to vote for the corrupt candidate. Those with intense preferences for honesty in government had a .08 smaller probability of voting for the corrupt candidate than did subjects who expressed indifference about this concept.

The relatively small number of subjects in this exploratory experiment renders these findings tentative and precludes more detailed analysis. The present findings do seem to lend support, however, to the implicit trading explanation of voting for corrupt politicians. Especially noteworthy is the effect of knowledge of the candidates' issue positions. Information on both the Vietnam and the domestic policy positions of the candidates affected voting for corrupt politicians, while information on the candidates' party seems to have had little effect.

Finally, we should note what happens when the two major assumptions built into our experiment are relaxed. Our theory suggests that an incentive exists for voters to support corrupt politicians even when the message that they are corrupt is credible. If it is not credible, voters should be even more likely to support corrupt politicians, as long as there remains a difference in the candidates' policy positions. But if these latter differences disappear, then even an uncertain corruption message should produce disaffection from the culprit, because the costs to the voter of switching from a "maybe corrupt" candidate to one without the taint of corruption would be minimal. The

Table 5. Estimated Coefficients for Honesty in Government Model

Term		Coefficient	Standard Error	F Level
A	(Constant)	.11	.11	
b_1	(Party)	.14	.14	4.89
b_2	(Vietnam)	.24	.15	9.81
b_3	(Domestic)	.31	.13	8.22
b_4	(Poll)	.14	.14	4.42
b_5	(Party-Vietnam)	–.07	.20	3.36
b_6	(Party-Domestic)	–.30	.18	7.06
b_7	(Party-Vietnam-Poll)	–.08	.18	3.64
b_8	(Party-Domestic-Poll)	.11	.19	13.25
b_9	(Party ID)	–.08	.09	5.46
b_{10}	(Vietnam Intense)	.32	.12	23.34
b_{12}	(Party ID and Vietnam Intense)	–.10	.18	4.01
b_{14}	(Intense on Honesty)	–.08	.07	6.22
(N = 208)				

Note: Multiple R = .44

extent of defection in this case should be a function of how strongly the voter values honesty in government.

Discussion

One implication of the theory that voters trade implicitly with corrupt politicians is that the pool of people with whom corrupt politicians can at least potentially trade is much larger than the number who would vote for them because of material inducement(s). Using only explicit trading and material inducements, corrupt politicians would be restricted to the number of people with whom they could barter, either directly or through lieutenants. Since such trading is costly in organizational resources, this number would be relatively small. Through implicit trading of nonmaterial inducements, however, corrupt politicians can trade with everyone who is aware of the election, has one or more intense preferences and/or is not especially enthusiastic about honesty in government. In the United States, with its practically universal literacy and well-developed media, this would effectively mean most voters. While the number of attempted trades would probably be restricted for strategic reasons (e.g., to form minimum winning coalitions), it would seem that there are so many potential voters with whom corrupt politicians could trade that the latter need not fear electoral reprisal for their indiscretions.

A second implication concerns the conventional wisdom that political machines flourish or perish as a function of the political utility of material inducements. The present analysis suggests that machine politicians who are corrupt may owe their re-election to implicit trading as much as or more than to the distribution of material inducements among voters. Moreover, to the extent that machine politicians rely on implicit trading, they are no different from nonmachine corrupt politicians – an assertion which runs counter to the Progressives' assumption that abolishing machines would decrease the likelihood of corrupt officials being re-elected.[23]

A third inference of the implicit trading theory is that corrupt candidates have even more incentive to take distinct issue positions than do noncorrupt candidates. Contrary to the theory that candidates converge on the issue position occupied by the largest number of voters, the exigencies of American elections (such as the need to amass financial contributions and to obtain primary as well as general election victories) apparently lead candidates to move somewhat away from the modal position. Corruption would seem to accentuate this divergence.[24]

Finally, at least on the surface, the implicit trading theory seems to account for the tendency of voters and others to focus on official corruption during periods of economic problems. When voters' expectations of policy satisfaction are low, office holders are more likely to be held accountable for illegal activity (e.g., the recent ousters of national leaders in Japan, England, West Germany, Italy, and the United States) – albeit not always at the polls.

---

[23] For related speculation, see Edward Banfield, *Political Influence* (New York: The Free Press, 1961), p. 259.

[24] The best statement of the convergence model is given by Downs, *Economic Theory of Democracy.* Multidimensional versions of this model are presented in Otto Davis et al., "Development of a Mathematical Model." For a demonstration of divergence in the 1968 presidential election, see Benjamin I. Page, "Presidential Campaigning, Party Cleavage, and Responsible Parties," unpublished paper, University of Chicago, 1974.

Conclusions

Our purpose has been to examine the notion of electoral support for corrupt politicians. We have isolated three different, though not mutually exclusive, explanations of why people would vote for corrupt politicians and have suggested that the implicit trading explanation accounts for large-scale corruption voting under modern conditions, whereas the other two could not.

In conclusion, this paper is only a first step. The implicit trading theory simply rescues for research the role of political corruption as a component in individual voting decisions, a subject that the ignorant voter theory and the material inducement theory tend to conceal. We leave for future research a host of additional questions about this subject. For example, would some kinds of candidate corruption be weighted more or less heavily than our illegal land sale case? Are people with relatively sophisticated political attitudes (e.g., those with much political information and/or ideologies which link policy, institutional, and candidate information in causal patterns) more likely than others to devalue official corruption? Or do the politically sophisticated value honesty in government more highly or weight this value more heavily in making voting decisions than other people?[25]

In addition, we suggest that these and other questions about the role of candidate corruption in the voters' choice can fruitfully be studied in experimental as well as natural research settings.[26] The exploratory experiment reported on here only begins to tap the full power of this research procedure.

Appendix

To better explain the procedure employed, let us take the fourth treatment (Vietnam Policy/Party Identification/Opinion Poll-Preference/Corruption-Vote) and follow it through the election, displaying the experiment information as it appeared on the subject's TV monitor. After the introductory material and random respondent assignment to the fourth experimental group, the student was introduced to the election and the candidates.

Screen 1:

> THE TWO CANDIDATES IN OUR VOTING SIMULATION ARE RUNNING FOR THIS DISTRICT'S SEAT IN THE U.S. HOUSE OF REPRESENTATIVES, NEITHER CANDIDATE IS THE INCUMBENT, i.e., NEITHER HOLDS THIS OFFICE TODAY.

Screen 2:

> CANDIDATE JONES IS NOW A LAWYER IN UNIVERSITY TOWN AND IS ON THE GOVERNOR'S COMMITTEE ON THE STATE'S ECONOMY.
>
> CANDIDATE BROWN IS ALSO A LAWYER IN UNIVERSITY TOWN AND SERVES AS SPECIAL LEGAL COUNSEL FOR THE STATE BOARD OF EDUCATION.

Care was taken in designing the information contained in Screens 1 and 2 to control any independent effects the information might have on the results. Neutral candidate names, "Jones" and "Brown," were used to control any identification with a "real world" politician. The automatic advantage an incumbent enjoys was neutralized, while differences in candidate career and background information were minimized.

The Vietnam policy message was the next manipulation our respondent received:

Screen 3:

> CANDIDATE JONES WAS A VOCAL OPPONENT OF THE VIETNAMESE WAR SINCE 1966. CANDIDATE BROWN SUPPORTED PRESIDENT NIXON'S VIETNAMIZATION POLICY SINCE 1968.

Policy positions on Vietnamization were chosen for the foreign policy manipulation because of its obvious saliency among the intended audience, college undergraduates, and because of its potential impact on attitude change.

Following the Vietnam policy statements, the respondent received the political party affiliations of the candidates.

Screen 4:

> CANDIDATE JONES IS A DEMOCRAT.
> CANDIDATE BROWN IS A REPUBLICAN.

Next the respondent received information in the form of an opinion poll.

---

[25] James Q. Wilson and Edward Banfield, "Political Ethos Revisited," *American Political Science Review*, 65 (December, 1971), 1048–1062. See also, Herbert McClosky, "Consensus and Ideology in American Politics," *American Political Science Review*, 58 (June, 1964), 361–382, showing that more political leaders than followers advocate clean government.

On the relationship between economic conditions and voting, see Gerald K. Kramer, "Short-Term Fluctuations in U.S. Voting Behavior, 1896–1964," *American Political Science Review*, 65 (March, 1971), 131–143.

[26] For attempts to explain recent trends concerning trust in government, see Arthur H. Miller, "Political Issues and Trust in Government: 1964–1970," *American Political Science Review*, 68 (September, 1974), 951–972, and Jack Citrin, "Comment: The Political Relevance of Trust in Government," *American Political Science Review*, 68 (September, 1974), 973–988.

Screen 5:

> THE INDIVIDUALS WHO PRECEDED YOU IN THIS EXPERIMENT GAVE THE FOLLOWING INDICATION OF THEIR CANDIDATE PREFERENCE AT THIS POINT:
>
> | | |
> |---|---|
> | CANDIDATE BROWN | 45% |
> | CANDIDATE JONES | 55% |

Although the students were told that the poll results were based on the responses of previous subjects, in fact the percentages reported for the two candidates were randomly generated with the only contraints being that each respondent viewed different percentages and that they add to 100%.

The presentation of the opinion poll results concluded the "information" component of the experiment. On the basis of this information, subjects assigned to experimental route 4 were then asked which of the two candidates they preferred so far in the election.

Screen 6:

> TO UPDATE OUR OPINION POLL, PLEASE INDICATE YOUR PREFERENCE OF THE TWO CANDIDATES AT THIS POINT IN THE ELECTION.
>
> CANDIDATE JONES CANDIDATE BROWN
>
> PLEASE TYPE IN THE LAST NAME OF THE CANDIDATE OF YOUR CHOICE.

Whichever candidate was preferred by the subject, he was informed of that choice's corrupt behavior. Let us assume that in Screen 6 above, our student preferred Candidate Jones.

Screen 7:

> THE DEMOCRATIC SENATOR FROM THE STATE HAS JUST ANNOUNCED THAT HE IS WITHDRAWING HIS ENDORSEMENT FOR CANDIDATE JONES. HE EXPLAINED HIS ACTIONS AS FOLLOWS: "ALTHOUGH CANDIDATE JONES SHOULD BE PRESUMED TO BE INNOCENT UNTIL THE CRIMINAL SUIT BROUGHT AGAINST HIM HAS BEEN TRIED IN A COURT OF LAW, I FEEL IT IS IN THE BEST INTERESTS OF THIS STATE THAT CANDIDATE JONES TERMINATE HIS STATE RELATED ACTIVITIES."

Screen 8:

> WHILE IN THE STATE ASSEMBLY, CANDIDATE JONES IS ALLEGED TO HAVE USED HIS POSITION AS CHAIRMAN OF THE PUBLIC ROADS COMMITTEE TO AUTHORIZE CONSTRUCTION OF THE NEW STATE ROUTE 54 THROUGH PROPERTY HIS FAMILY OWNED DOWNSTATE. THE SALE OF THE LAND TO THE STATE REPORTEDLY BROUGHT 1.5 MILLION DOLLARS.

If Candidate Brown was chosen, the source was a Republican governor; otherwise the message was the same. After receipt of the information that his preferred candidate was corrupt, the respondent was asked to cast his final ballot. This concluded the experiment.

Screen 9:

> OFFICIAL BALLOT
>
> PLEASE VOTE FOR THE CANDIDATE OF YOUR CHOICE BY TYPING IN HIS LAST NAME.
>
> CANDIDATE JONES (D)CANDIDATE BROWN (R)

The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory*

ABRAHAM H. MILLER
University of Cincinnati

LOUIS H. BOLCE
Fordham University, Lincoln Center

and

MARK HALLIGAN
Northwestern University Law School

Introduction

In the intellectual quest for a systematic understanding of the etiology of political and collective violence, relative deprivation theory has emerged as a prevalent explanatory device. One form of relative deprivation theory has received particularly strong scholarly attention through theoretical elaboration and empirical application to a variety of significant episodes of political and collective violence. This form is the progressive variety of relative deprivation and is most commonly referred to as the "J-curve."[1]

The J-curve resulted from an attempt by James C. Davies to reconcile what he perceived as two antithetical explanations of the phenomenon of revolution. According to Davies, two of the most famous students of revolution, Karl Marx and Alexis de Tocqueville, had arrived at completely opposite conclusions about the causes of revolution. Drawing on Marx's work in *The Communist Manifesto*, Davies argued that Marx perceived revolutions as most likely to occur when things get worse, when the misery of the proletariate increased relative to the economic standard of living of the bourgeoise. In contrast, Tocqueville had advanced the theory that revolutions occurred when a previously oppressive regime released the yoke and created expectations it could not fulfill.[2]

Davies attempted to reconcile these divergent explanations by arguing that each of these perspicacious students of revolution had accurately rendered part of the picture of revolu-

---

*This research was funded by the University of Concinnati Research Council through an award to Professor Miller. The data were obtained from the Inter-University Consortium for Political Research through the facilities of the University of Cincinnati Behavioral Sciences Laboratory. The authors alone are responsible for the use and interpretation of the data. At the time of this study, Louis H. Bolce was a Charles Phelps Taft Graduate Fellow in Political Science and Mark Halligan was an Undergraduate Fellow in Political Science. William Klecka and Alfred Tuchfarber, University of Cincinnati, Ted Robert Gurr, Northwestern University, and four anonymous reviewers contributed useful and insightful suggestions which are gratefully acknowledged.

[1] James C. Davies, "Toward a Theory of Revolution," in *Studies in Social Movements,* ed. Barry McLaughlin (New York: The Free Press, 1969), pp. 85–109. Hardly an anthology on revolution or social movements exists without including this work. James A. Geschwender, "Explorations in the Theory of Social Movements and Revolutions," *Social Forces* 47, 2 (December, 1968), 127–135, has attempted to elaborate the theory in terms of cognitive dissonance.

[2] Alexis de Tocqueville, *The Old Regime and the French Revolution*, translated by Stuart Gilbert (Garden City, N.J.: Doubleday Anchor, 1955), p. 177. Tocqueville's observation that those parts of France which experienced the greatest degree of positive change also showed the greatest popular discontent is sometimes referred to as Tocqueville's paradox. Two different explanations of the paradox exist. The most common, and the one used by Davies and in this research, is the rising expectations thesis. A less well-known interpretation is referred to as "the present value of the past." This argues that present achievement is devaluated by the perception of past costs. See, in this regard, Charles Wolf, Jr., "The Present Value of the Past," mimeo (Santa Monica: The Rand Corporation, 1969).

tion, but not the total picture. The complete depiction of the causes of revolution awaited the fashioning of both components into a single entity, which Davies engagingly accomplished. Davies argued that revolutions occur when a period of progress is followed by a period of sharp reversal and decline.

This rise-and-decline phenomenon can be read as: first Tocqueville then Marx. As Davies notes: "Revolutions are most likely to occur when a prolonged period of objective economic and social development is followed by a short period of sharp reversal. The all-important effect on the minds of people in a particular society is to produce, during the former period, an expectation of continued ability to satisfy needs — which continue to rise — and, during the latter, a mental state of anxiety and frustration when manifest reality breaks away from anticipated reality."[3] The theory is depicted in Figure 1 below.

At a conceptual level the theory has a great deal of appeal. It is the type of theory that is very attractive to social scientists. It reconciles apparent contradictions in the work of two leading theorists and creates a parsimonious explanatory structure. In the search for conceptual simplicity, clarity and phenomenological regularity, it yields, at a minimum, heuristic success and beyond that has had ostensible empirical corroboration.

From the perspective of empirical theory, however, there are several primary empirical, methodological, and conceptual problems inherent in the structure and application of the J-curve. These problems have consequences for the J-curve theory specifically and relative deprivation theory generally.

It is the purpose of this research to examine some of these problems in the context of the application of the J-curve to the black urban riots. Although the primary objective of the research is to test the J-curve theory, consideration will also be given to the substantive

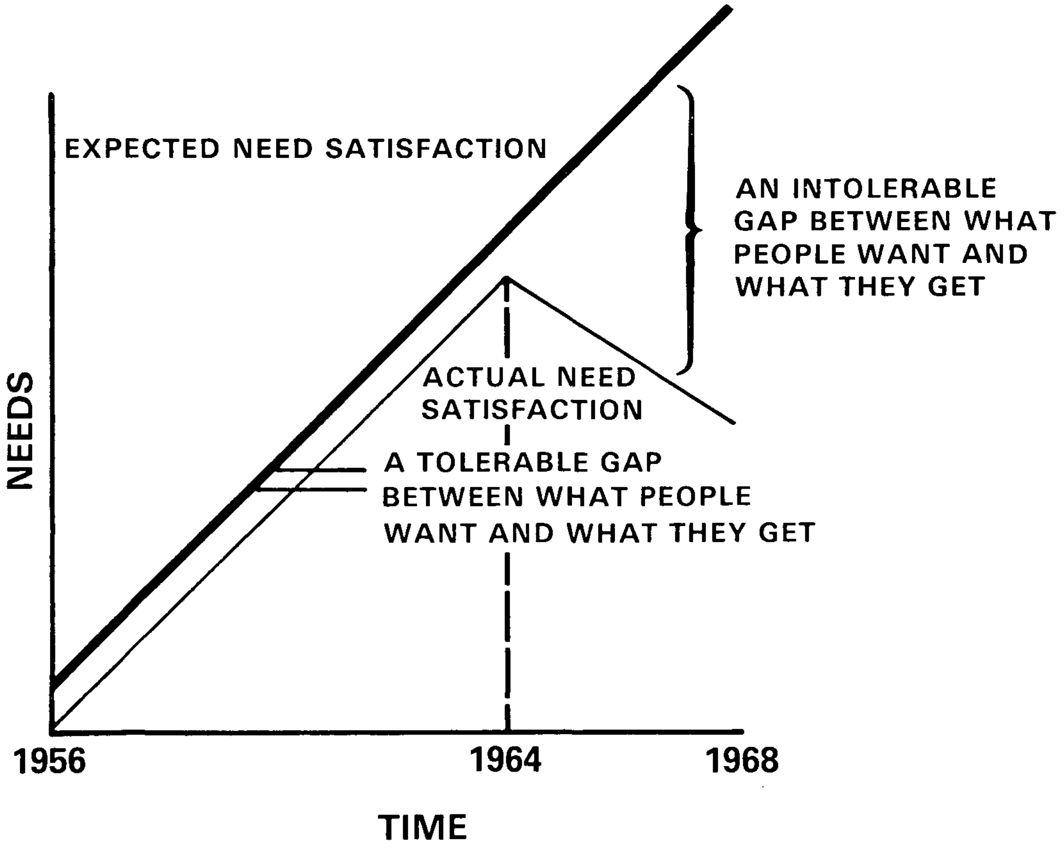

EXPECTED NEED SATISFACTION
AN INTOLERABLE GAP BETWEEN WHAT PEOPLE WANT AND WHAT THEY GET
ACTUAL NEED SATISFACTION
A TOLERABLE GAP BETWEEN WHAT PEOPLE WANT AND WHAT THEY GET
NEEDS
1956
1964
1968
TIME

Figure 1.
Pictorial Representation of Davies's J-Curve
(Adapted from James C. Davies, "The J-Curve. . . ," p. 691)

---

[3] Davies, "Toward a Theory of Revolution," p. 86.

implications of the analysis as it affects our understanding of the riots.

Seemingly, a number of different events could have been chosen to examine the theoretical and empirical application of the J-curve. The black urban riots were chosen, in part, because Davies, himself, has applied his theory to this set of events.[4] Consequently, it is possible to follow his operationalizations, avoiding questions of operational comparability, and to assess the theory in an appropriate empirical setting. Moreover, one of the major criticisms which we level against the application of the J-curve, and most relative deprivation theories, is the problem of reductionism, sometimes referred to in the philosophy of science as the level-of-analysis issue: the validity of using data and concepts developed at one level of analysis to test theories formulated at a different level.[5] (This problem is comparable to the one raised by W. S. Robinson's[6] criticism of the use of aggregate correlations to draw inferences concerning the behavior of individuals.) A variety of data exists for the study of the black urban riots, making it possible to conduct analytical comparisons and reconstruct operationalizations at various analytical levels. As a result, the level-of-analysis problem can be empirically assessed rather than simply noted as a caveat affecting the research.

The Black Urban Riots and Relative Deprivation Theory

In his review of the sociological literature on the black urban riots, Clark McPhail[7] notes that research designs based on relative deprivation theory have played a prominent role in the attempts to explain these events. The results of these studies, in making a case for relative deprivation as a primary explanation of the riots, have been largely disappointing, despite some rather sophisticated and well-conceived methodological procedures.[8] As McPhail notes ". . . there is considerable reason for rejecting the sociological and popular cliche that absolute or relative deprivation and the ensuing frustration or discontent or despair is the root cause of rebellion."[9] On the basis of his observations, McPhail suggests reconsideration of Gurr's assertion that relative deprivation is ". . . as fundamental to understanding civil strife as the law of gravity is to atmospheric physics. . . ."[10]

Davies, in his assessment of the urban riots, strikes a more confident note: "The Negro rebellion appears to have been preceded by the same J-curve of expectations that are first gratified and then frustrated."[11] In contrast to Davies's work, the studies cited by McPhail are generally based on analyses of socioeconomic and political characteristics of riot versus nonriot cities. Relative deprivation is usually measured in these studies by comparing various socioeconomic, census data characteristics of the black community with those of the white community. The underlying logic is that the riot cities should manifest greater disparities along these dimensions than the nonriot cities.

Davies's work is based on population characteristics; nonetheless, these are also aggregates.

---

[4] James C. Davies, "The J-Curve of Rising and Declining Satisfactions as a Cause of Some Great Revolutions and a Contained Rebellion," in *The History of Violence in America*, ed. Hugh Davis Graham and Ted Robert Gurr (New York: Bantam Books, 1970), pp. 690–731.

[5] In the philosophy of science this problem is sometimes alluded to as a problem in reductionism. See, for example, May Brodbeck, "Methodological Individualism: Definition and Reduction," in *Readings in the Philosophy of the Social Sciences*, ed. May Brodbeck (New York: The MacMillian Co., 1968).

[6] W. S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, 15 (June, 1950), 351–357. See also: L. A. Goodman, "Some Alternatives to Ecological Correlation," *American Journal of Sociology*, 64 (May, 1959), 610–625; O. D. Duncan and B. Davis, "An Alternative to Ecological Correlation," *American Sociological Review*, 18 (December, 1953), 665–666. O. D. Duncan, R. P. Cuzzort, and B. Duncan, *Statistical Geography* (Glencoe: The Free Press, 1961), note, as we do, that the problem arises wherever the units of analysis are changed. The change need not be from one distinct type of unit (e.g., individual) to a different type (e.g., group).

[7] Clark McPhail, "Civil Disorder Participation: A Critical Examination of Recent Research," *American Sociological Review*, 36 (December, 1971), 1058–1073.

[8] See, in this regard, Milton Bloombaum, "The Conditions Underlying Race Riots as Portrayed by Multidimensional Scalogram Analysis: A Reanalysis of Lieberson and Silverman's Data," *American Sociological Review*, 33 (February, 1968), 76–91; Bryan T. Downes, "Social Characteristics of Riot Cities: A Comparative Study," *Social Science Quarterly*, 49 (December, 1968), 504–520; Stanley Lieberson and Arnold Silverman, "The Precipitants and Underlying Conditions of Race Riots," *American Sociological Review*, 30 (December, 1965), 887–898; Seymour Spilerman, "The Causes of Racial Disturbances: A Comparison of Alternative Explanations," *American Sociological Review*, 35 (August, 1970), 627–649; and Jules Wanderer, "An Index of Riot Severity and Some Correlates," *American Journal of Sociology*, 74 (March, 1969), 500–505.

[9] McPhail, "Civil Disorder Participation," p. 1064.

[10] Ted Robert Gurr, "Urban Disorder: Perspectives From the Comparative Study of Civil Strife," in *Riots and Rebellion: Civil Violence in the Urban Community*, ed. Louis H. Mascotti and Don R. Bowen (Beverly Hills: Sage Publications, 1968), p. 52.

[11] Davies, "The J-curve," p. 717.

The index for measuring relative deprivation is constructed by dividing family income by average years of schooling for the total and nonwhite populations. The ratio of the resulting nonwhite quotient to the white quotient is described as a measure of nonwhite satisfaction. The nonwhite satisfaction ratio is then plotted from 1940 to 1967 to determine whether a J-curve can be traced for the period of the riots. The numeric data and plot are presented in Table 1 and Figure 2 below.

Although in some remote fashion Figure 2 might be construed to illustrate a J-curve, the reversal in need fulfillment takes place during the wrong years – predicting a peak of unrest in the mid-1950s. Despite this, the data from the black urban riots are not seen as disconfirming the theory. Davies simply argues that the riots needed the exacerbation of southern violence a decade later to produce the aggression that began with the frustration demonstrated by the ratio of education to income.[12]

This eclectic interpretation of the data raises issues concerning the validity of the J-curve as an explanation of the urban riots. Moreover, Davies's logic, as that of his compeers in

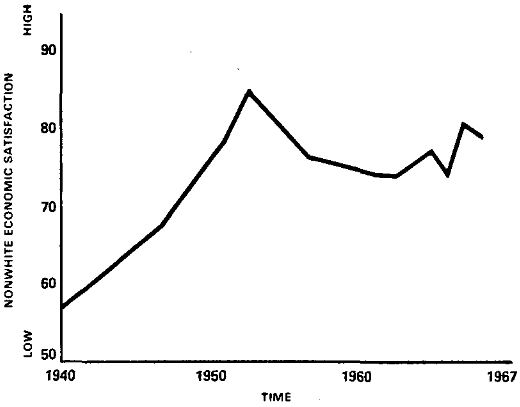


Figure 2.
Index of Nonwhite Economic Satisfaction
(Adapted from James C. Davies, "The J-Curve. . . ," p. 724)

Table 1. Davies's Origin and Time Sequence of Black Frustration (adapted from James C. Davies, "The J-Curve. . . ," p. 726)

	Average Family Income Divided by Average Years of Schooling		Nonwhite Satisfaction	Nonwhite Frustration
Year	Total Population	Nonwhite Population	Column 2 as % of Column 1	Column 3 from 100%
1940	$\frac{\$1231}{8.4} = 146.3$	$\frac{\$\ 489}{5.8} = 84.3$	57.5	42.5
1947	$\frac{3031}{9.0} = 336.8$	$\frac{1614}{6.9} = 233.9$	69.4	30.6
1950	$\frac{3319}{9.3} = 356.9$	$\frac{1869}{6.8} = 274.9$	77.0	23.0
1952	$\frac{3890}{10.1} = 385.1$	$\frac{2338}{7.1} = 329.3$	85.8	14.5
1957	$\frac{4971}{10.6} = 469.0$	$\frac{2764}{7.7} = 359.0$	76.5	23.5
1960	$\frac{5620}{10.6} = 530.2$	$\frac{3233}{8.2} = 394.3$	74.4	25.6
1962	$\frac{5956}{11.4} = 522.5$	$\frac{3330}{8.6} = 387.2$	74.1	25.9
1964	$\frac{6559}{11.7} = 560.6$	$\frac{3839}{8.9} = 431.3$	77.0	23.0
1965	$\frac{6951}{11.8} = 589.6$	$\frac{3995}{9.0} = 443.8$	75.3	23.7
1966	$\frac{7436}{12.0} = 619.7$	$\frac{4628}{9.2} = 503.0$	81.2	18.8
1967	a	a	78.9	20.2

[a] Absent on original.

---

[12] *Ibid.*, p. 724.

sociology cited by McPhail,[13] is not congruent with the empirical and theoretical requirements of relative deprivation theory. The requisite level of analysis has been violated in the assertion that the presence of aggregate, nonperceptual characteristics ostensibly related to black frustration is an appropriate indication that individual black people are indeed experiencing frustration.

The importance of this logical gap is best understood by looking at the psychological basis of relative deprivation theory. Relative deprivation theory claims theoretical descent from the psychological research of John Dollard and his Yale colleagues.[14] The Yale team established what has come to be known as the frustration-aggression hypothesis. This hypothesis, as it has evolved, proposes that frustration produces various responses, one of which is aggression, and if nonaggressive responses do not relieve the frustration, the probability of an aggressive response increases.[15]

Relative deprivation theory has built upon this hypothesis in viewing relative deprivation as a major component of the chain of events leading to aggression. It is this deprivation that produces frustration; frustration produces aggression; aggression produces civil violence; and civil violence can be translated into political violence.[16]

The language of relative deprivation theory is replete with reference to this psychological foundation. As Davies notes: "Political stability and instability are ultimately dependent on a state of mind, a mood, in a society. Satisfied or apathetic people who are poor in goods, status, and power can remain politically quiet and their opposites can revolt. . . ."[17]

Relative deprivation is, thus, an individual experience, and, despite objective, aggregate indicators, relative deprivation can only take place if people perceive themselves to be deprived. Economic indicators may demonstrate that the availability of goods, status, or power may be low or may have diminished from some previous level, but this does not demonstrate that people perceive themselves to have been deprived and are consequently discontent.

With the exception of work by Grofman and Muller,[18] relative deprivation theorists generally use the language and theoretical foundation of individual data but perform their empirical testing with aggregate social and economic indicators. The chasm that separates objective societal conditions from subjective perceptions seems to have trapped many researchers who have made enthusiastic and confident assertions about the perceptual basis of individual behavior from theoretically remote aggregate indicators.[19]

Given the above considerations, how would Davies's index appear if he had obtained individual-level data (overlooking momentarily the entire question of perceptual vs. nonperceptual data and simply looking at the difference in the unit of analysis)? From our individual data, taken from the Michigan Survey Research Center election studies from 1956–1968, Davies's operationalizations were replicated and his index was reconstructed. The data are displayed below for both whites and blacks and by region in Table 2 and Figure 3.

The above data and Figure 3 do not demonstrate anything as parsimonious as a J-curve. The index of black frustration varies without consistent pattern, and the overall impression it imparts is that the differences between blacks and whites are so trivial as to be substantively inconsequential. In four out of the seven years analyzed, blacks, contrary to the interpretation originally derived from Davies's data, receive a slightly higher income return for years of

---

[13] McPhail, "Civil Disorder Participation," p. 1059.

[14] John Dollard et al., *Frustration and Aggression* (New Haven: Yale University Press, 1939).

[15] This statement is based on the elaboration of Dollard's work by Neal E. Miller et al., "The Frustration-Aggression Hypothesis," *Psychological Review*, 48 (July, 1941), 337–342. See also: Leonard Berkowitz, *Aggression: A Social Psychological Analysis* (New York: McGraw Hill, 1962), in *Advances in Experimental Social Psychology*, Volume 2, ed. Leonard Berkowitz (New York: Academic Press, 1965).

[16] Gurr, *Why Men Rebel* (Princeton: Princeton University Press, paperback, 1970).

[17] Davies, "The J-curve," pp. 86–87.

[18] Bernard N. Grofman and Edward N. Muller, "The Strange Case of Relative Gratification and Potential for Political Violence: The V-Curve Hypothesis," *The American Political Science Review*, 67 (August, 1973), 514–539.

[19] In addition to the sociological literature noted above in footnote 8, see, for example: Ivo K. Feierabend, R. L. Feierabend, and B. A. Nesvold, "Social Change and Political Violence: Cross-National Patterns," in *Anger, Violence and Politics: Theories and Research*, ed. Ivo K. Feierabend, Rosalind L. Feierabend, and Ted Robert Gurr (Englewood Cliffs, N.J.: Prentice Hall, Ivo K. Feierabend, Rosalind L. Feierabend, "Aggressive Behaviors within Polities 1948–1962: A Cross-National Study," *Journal of Conflict Resolution*, 10 (September, 1966), 249–271; Raymond Tanter and Manus Midlarsky, "A Theory of Revolution," *Journal of Conflict Resolution*, 11 (September, 1967), 264–280; and Ted Robert Gurr, "A Causal Model of Civil Strife: Using New Indices," in *Anger, Violence and Politics*, ed. I. K. Feierabend et al. Much to his credit, of all the authors cited, Gurr, *Ibid.*, p. 210, is the only one to acknowledge the level-of-analysis problem.

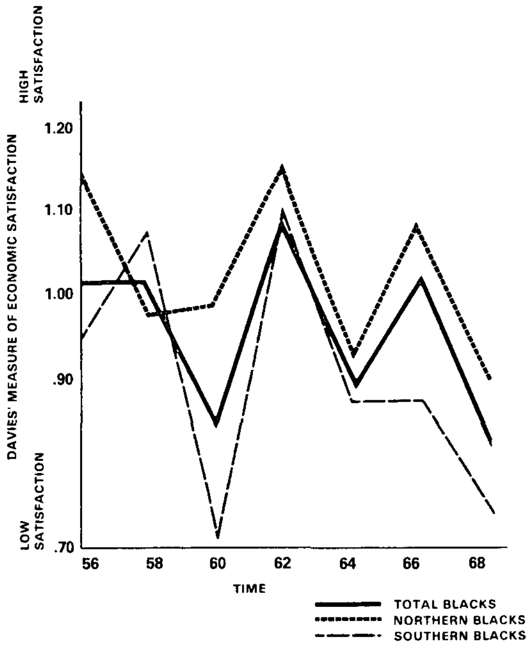


Figure 3.
Index of Black Economic Satisfaction from Individual Data

education than do whites. This is not as surprising a finding as it first appears (as we will demonstrate below), but is rather a penetrating commentary on the inappropriateness of the index as a measure of black frustration. Although income generally varies with education, it is those with lower education who receive a higher per annum income return for unit of education. Higher education yields a considerable amount more money over a working lifetime. On a per annum basis, however, it takes many increments in years of schooling to produce modest increments in income. Black people tend to cluster at the bottom end of the education distribution where the ratio of income to education is highest. As a result, black people have higher income to education ratios. This is not to argue the absurd position that whites are frustrated relative to blacks. Rather, what is discerned from this finding is the futility of attempting to measure a perceptual concept such as frustration with aggregate, objective indicators. Moreover, it is solely because Davies's index is based on a ratio of two aggregates that the inappropriateness of the index is not readily discernible. The manner in which the aggregations conceal what is occurring can be readily observed in Table 3 below. In Table 3 the level of education is controlled by partition design, and the data indicate that it is the lower levels of education that yield the best ratio of income to years of schooling.

Having demonstrated the inappropriateness of Davies's index for testing the J-curve theory in the context of the urban riots, we attempted a refinement of the operationalization. Given relative deprivation theorists' reliance on objective data, we were interested in whether changing the unit of analysis, from aggregate to individual, and refining the measurement might yield something similar to a J-curve or other theoretically parsimonious configuration. Also, we were interested in observing if more sophisticated operationalizations of the objective but individual-level data would correspond to our perceptual measures (see below).

To accomplish this refinement in measurement, two objective measures based on the regression of income on education, after parti-

Table 2. Individual-Level Objective Data Measures of Davies's Operationalization of Black Economic Satisfaction[a]

	Average of Each Head's Income Score Divided by Each Head's Years of Schooling Score[b]						Black Satisfaction Ratio of Column 2 to Column 1		
Year	Total White	North White	South White	Total Black	North Black	South Black	Total Black	North Black	South Black
1956	1.68	1.72	1.55	1.72	1.99	1.47	1.02	1.15	.95
1958	1.62	1.62	1.61	1.66	1.60	1.74	1.02	.98	1.08
1960	.55	.55	.51	.47	.54	.40	.85	.99	.71
1962	1.84	1.91	1.70	2.01	2.20	1.88	1.09	1.15	1.10
1964	.59	.59	.59	.53	.56	.52	.90	.93	.88
1966	.54	.55	.52	.55	.59	.46	1.01	1.08	.88
1968	.70	.70	.70	.58	.63	.52	.83	.90	.75

[a] Scores are not translatable into dollar terms as they reflect the relationship between measurements on coding categories. Codes are not always comparable from year to year, but this is of no consequence since we are interested in the differences in any one year between whites and blacks and how these differences vary longitudinally. Column 3, the nonwhite satisfaction index, multiplied by 100 is similar to the nonwhite satisfaction index in Table 1.

[b] Only heads of household were used.

Table 3. Education by Income Quotients with Level of Education, Race, and Region Controlled[a]

	All Educational Groups[b]	Grade School	High School	College and Above
Total white	1.84[c]	3.10	1.53	0.86
North white	1.91	3.13	1.57	0.85
South white	1.70	3.00	1.43	0.88
Total black	2.01	2.67	1.08	0.85
North black	2.20	3.77	1.11	0.98
South black	1.87	2.17	1.05	0.55

[a] Data are from 1962 only since the issue is a cross-sectional one and not a longitudinal one. The year was chosen at random.

[b] Heads of household only.

[c] Score is the average of each head's income score divided by each head's year of schooling score.

tioning race and region, were created. One of these is called the "differ variable," and it computes the difference between predicted income for blacks subtracted from predicted income for whites, when income is regressed against education. The second and more elaborate measure is called the "discrim variable," and it is constructed by subtracting a black individual's actual income from the income he would expect if he were white.[20] This creates two objective individual-level measures of black relative deprivation. The results of these operationalizations are displayed below in Table 4 and Figures 4 and 5.

Regardless of operational differences, the trends for Figures 4 and 5 demonstrate the same time-series pattern. In neither case do we observe a J-curve. It is also important to note the divergences between the black population in each of the two regions. The national configuration is often an averaging of radically different trends between northern blacks and southern blacks. (This demonstrates the importance of looking at specific groups rather than treating the society as a monolith, or failing to make important distinctions within

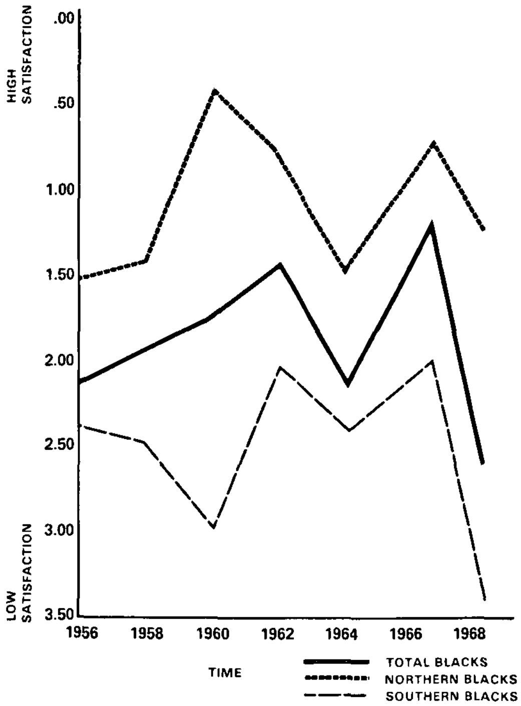


Figure 4.
Differ Operationalization of
Black Economic Satisfaction

---

[20] This latter operationalizations was suggested, in part, by Otis D. Duncan, "Discrimination Against Negroes," in *Social Intelligence for America's Future,* ed. Bertram M. Gross (Boston: Allyn and Bacon, 1969). Discrimination is measured by taking the slope and intercept of the regression equation for whites and plugging in the observed independent variable (education) scores for blacks. The resultant predicted dependent variable (income) scores for blacks are subtracted from the blacks' actual score on the dependent variable. The residual [Yp (if respondent were white) – Ya] is the measure of deprivation.

A wholly individual measure, based on differences between actual and predicted income, was desired for the "differ" operationalization. Such a measure would have been very similar to the one used for the "discrim" operationalization. In the calculation of measures of association, the type of measure is feasible. In calculating summary measures for the purpose of obtaining averages for blacks, this is not possible. In the case of any individual black, the "differ" is the residual between predicted income and actual income. The average of the residuals $[\Sigma(\hat{Y}i-Ya)/N]$ however, always equals zero. The operationalization is meaningful for an individual black, but it aggregates to zero when cumulated for the group. To provide an analogous measure for the purpose of calculating an average, the average difference of predicted income for whites and blacks is calculated, given each groups' respective regression equation. Thus the average score is based on $\Sigma\hat{Y}i/N$ for blacks subtracted from $\Sigma\hat{Y}i/N$ for whites.

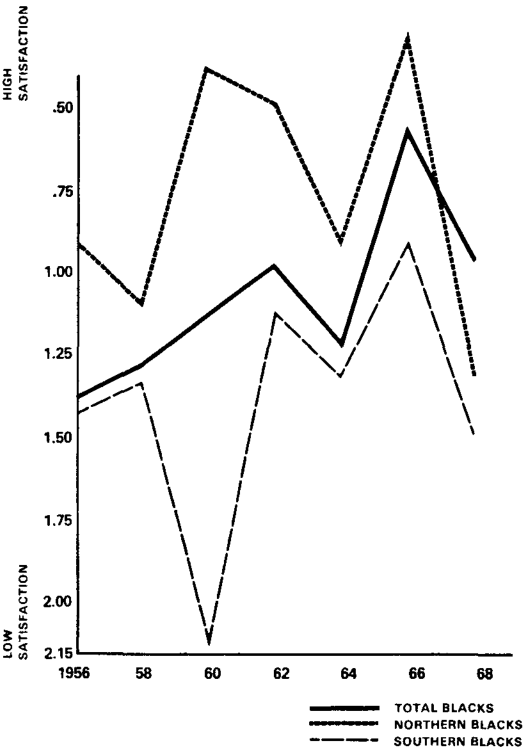


Figure 5.
Discrim Operationalization of Black Economic Satisfaction

groups, a failure often necessitated by aggregate analysis.)

Being concerned with the urban riots, a northern phenomenon, we must focus specifically on the trends for northern blacks. Although the data (see Figures 4 and 5) do not follow the J-curve pattern, from 1958–1960 there is some indication of rising need fulfillment, and from 1960–1964 there is some indication of the loss of previous gains. But from 1964 to 1966 there is another rise followed by a decline in 1966 through 1968. The J-curve interpretation is rejected because the rising need-fulfillment pattern is only of two year's duration, hardly commensurate with the long-term increases required by the theory; and the overall pattern, despite segments of rise and decline, is not commensurate with the J-curve.

What the data do reveal most dramatically is a series of extreme fluctuations and changes. There is neither a constant trend of improvement, deterioration, nor stability but a series of positive surges and negative declines. This pattern of instability may be causally related to the urban riots. Certainly, a wide variety of research[21] has argued that political violence is a function of the ambiguity and dissonance associated with social change, even when the pattern of change is positive. Before pursuing in any detail these implications from the objective data, it is necessary to first determine if these patterns can be validated by perceptual data – especially given the methodological and conceptual issues which were raised with regard to the use of even individual-level, objective data. It is to this issue that we now turn.

The Correspondence Between Objective and Perceptual Indicators of Relative Deprivation

The differ and discrim operationalizations are a marked improvement over the kinds of

---

[21] Murray Edelman, *Politics as Symbolic Action: Mass Arousal and Quiescence* (Chicago: Markham, 1971); Grofman and Muller, "The Strange Case of Relative Gratification and Potential for Political Violence"; Feierabend et al., "Social Change"; Seymour Martin Lipset, *Political Man* (Garden City: Anchor Books, 1963), esp., pp. 54–55; Michael Aiken et al.,

Table 4. Individual-Level, Regression-Based Objective Measures of Black Economic Satisfaction

	Differ Operationalization			Discrim Operationalization		
Year	Total Blacks	North Blacks	South Blacks	Total Blacks	North Blacks	South Blacks
1956	2.12[a]	1.50	2.37	1.35[b]	0.87	1.40
1958	1.95	1.40	2.47	1.26	1.07	1.31
1960	1.77	0.43	2.95	1.10	0.36	2.12
1962	1.53	0.81	2.03	0.96	0.53	1.10
1964	2.15	1.46	2.40	1.21	0.91	1.31
1966	1.25	0.72	1.99	0.55	0.29	0.91
1968	2.50	1.35	3.40	1.31	0.94	1.47

[a] The differ operationalization measures the difference of white predicted income minus black predicted income. The differ is computed by taking Σ Yi/N for whites minus Σ Yi/N for blacks. The operationalization is modified when based on noncumulated individual scores. See Footnote 20.

[b] The discrim operationalization is measured by taking the average of the residual between the predicted income for blacks (assuming they are white, by taking the predictions based on the intercept and slope of the white regression equation) minus black actual income.

information and measures generally available to relative deprivation researchers, since these are based on individual rather than aggregate measures. There still, however, remains the very important question of whether or not even individual-based objective measures can be substituted for perceptual measures.

This issue is far more complex than it initially appears and must be broken down into two component issues. First, there is the question of whether valid substitutions can be made on a data-point-by-data-point basis. Second, it is conceivable that the general pattern resulting from different types of data might be quite similar, although the points themselves might be unrelated for any given year. (One explanation for this phenomenon is the time gap between an event and its psychological recognition.) In such instances the general theoretical interpretation provided by the two data sets might be the same, despite the specific temporal differences.

The preceptual indicators are based on three items, dealing with financial need satisfactions, from the 1956 through 1968 Michigan election studies. These items are: (1) whether the respondent (R) perceived his financial situation as being better or worse than the year before; (2) R's expectation of his future financial prospects; and (3) R's perception of his financial satisfaction. Each of these perceptual indicators was correlated with the three preceding individual-level, objective measures of relative deprivation: (1) the reoperationalized Davies index; (2) the differ operationalization and (3) the discrim operationalization. The correlation matrix for the six items – three perceptual and three objective – is displayed in Table 5 below.

Some of the data in the above matrix are presented solely for informational purposes, for we did not anticipate that all the interrelation-

Table 5. Intercorrelation Matrix of Objective and Perceptual Data

Year	R's Perception of the Trend of Finances: Total Black	R's Perception of the Trend of Finances: North Black	R's Perception of the Trend of Finances: South Black	R's Perception of Future Financial Prospects: Total Black	R's Perception of Future Financial Prospects: North Black	R's Perception of Future Financial Prospects: South Black	R's Perception of Financial Satisfaction: Total Black	R's Perception of Financial Satisfaction: North Black	R's Perception of Financial Satisfaction: South Black
				Davies's Operationalization					
1968	−.053[a]	−.098	−.024	−.109	−.086	−.161	.148[c]	.219	.016[b]
1966	.095	.021	.256	.070	−.223	.449			
1964	.112	.214	.051	.068	−.008	.106	.136	.235	.081
1962	.412	.384	.432	.025	−.284	.294			
1960	.374	.374	.282	.233	.204	.326	.401	.327	.479
1958	.091	.163	.018	.038	−.054	.176	.112	.254	−.093
1956	.023	−.132	−.019	−.140	−.318	−.012	.011	.021	.002
				Differ Operationalization[d]					
1968	−.053	−.097	.012	−.123	−.094	−.143	.131	.216	−.008
1966	.149	.203	.101	.018	−.085	.096			
1964	.274	.359	.231	.116	−.076	.247	.195	.240	.179
1962	.492	.636	.355	.196	−.009	.383			
1960	.665	.743	.405	.259	.431	.231	.410	.414	.369
1958	.438	.623	.293	.007	−.275	.193	.276	.561	.052
1956	.281	.436	.195	.026	−.079	.053	.048	.098	.043
				Discrim Operationalization					
1968	−.073	−.131	−.051	−.154	−.144	−.200	.121	.198	.016
1966	.144	.174	.127	−.014	.085	.096			
1964	.232	.302	.180	.079	.058	.159	.205	.243	.189
1962	.492	.631	.378	.184	−.030	.353			
1960	.649	.735	.389	.264	.431	.254	.421	.417	.489
1958	.323	.496	.181	−.002	−.197	.126	.256	.486	.031
1956	.201	.388	.022	−.004	−.080	−.013	.028	.063	.022

[a] Entries are Pearson's product moment correlations.

[b] Life satisfaction was used in 1968 due to change in protocol.

[c] Perceptual variable unavailable for 1966 and 1962.

[d] See Footnote 20 for a discussion of this operationalization.

Economic Failure Alienation and Extremism (Ann Arbor: The University of Michigan Press, 1968); Barrington Moore, Jr., "Revolution in America," *New York Review of Books*, 12 (January 30, 1969), 6–12.

ships would constitute an accurate test of the correspondence between objective and subjective indicators. The Davies's measure has been shown to have little value in assessing relative deprivation. Also future financial expectations are significantly different in conceptual meaning from current assessments of discrimination, and it was anticipated that future financial expectations would not correlate well with the objective measure. The main areas of interest within the matrix revolve around the correspondence between the differ and discrim operationalizations and R's perceived trend of finances and R's financial satisfaction.

In evaluating the matrix, the correlation coefficient of .25 was chosen as the minimum level for accepting a relationship between indicators.[22] Even at this obviously low level, the objective operationalizations, on a point-by-point comparison, failed to demonstrate a relationship with the perceptual indicators. In the case of the relationship between the differ and discrim operationalizations and financial satisfaction, the .25 hurdle is crossed only 40 per cent of the time for northern blacks and 20 per cent of the time for southern blacks. The relationship between the discrim variable and the perceived trend of one's finances shows little overall consistency for the total black population, despite good results generally for northern blacks. In this case the .25 level is exceeded 71 per cent of the time.

The relationships between the differ and discrim operationalizations on the one hand and the perceived trend of one's finances on the other are the best of the group, although still not very encouraging. These correlations exceed the .25 mark 71 per cent of the time for the northern black population but only 43 per cent (for differ) and 28 per cent (for discrim) of the time in the southern black population. Interestingly, the inappropriateness of the Davies's operationalization seems further underscored by its weak correspondence with the perceptual measures, being the most poorly related of the three.

The strength of the correspondences between the differ and discrim operationalizations and the trend of finances variable is at times quite good. Note, for example, the relationship for northern blacks from 1958 through 1962. We are not sure why this correspondence exists and then radically tapers off, reaching a negative relationship in 1968. And we are reluctant to invoke any substantive explanations since the overall pattern of the data is not encouraging from the perspective of point-by-point substitutions. Interpreting the few cases that have correspondences might well tend to force systematic meaning where the overall picture seems to indicate that there is none.

Whether this lack of correspondence is also true of the trends of the subjective and objective data sets (as distinct from point-by-point comparisons) will be considered in the next section. We also wish to note that the analysis has not assessed the possibility that in more dramatic circumstances, such as full-scale revolutions, the convergence and salience of events might be so overwhelming as to be reflected in various kinds of assessments. Although our analysis does not preclude this possibility, at present, this is still an article of faith which has not been subjected to empirical verification.

A View of the Urban Riots from Perceptual Indicators

If Davies's argument is correct and if our objective data are too remote from the structure of the theory to test it adequately, our empirical observations of perceptual data should demonstrate the following results.

(1) From 1956 to 1964 the proportion of northern blacks perceiving positive changes in the trend of their finances should increase steadily.

(2) After 1964 the proportion of northern blacks perceiving positive changes in the trend of their finances should begin to decline continually and substantially.

The above two points express succinctly and in empirically relevant terms the essence of the J-curve as applied to the requisite conditions of the urban riots. In addition to this depiction of the rise and decline scenario, there is another equally important consideration. This relates to the untested assumption of relative deprivation theory generally and the J-curve specifically that one's current trend of need fulfillment is positively and strongly related to future financial expectations. In the context of the J-curve this of course occurs during the period of rising gratifications. Its occurrence, however, is one of the most vital aspects of the theory. For if there is no correspondence between current levels of need fulfillment and future expectations of need fulfillment, the theory is wholly untenable. Without such correspondences an intolerable gap can not occur (see Figure 1). These observations lead to two additional considerations:

---

[22] Correlations of .25 in social science data are generally viewed as quite favorable. In this case, however, we have chosen to view .25 as a minimal level of acceptance because a criterion for the establishment of a valid substitute measure for different levels of analysis is being considered. Without engaging in a lengthy discourse on the philosophical issues involved in the level-of-analysis question, it is clear that such substitutions do involve an issue of validity, i.e., whether one measure is a valid substitute for another. Correlational requirements for measures of reliability and validity are far greater than for mere assertions of substantive importance. See for example, Fred N. Kerlinger, *Foundations of Behavioral Research* (New York: Holt, Rinehart and Winston, Inc., 1965), 444–462.

(1a) From 1956 to 1964 the trend of finances for northern blacks should be positively and strongly related to future financial expectations.

(2a) After 1964, the pre-1964 relationship between the trend of finances for northern blacks and their future financial expectations should reverse itself.

The rejection of *any* one of the above four statements would result in a rejection of the J-curve theory as an explanation of the black urban riots. Nonetheless, we will review the results as applied to all four statements because we are not only interested in testing the theory, but we are also concerned with the presence or absence of empirical justification for the theory's basic assumptions. Moreover, special concern surrounds statements 1a and 2a because they are also fundamental to the assumptions of relative deprivation theory generally.

Before examining the data, there is an item concerning the demarcation of time frames for rising and declining need fulfillment that merits attention and commentary. This concerns the appropriateness of looking at the post-1964 period as the likely area of decline and hinges on the interpretation of the Watts riot which occurred in August of 1965. If this event is perceived as having initiated the riot phase, then the period before 1964, it has been suggested, is the more appropriate place to look for financial reversals. Watts would then be considered as the point of the "intolerable gap," where the disparity between continually rising expectations and falling actual need fulfillment culminates in civil disorder.

Such issues might have been readily resolved if the J-curve theory had been empirically more rigorous in specifying the operationalization of the "intolerable gap." One of the difficulties with all forms of *post hoc* theory is that they manifest an empirical prowess greater in appearance than in fact. The requirements of a theory tested through prediction would of necessity mandate an operationalization of critical concepts. Where theory is constructed after instead of before prediction, such empirical precision is too easily overlooked. This lack of specification further complicates analysis because all forms of civil disorder, even coup d'etats, are often a series of events rather than a single event. These events can occur in close temporal proximity, but, sometimes, as in the case of the riots, there is substantial temporal space between them. As to which single event among the set of events coincides with the crucial intolerable gap is an issue that is generally ignored.

Further specification by the original theory could have provided some assistance in properly assessing the role of Watts in this scenario. However, even if Watts is considered the initiator of this phase of riot activity, the two-year gap between Watts and the preponderance of riot activity strongly suggests that the downward cycle of perceived need fulfillment began about 1964, was in evidence by August, 1965, but it did not approach the intolerable gap phase until 1967.

An interpretation of Watts as coming at the bottom of the need fulfillment spiral would have to focus on the sharp decline from 1960 to 1962 in Table 6 and Figure 6. (Interpretation is restricted to the perceptual data because they constitute the best indicators.) But this analysis would be undermined by the sharp rise in need fulfillment occurring from 1962 to 1964. It would also be forced to ignore the important cycle of fluctuation and change throughout the data set, not to mention the two-year gap between Watts and the other riots. For these reasons, we have chosen to maintain 1964 as the point of demarcation between rising and declining need fulfillment in the hypotheses above.

Turning now to the data and consideration of increased need fulfillment, Table 6 and Figure 6 below demonstrate that blacks did not experience increased need fulfillment from 1956 to 1964. Instead of a trend of continual improvement, there exists a series of sharp fluctuations, not at all unlike the results obtained with the objective data. If the northern blacks are compared to northern whites, the contrast in this regard is rather marked. Across time relatively stable percentages of northern whites perceive their finances as improving, while percentages of northern blacks experiencing the same perception fluctuate dramatically.

Except for the tremendous upsurge in 1964, characteristic of all groups, northern whites show a pattern of marked stability. Excluding 1964, the range of responses varies from a low of 34 per cent to a high of 39 per cent. Given considerations of sampling error, it is not unreasonable to assume that a straight line would, with the exclusion of 1964, accurately characterize these data. Southern whites also show a fairly stable pattern, but the fluctuation that does exist ensues from the close temporal proximity between the highest and lowest points in the longitudinal distribution. Financial optimism triumphed in 1964 but crashed disastrously by 1966. Outside of that sharp downward spiral the pattern is seemingly constant. Even with due consideration of the sharp drop from 1964 to 1966, the range for southern whites runs from a low of 29 per cent to a high of 41 per cent, with a difference of only 12 per cent.

The argument for the stability of white perceptions is best appreciated by looking at the corresponding data for the blacks. Northern blacks run a gamut of alternate high to low and low to high fluctuations, with a range from 21 per cent to 46 per cent, or five times that of northern whites. For southern blacks the pattern is similar except there is somewhat more stability, each low ebb is not necessarily followed by another high point, and the period from 1964 on is virtually constant. The range is 23 per cent, or nearly twice that of southern whites.

Interestingly, if the same longitudinal relationships are examined from the perspective of the respondents' conception of their financial satisfaction or their expectation of their future financial satisfaction, the same phenomenon of white stability and black instability is repeated (see Tables 7 and 8 below).

The data clearly indicate that *it is the northern black population that experiences the greatest instability*. Even incorporating the impact of 1964 – a year which appears to have produced sharp financial optimism among all groups – the variation among whites in both the North and South is nowhere nearly as sharp as it is for blacks; and it is again the northern blacks, not the southern blacks, who illustrate the greatest instability.

Is it likely that this series of fluctuations has emerged as a consequence of systematic differences in sampling design between off-year and presidential election year studies? Or is the instability, perhaps, due to the differences in sample size between blacks and whites, with blacks demonstrating more instability simply because of smaller Ns? The pattern of fluctuation and change observed in Figure 6 and Table 6 can not be dismissed as mere aberrations in the data resulting from systematic changes in the sampling frame between presidential and nonpresidential election studies. The Survey Research Center did not use different sampling frames in the off-year and presidential year studies. From 1956 through 1960, the identical

Table 6. R's Perception of the Trend of his Financial Situation by Race and Region

	1956	1958	1960	1962	1964	1966	1968
Northern white							
Better	39%	34%	36%	35%	48%	37%	34%
Same	43	45	47	46	38	37	45
Worse	19	22	18	20	14	26	21
(Total)	101%[a]	101%[a]	101%[a]	101%[a]	100%	100%	100%
(N)	(1168)	(1165)	(1179)	(777)	(3017)	(797)	(1644)
Northern black							
Better	33%	21%	46%	21%	44%	33%	32%
Same	43	49	30	52	40	41	44
Worse	24	30	25	27	16	27	25
(Total)	100%	100%	101%[a]	100%	100%	101%[a]	101%[a]
(N)	(63)	(84)	(77)	(52)	(184)	(78)	(110)
Southern white							
Better	40%	32%	35%	38%	41%	29%	33%
Same	44	46	49	47	44	45	50
Worse	17	22	17	15	16	26	17
(Total)	101%[a]	100%	101%[a]	100%	101%[a]	100%	100%
(N)	(425)	(464)	(544)	(382)	(1146)	(322)	(690)
Southern black							
Better	28%	32%	19%	17%	40%	40%	38%
Same	51	32	42	50	43	35	48
Worse	22	37	40	33	17	25	14
(Total)	101%[a]	101%[a]	101%[a]	100%	100%	100%	100%
(N)	(83)	(76)	(91)	(58)	(234)	(57)	(132)

[a] Sums to more than 100 per cent because of rounding.

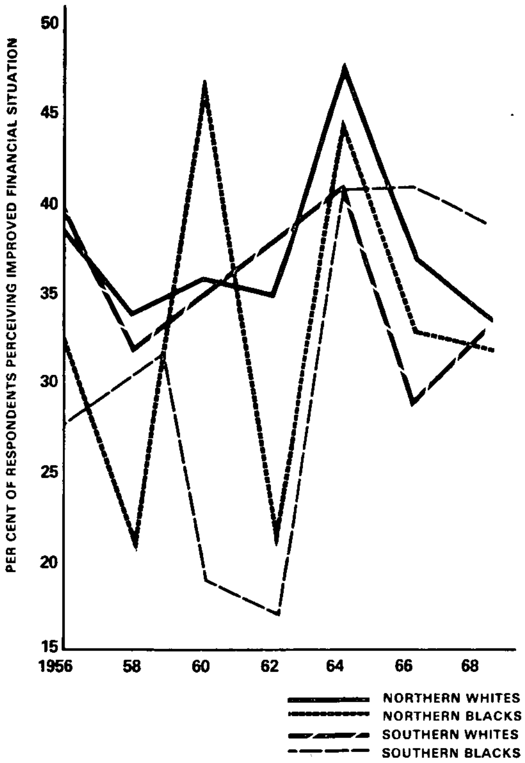


Figure 6.
Percentage of Respondents Perceiving the Trend of their Financial Situation as Improving

sampling frame was used. The stability of this design was reinforced by the presence of a large panel study for this time period. Consequently, the sharp fluctuations and changes observed in Figure 6, for the 1956 through 1960 period, can hardly be attributed to changes in the sampling frame. In 1962 the sampling design was updated and the resulting frame was used through and including 1968, making it impossible to attribute changes observed in this period to changes in the sampling design. Even the changes between 1960 and 1962 can not be attributed to the change in sampling frames, for the large metropolitan self-representing PSUs, where some two-thirds of the northern blacks reside, remained as self-representing units.[23]

How about the question of sample size? Can the changes be attributed to differences in sample size for blacks and whites? Do the blacks display more volatile patterns simply because their sample size is smaller? This argument is untenable because it overlooks the crucial implications of the Law of Large Numbers (Bernoulli's theorem).[24] The critical question is not whether the black sample is smaller or larger than the white sample. The critical question is whether the black sample, both in the North and South, is sufficiently large to meet the requirements of Bernoulli's Theorem. As Table 6 demonstrates, these well-known criteria for statistical inference are more than amply met by the sample Ns.

Although the above commentary would seem to lay this issue to rest, we decided to pursue this matter one step further. A ten per cent sample of white respondents was selected by means of a random number generator applied to each discrete data point, with region controlled. If the volatility for blacks relative to whites is purely a function of differences in sample size, then a sample of whites severely reduced in size should demonstrate the same volatile pattern as that observed for the blacks. Yet, the ten per cent sample was virtually indistinguishable from the larger sample in both the case of northern and southern whites. This finding plus the information on the sampling frames would appear to eliminate the assumption that the results could be attributed in any meaningful way to the sampling design or the differences in sample size.

Now that we have demonstrated that the observed results could not be meaningfully attributed to sample size or design, and that rising expectations did not characterize the trend of need fulfillment for northern blacks prior to 1964, is there any indication the need fulfillment declined sharply after 1964? Again, if we look at Table 6 and Fiture 6, we note a drop from 44 per cent in 1964 who perceived the trend of their finances as improving, to 33 per cent in 1966. This is, however, a smaller drop than took place in the two previous periods of decline, and the relationship stabilized in 1968. Consequently, there is no confirmation of a continual decline after 1964, as the theory would predict.

Turning now to the all-important assumption of relative deprivation theory generally that current need fulfillment is related to expectations of future need fulfillment, this is shown to be generally unsubstantiated (see Table 9). The assumption could be demonstrated as warranted for northern whites, and to a far lesser extent for southern whites; however, in the case of northern blacks it is most untenable. (This result provides another indication of the importance of performing the analysis within specific subgroups as opposed to treating the society or the group as a monolith, a problem inherent in aggregate data analysis.) Consequently, this assumption which is virtually axiomatic to all forms of relative deprivation theory is not as universal as its strategic role demands. This relationship obviously varies from group to group and from circumstance to circumstance. All we can say is that sometimes, and for some groups, future expectations are a function of current need fulfillment, and at other times, and for other groups, this relationship does not hold.[25] Indeed, this is hardly the kind of axiomatic statement on which a science of revolution is to be built.

---

[23] We wish to thank Leslie Kish and Irene Hess of the sampling section of the University of Michigan's Institute for Social Research for explaining the sampling design to us and clarifying this issue.

[24] The theorem is represented as follows: probability $(|f/n - p| \geqslant \epsilon) \rightarrow 0$ as $N \rightarrow \infty$; where f is the frequency of occurrence of event X among N trials, p is the probability of X, ϵ is some arbitrarily small positive number. It is generally assumed that good results are produced with N greater than 30. It also is at this point that the normality assumption of the Central Limit Theorem can be relaxed.

An important implication of these results is that for blacks, as opposed to whites, perceptions of past experience are not instrumental in shaping perceptions of the future. This would suggest that although whites can rely, to some degree, on past experience as a means of interpreting the future, blacks cannot. It is probably not unwarranted to infer that this is an additional indication of the ambiguous and unstable environment which blacks inhabit.

The data further indicate that instead of the increasingly strong relationship between trend

Table 7. Respondent's Perceived Financial Satisfaction by Race and Region

	1956	1958	1960	1962	1964	1966	1968[b]
North (white)							
(Pretty Well) Satisfied	41%	38%	36%	a	46%	a	26%
(More-or-Less) Satisfied	42	42	42		40		65
Not Satisfied (At All)	17	20	22		14		9
	100%	100%	100%		100%		100%
N =	(1168)	(1163)	(1179)		(3012)		(1684)
North (black)							
(Pretty Well) Satisfied	29%[b]	16%	22%		31%		15%
(More-or-Less) Satisfied	32	26	26		39		65
Not Satisfied (At All)	40	58	52		30		20
	101%[c]	100%	100%		100%		100%
N =	(63)	(86)	(76)		(185)		(109)
South (white)							
(Pretty Well) Satisfied	49%	50%	45%		46%		24%
(More-or-Less) Satisfied	34	34	35		42		66
Not Satisfied (At All)	17	16	20		13		10
	100%	100%	100%		101%[a]		100%
N =	(429)	(465)	(553)		(1146)		(696)
South (black)							
(Pretty Well) Satisfied	24%	30%	17%		32%		12%
(More-or-Less) Satisfied	37	37	42		34		74
Not Satisfied (At All)	39	33	42		35		14
	100%	100%	101%[c]		101%[c]		100%
N =	(83)	(76)	(91)		(235)		(131)

[a] Data unavailable for 1962 and 1966.

[b] In 1968, for "Life Satisfaction" the categories are: Completely Satisfying, Pretty Satisfying, Not Very Satisfying.

[c] Column adds to more than 100 per cent because of rounding.

---

[25] Ted Robert Gurr informs us (Correspondence 12/27/74) that although he agrees that the linkage between future expectations and current need fulfillment is a fundamental assumption of relative deprivation theory, the relationship may vary within the social structure. In the course of yet uncompleted research (with Raymond Duvall) Gurr has theorized that individuals in the middle range of attained values (need fulfillment) appear to expect more gains in the future than those at the extremes of the distributions. If Gurr is correct, this would require some critical restatement of the underlying assumptions of relative deprivation theory. Some preliminary analyses that we have undertaken demonstrate that for blacks, those at the lowest end of the continuum have the highest expectations. For example, preliminary runs from SRC's Campbell-Schuman data set indicate that young (18–35) northern, black males are most dissatisfied with their current situation and most optimistic about the future. Needless to say, the issue Professor Gurr raised is an important one that could conceivably have a major impact on relative deprivation theory.

Table 8. R's Perception of Future Financial Prospects by Race and Region (1956–1968)

	1956	1958	1960	1962	1964	1966	1968
North (white)							
Get better	43%	41%	39%	39%	45%	37%	34%
Stay the same	47	46	50	55	45	51	55
Get worse	10	13	11	6	10	12	10
	100%	100%	100%	100%	100%	100%	99%[a]
N =	(1094)	(1130)	(1107)	(686)	(2884)	(700)	(1644)
North (black)							
Get better	60%	68%	54%	36%	72%	37%	27%
Stay the same	29	26	39	55	24	57	58
Get worse	10	6	8	10	4	6	15
	99%[a]	100%	101%[a]	101%[a]	100%	100%	100%
N =	(58)	(84)	(67)	(42)	(173)	(63)	(110)
South (white)							
Get better	44%	41%	38%	38%	40%	28%	35%
Stay the same	48	46	54	57	51	59	55
Get worse	8	13	8	5	9	13	10
	100%	100%	100%	100%	100%	100%	100%
N =	(396)	(450)	(499)	(358)	(1098)	(287)	(690)
South (black)							
Get better	49%	53%	61%	21%	60%	51%	31%
Stay the same	40	33	31	71	35	36	59
Get worse	10	13	9	8	5	13	11
	99%[a]	99%[a]	101%[a]	100%	100%	100%	101%[a]
N =	(77)	(75)	(81)	(48)	(224)	(45)	(132)

[a] Does not sum to 100% because of rounding error.

Table 9. Correlation Between R's Perceived Trend of Financial Fulfillment and R's Future Financial Expectations by Race and Region

	1956	1958	1960	1962	1964	1966	1968
Northern whites	.39	.39	.39	.41	.50	.44	.44
Southern whites	.22	.45	.29	.37	.28	.54	.42
Northern blacks	.05	–.24	.71	.02	.34	.44	.51
Southern blacks	–.01	.24	.44	.33	.42	.24	.25

of finances and financial expectations, as was predicted by the J-curve theory for the period between 1956 and 1964, northern blacks exhibit an erratic pattern, with correlation coefficients ranging from a low of –.24 in 1958 to a high of +.71 in 1960. Moreover, instead of the coefficients declining after 1964, they actually increase (see Table 9).

The perceptual data indicate that in the case of each of the four hypotheses relating the J-curve to the black urban riots of the 1960s, the theory was rejected. Equally important is the observation that one of the basic axiomatic components of the J-curve theory and relative deprivation theory generally was found to be unconfirmed – that future expectations of need fulfillment are a function of current perceptions of need fulfillment.

It has been suggested that although the J-curve theory as disconfirmed by the data *as an explanation of the urban riots,* this is not sufficient reason for implying a general lack of confidence in the theory. This argument is based on the consideration that Davies may have erred in attempting to extend his theory beyond the parameters for which it was designed. Davies's theory, it has sometimes been asserted, was directed at revolutionary events, and the urban riots, whatever else they may be called, were not revolutionary events. The lack of confirmation in the instance of the urban riots, it has been sometimes further suggested, is simply the result of attempting to explain diverse phenomena with a single dynamic. This argument is worthy of attention for two reasons: (1) It is consonant with arguments offered in behalf of middle-range theories.[26] (2) In a recent communications exchange between Davies and David Snyder writing with Charles Tilly, Davies appears to have made a similar point.[27]

Snyder and Tilly[28] assert that a longitudinal evaluation of political violence in France from 1830 to 1960 does not fit the J-curve. In response, Davies[29] argues that a long, whole-country series analyzing various events of civil disorder is not an adequate test of his theory. Davies implies that his theory need not be expected to account for nonrevolutionary events. In light of his own work, this is a rather astounding argument. Even if one were to view Davies's application of the J-curve to the urban riots as an over-extension of the theory, such a position would hardly be commensurate with Davies's original formulation of the theory or his own previous work. As Snyder and Tilly note, "... Davies himself had applied his scheme to student protests, draft riots, the protests of American blacks in the 1960s not to mention the Pullman strike, the Dorr Rebellion, plus many other conflicts of all shapes and sizes."[30]

If Davies is to be consistent in his contention that Snyder and Tilly's work is irrelevant to his arguments, then it is incumbent upon him to propose a reformulation of the parameters of his own theory. Certainly, the case can be made in terms of middle-range theory that a variety of historical experiences cannot, at this stage of social science, be explained in the context of a single dynamic. In Davies's case, this reconstruction is at best difficult, for the development of the theory itself is based on a diversity of types of civil disorder. Davies's development and previous applications of the theory indicate that the attempt to explain the urban riots is wholly in accord with the theory's general formulation and application. It could not be construed as an attempt to apply the theory outside the parameters for which it was intended.

The Black Urban Riots: Some Suggestive Theoretical Issues

In the period preceding and during the riots, the black community did not experience a sustained improvement in its economic situation followed by a sharp reversal. Instead of this J-curve scenario, the black community generally and the northern black community specifically displayed dramatic ambiguity and instability in describing the trend of their finances, their financial satisfaction, and their expectations of future financial improvement. These observations were borne out by both the objective and subjective data. The ebbs and flows of the points in any one year may vary from objective data to subjective data. Nonetheless the correspondence in the overall trend remains.

Were these perceptions of instability more marked in the 1960s than in any other previous contemporary period of black history? This we do not know. Even so, the very pattern of the data suggests an alternate explanation of the riots. Here, however, we must tread with caution. Relative deprivation studies typically observe a pattern of data, often aggregate data, together with a concomitant episode of collective or political violence. The two are then interpreted as being related, with some form of relative deprivation, represented by the data configuration, being assumed as the cause of the ensuing violence. In terms of classical considerations of causality and experimental design, this type of deductive reasoning leaves much to be desired.[31] It is, however, the basis for many of the theoretical interpretations of revolution and the underlying causal mechanism of virtually all relative deprivation studies. Sears and McConahay[32] note this type of logic is also the basis for the relative deprivation approach to the study of the black urban riots.

While acknowledging the limitations of such procedures, we must also note the difficulty in obtaining the type of data that would directly link the assumed preconditions of violence with the acts of violence. This is especially true of historical studies of violence, but this problem is also prevalent in studies of contemporary episodes of violence. The research of Gurr,[33]

---

[26] For a discussion of this position see Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: The Free Press, 1957), pp. 5–11.

[27] Davies, "Comments [on Snyder and Tilly]," *American Sociological Review*, 39 (August, 1974), 607–612.

[28] David Snyder and Charles Tilly, "Hardship and Collective Violence in France, 1830–1960," *American Sociological Review*, 37 (October, 1972), 520–532.

[29] Davies, "Comments," pp. 607–610.

[30] Snyder and Tilly, "Comments [On Davies]," *American Sociological Review*, 39 (August, 1974), 611.

[31] For a discussion of causal inference in classical experimental design, see Donald T. Campbell and Julian C. Stanley, *Experimental and Quasi-Experimental Design for Research* (Chicago: Rand McNally, 1963).

[32] David O. Sears and John B. McConahay, *The Politics of Violence* (Boston: Houghton Mifflin, 1973), pp. 90–91, footnote 1.

[33] Gurr, *Why Men Rebel.*

Davies,[34] and the Feierabends,[35] to name but a few of the most regarded scholars working in this area, have manifested these inherent and often unavoidable difficulties.

These limitations should not preclude incisions into the problems of collective and political violence. They must, however, caution us about the limitations of our inferences, and make us aware that we are perhaps more engaged in an exploration enterprise than in "confirming" hypotheses.

With such caveats in mind, we are struck by the suggestiveness of our own data. The data demonstrate that the black community generally, and the northern black community particularly, experienced extreme fluctuation and ambiguity in its perceptions of the trend of its finances, its financial satisfaction, and its expectations of financial improvement. These extreme fluctuations and ambiguities of black perceptions might have led to the urban riots of the 1960s.

The pattern of data fits both the theoretical writing by Feierabend et al.[36] based on observations from aggregate data, and the work by Grofman and Muller[37] based on individual data. Feierabend et al. see constant flux and its attendant uncertainties as producing frustration. As they note, "uncertainty is a special quality of expectations. Ambiguity as to whether the future will bring disaster or salvation should be considered a distressful experience, adding to the present sense of frustration. Only in the case of disaster is certainty more likely to be judged as more frustrating than uncertainty."[38] Frustration, of course, leads to aggression; and aggression, under the proper circumstances, will lead to political violence.

Similar observations have been made by Grofman and Muller. They found that "the greatest potential for political violence is manifested both by individuals who perceive negative change . . . *and* by individuals who perceive positive change. . . , while those who perceive no change manifest the least potential for political violence."[39]

The theoretical and empirical foundations for these observations are anchored in a long tradition of social science research. In his famous study of suicide, Emile Durkheim[40] noted that anomie could be produced by social disruption irrespective of whether it resulted from economic disaster or an abrupt transformation leading to power and wealth.

Ambiguity, fluctuation, and uncertainty as conditions which contribute to one's susceptibility to be mobilized for revolt have been addressed in the works of Barrington Moore, Jr.,[41] Michael Aiken,[42] Seymour Martin Lipset,[43] Norman Cohn,[44] and Murray Edelman.[45] In a speculative essay, Edelman has applied this interpretation directly to the black urban riots. He argues that man's expectations of the distant future influence the cognitions that explain his current situation. The important expectations of status and security come from governmental acts. The inconsistent fluctuation between responsiveness and nonresponsiveness to black needs by the government produced alienation, fear, and anger. In this climate, Edelman notes, a precipitating incident can easily touch off violence. Moreover, he goes on to argue, the greatest ambiguity existed for northern blacks.[46]

The interpretation suggested by our perceptual data is highly congruent with these theoretical statements that fluctuation, change, and ambiguity create situations highly conducive to violence. Edelman's suggestion that ambiguity is greatest among the northern blacks is also confirmed by our perceptual data.

What can be said with some confidence is that the black community generally and the northern black community specifically experienced a great deal of perceptual uncertainty with regard to their financial situation and their expectations of their future financial circumstances. We can also argue that there is at least a substantial set of theoretical and empirical work to indicate that such conditions have a high probability of precipitating political violence. It is at least plausible to note that the pattern of fluctuation and change and the attendant perceptual ambiguities may be components of the causal nexus that culminated in the black urban riots. Certainly, this argument is no less plausible nor less buttressed by empirical evidence than most of the current explanations of the riots. The difficulty resides in the linkage between the pattern of the data on the one hand and the violence on the other. Until it is possible to bridge this chasm in a causally meaningful fashion, our results must remain suggestive avenues for further research.

---

[34] Davies, "The J-curve."

[35] Feierabend et al., "Social Change."

[36] *Ibid.*

[37] Grofman and Muller, "The Strange Case of Relative Gratification."

[38] Feierabend et al., "Social Change," p. 637.

[39] Grofman and Muller, p. 514.

[40] Emile Durkheim, "Anomie and Suicide," in *Sociological Theory,* ed. Lewis A. Coser and Bernard Rosenberg (New York: MacMillian, 1970), p. 529.

[41] Moore, "Revolution in America."

[42] Aiken et al., *Economic Failure, Alienation and Extremism.*

[43] Lipset, *Political Man.*

[44] Norman Cohn, *The Pursuit of the Millennium* (New York: Harper Torchbooks, paperback, 1961), p. 319.

[45] Edelman, *Politics as Symbolic Action.*

[46] *Ibid.*, pp. 20–21.

Summary

The purpose of this research was to test the J-curve theory in the context of the black urban riots by using both perceptual and objective indicators. The results demonstrate that the J-curve theory is not a valid explanation of the riots. Several empirical, methodological, and conceptual problems were also noted. These basically revolved about the regrettable tendency of researchers to test a theory based on individual perception with aggregate and objective data.

The research demonstrated that: (1) Aggregate indicators can be highly misleading substitutes for individual indicators. The J-curve's use of an aggregate index to measure black frustration was found, upon examination of the same measure based on individual data, to be highly misleading. Moreover, the inappropriateness of the operationalization was concealed by the process of aggregation. (2) Objective indicators cannot be used as direct substitutes for perceptual indicators, even when both are based on individual data. There is, however, some indication from our analysis that while substitutions are unwarranted on a point-by-point basis, substitutions may be justified when based on the overall trend of the data. The generality of this conclusion, however, awaits further confirmation. (3) Regardless of the nature of the data configuration, societies cannot be viewed as monoliths. It is imperative that specifications and distinctions be made for various groups. As our data show, trends for the total black population often obscured crucial divergencies between northern and southern blacks. Relative deprivation-based theories that treat societies as monoliths may well conceal as much as if not more than they reveal. If statements such as "revolutions are most likely to occur when things get better" are to have any theoretical meaning, then it must be demonstrated for whom things have gotten better. Is it indeed the revolutionary class? When unspecified data demonstrate that things improved for the society as a whole, can we be sure *that things did not simultaneously get worse* for the revolutionary class? Is it the rising expectations resulting from overall improvement that caused the revolution? Or is it an increasing gap between the revolutionary class which experiences no improvement, and the rest of the society that experiences increased need satisfaction? The monolithic approach cannot assist the student of revolution in resolving such questions and may well lead to deceptively simple answers. (4) The critical *assumption of the J-curve theory and relative* deprivation theory generally that future expectations of need fulfillment are a function of current need fulfillment trends was shown to be largely unsubstantiated. The data did indicate modest correlations for these two variables for northern whites, and to a lesser extent for southern whites and southern blacks. There was no basis for this assumption when applied to the crucial northern black community. Since this assumption is one of the axioms of the J-curve, its consequent lack of verification should at least raise the question of how the axioms of the J-curve might be restructured to accommodate this information.

To students of revolution who have used relative deprivation type theories, this research advises caution. Such caution ensues from a firmer recognition of the problems of analysis of historic revolutionary events that derive from data which are of necessity based on aggregate, objective indicators. Such analysis might be thoroughly misleading, as this research has demonstrated. This factor has even caused some researchers to forsake relative deprivation theory as an explanatory device. Such rejection is not based on theoretical limitations but on the inability to fit relative deprivation theory to appropriate data. At the same time, if our observation is correct that even in the absence of point-by-point correlations between objective and subjective indicators, there is consistency between the overall trends, relative deprivation theory might still have great promise. It remains for research to be directed at this problem through simultaneous analysis at both the individual and aggregate levels.

Finally, we propose some avenues for further research on the substantive issue of the etiology of the black urban riots. The cycles of fluctuation and change that emerged from our analysis of perceptual data fit an extensive theoretical literature on the causes of civil disorder and revolution. Although we are reluctant to follow many of our colleagues across the inferential chasm that depicts a causal relationship when a diagrammatic configuration of relative deprivation exists in the same temporal space as an act of violence, the congruence of our findings with other theoretical statements suggests that the fluctuation-change hypothesis might well be a fruitful area for further research into the causes of the black urban riots.

Equality and Human Need

MARVIN ZETTERBAUM
University of California, Davis

Equality and Recognition

The good or the valuable is revealed to man by God; or, it arises out of man's rational awareness of the requirements of his nature and of the place of that nature within the cosmos; or, it is a reflection in man's consciousness of the unfolding dialectic of history; or, it is a human response to environmental and human contingencies of reinforcement; or, it originates simply in man's fiat or will. Whether from God, nature, reason, history, necessity, environment, culture, or will; whether destiny, discovery, or invention, there is no understanding of value without recourse to human nature. From Aristotle to scientific social scientist there is on this point no dispute. Where Aristotle notes that "it is the peculiarity of man, in comparison with the rest of the animal world, that he alone possesses a perception of good and evil, of the just and the unjust, and of other similar qualities. . . ,"[1] Milton Rokeach confirms, "man is the only animal that can be meaningfully described as having values."[2]

This paper is in part an exploration of how and whether it may be said that value arises from human need, how the concept of human need itself can be understood, and, specifically, how and whether the value of equality may be said to arise from some particular human need. If, in commencing this inquiry we begin with Thomas Hobbes, it is not because Hobbes is widely celebrated for his egalitarianism. If we turn to *Leviathan* for a defense of equality, it is not because we anticipate a treasure-trove of hitherto untapped and even unsuspected arguments in its behalf. Instead, it is because we find in Hobbes a way of thinking about equality and human need which with various turns and emphases continues to the present. That way of thinking is also the subject of this inquiry.

The argument for the natural equality of mankind found in the thirteenth chapter of *Leviathan* can scarcely be condensed; it occupies barely two paragraphs. It is so characteristically Hobbesian that its basic components – struggle and vanity – are as well the underpinnings of the entire structure of the *Leviathan.* Of course we are not enjoined to display toward our fellows that equal regard which follows from our being all children of the same God, created in His image. Nor in any precise sense do we find a variant of the argument Locke will use later as *his* justification of natural equality: that we are all members of the same species, born to the same rank and possessed of the same faculties, that, in brief, all men are men as all cats are cats and dogs, dogs. And we certainly do not find any claim about the sameness or identity of men; Hobbes is not so foolish as to deny the evident *inequality* of men in regard to their natural endowments, chiefly of mind and body. What we do find is the claim that, taken all in all, these natural differences do not count for very much. As Hobbes declares, ". . . when all is reckoned together, the difference between man and man, is not so considerable, as that one man can thereupon claim to himself any benefit, to which another may not pretend, as well as he."[3] Lest anyone mistake his meaning, he immediately remarks that natural strength is no bar to another's undertaking a hostile action, for the weaker can easily overpower the stronger either through guile or, what amounts to the same thing, by combining with others. The argument for the natural equality of mind is a bit more subtle: Hobbes recognizes that most men are vain enough to think themselves wiser than their fellows and concludes from their evident satisfaction with this state of affairs that each is content with his share and therefore the equal of every other.

---

[1] Aristotle, *Politics,* 1253a15.

[2] Milton Rokeach, *The Nature of Human Values* (New York: Free Press, 1973), p. 20. On the problem of the synonomous use of the terms "good" and "value," see Martin Diamond, "The Dependence of Fact Upon 'Value,'" *Interpretation,* Volume 2/3 (Spring, 1972), 229–231.

[3] Thomas Hobbes, *Leviathan,* ed. Michael Oakeshott (New York: Collier, 1962), p. 98.

Whatever may be said about Hobbes's argument, he clearly does not look upon the equality of men as a brute fact, as a simple description of their natural condition in the same way that we may say men are *in* nature along with streams, plants, other animals. To appreciate this is to understand the curious and otherwise inexplicable hesitancy Hobbes shows when he comes to set forth his ninth law of nature, the one against pride: that every man acknowledge every other as his equal by nature. Our needing a law of nature to affirm an already existing natural condition (no stone needs to be told to fall at the rate described by the law of gravitation) points to a certain ambiguity about the status of equality, one that Hobbes readily acknowledges. He tells us: "*If* nature therefore has made men equal, that equality is to be acknowledged: or *if* nature has made men unequal; yet because men that think themselves equal, will not enter into conditions of peace, but upon equal terms, such equality must be admitted."[4] The point is not that men are free to break laws of nature as stones are not, for if that were all that was at issue there would be no need to intrude the surprising admission that perhaps, after all, nature has not made men equal. What is decisive, then, is that, whatever the *truth* about natural equality, it is men's *opinions* about themselves and of each other that are politically determinative. Of course, men are so constituted by nature that they weigh, balance, and compare themselves with every other, but it is no conclusion of nature that the scales are in equilibrium. So far from fact is this the case that a convention backed by the awesome power of Leviathan must be invoked to assure the conditions of civil peace; the law of nature is contrary to our natural passion propelling us toward self-preference and pride.[5] Egalitarianism has little support in nature, but it is the necessary *precondition* for that political life which is to remedy the defects of nature. The "men that think themselves equal" are being induced by Hobbes to so think by an argument allegedly founding equality in nature.

Whatever, then, the truth about the equality of men by nature (and that truth is ultimately ambiguous), the origin of the requirement of political (i.e., conventional) equality lies in the fact that each man is constantly scrutinizing the gaze of the other to determine whether that other values him as he values himself. If not, he is driven to every extremity to extract from that other the proper sign of recognition, "for every man looketh that his companion should value him at the same rate he sets upon himself."[6] In fact, the rate a man sets upon himself is most often greater than an objective appraisal would warrant. In competing in this way for honor and dignity, it is inevitable that that competition degenerate into a war of every man against every other.

Still, if Hobbes's assertion that the natural condition of war is an inference from the passions is valid, an individual's overevaluation of himself cannot be the result of mere caprice. Rather, it must arise from the necessities of his nature. We need only remind ourselves that men are so constituted that they must toil ceaselessly in fulfillment of their desire; the *means* to accomplish this is what Hobbes defines as their *power.* In the final analysis, power is essentially social, determined far less (if at all) upon an objective appraisal of a man's natural abilities than upon an estimation of these by others. If men compete for honor and dignity, they compete for public or private signs that *others* assign them so much value or worth. For the value of a person is nothing other than what some other would be willing to pay for that person's "power."[7] The value or worth of a person is therefore not absolute, "but a thing dependent on the need and judgment of another. . . . And as in other things, so in men, not the seller but the buyer determines the price. For let a man, as most men do, rate themselves at the highest value they can; *yet their true value is no more than it is esteemed by others.*"[8] Men must set a high value upon themselves and extract from others the recognition or acknowledgement of that value if they are to have any fair prospect of "obtaining those things which a man from time to time desireth,"[9] that is, any prospect of felicity. The competition for honor and dignity, for recognition, is a basic human need.

It is not my intention to follow Hobbes as he moves from consideration of man as the matter, the raw material, of that artificial man, Leviathan, to the construction or articulation of the commonwealth itself, nor to inquire whether the conventional equality he insists upon as the precondition for political life is consistent with the equality of all but the sovereign. For my purposes it is sufficient only to have shown how the theme of equality enters political life (in Hobbes) through the theme of recognition. The law of nature affirming equality is in reality an hypothesis of reason counteracting natural inequality and the natural disposition toward self-preference and pride. It is civil society, not nature, that is the true locus or ground of equality. The drive for recognition dissociates men; equality reconciles them, makes possible that concord that is the health of the body politic. The ultimate justification for equality in Hobbes is thus an awareness of the urgent need to suppress the drive for recognition (or its consequences) if men are to associate in a way that guarantees them survival and reasonable comfort. The justification is utilitarian, one which satisfies those needs or passions man may be said to share with all other creatures – (the natural drives clustered around the need for preservation), while jettisoning or sacrificing that need which is man's alone – (the peculiarly human but nevertheless still natural need of recognition). Hobbes establishes equality by abstracting humanity from political life, or by reducing men to mere animal nature.[10] It cannot surprise us, therefore, that the argument for equality in Hobbes does not derive from any recognition of man's common humanity or brotherhood or that it is not associated with any claim for the equal dignity to which men are entitled by virtue of their common humanity.

---

[4] *Ibid.*, p. 120. Italics mine.

[5] *Ibid.*, p. 129.

[6] *Ibid.*, p. 99. Cf. also p. 131.

[7] *Ibid.*, pp. 72–73.

[8] *Ibid.*, p. 73. Italics mine.

[9] *Ibid.*, p. 55.

Implicit in all that Hobbes has said is the banal if melancholy conclusion that acknowledgment of a basic human need does not necessarily lead to measures that insure the fulfillment of that need. The human condition may very well frustrate its fulfillment. Things being what they are, it may indeed be the case that we must exchange our quest for honor and dignity for a loaf of bread. Moreover, *being human* is what makes possible the transmutation of human needs into animal needs, or what makes it possible for us to accept at all Hobbes's description of man as a description of ourselves. Hobbes is not simply asking us to check his reading of mankind against our own; more importantly, he is inducing us to *replace* our reading with his own by altering the way in which we read ourselves. But it is impossible to believe that a basic human need can be made simply to disappear – sublimate it, rechannel it, even suppress it, it will still be there readying itself to force its appearance. As Horace reminds us: "You may drive out Nature with a pitchfork, yet she will always hasten back."[11]

Because recognition is a phenomenon of the psyche, Hobbes ultimately deals with it by attempting to reshape the *opinions* that men have of each other, and thus the way they confront each other. This need not be the only way to take account of recognition, though we might think it the most consonant with the nature of the phenomenon itself. Locke is also concerned with the same phenomenon and similarly makes equality the precondition of genuine political life, though, as noted above, on the basis of an argument different from that of Hobbes. But here the relationship of equality to recognition is more oblique precisely because it was Locke's intention to deflect the concern with recognition from an obsessive preoccupation with the look of another to a concern for material well-being. Instead of contending with one another for affirmation of their status, men will busy themselves with the acquisition and enlargement of their properties. Locke thus shifts the locus of the struggle for recognition from the political to the private sphere, hoping thereby to achieve that civil peace and prosperity Hobbes thought to accomplish by the suppression of recognition altogether. Of course, this is only to deflect not to abolish recognition, and, in the event, men come to place a value upon themselves in relation to others by the size of their properties. That this had political consequences Locke both understood and welcomed.

On the other hand, Rousseau saw in any vestige of a struggle for recognition the seeds of moral and political corruption. It was Rousseau who understood that equality as a precondition of political life, if seen only as the possession by every citizen of the equal right to participate in the formulation of public policy, could very well coexist with inequality of social and economic condition and hence of status. These, in turn, would make a mockery of the equal rights themselves. Consequently, Rousseau bent every effort to so transform the nature of man while reconstituting him as citizen that the very sense of self and of particularity upon which the drive for recognition feeds would be obliterated.[12] Citizens thus emptied of their particular selves would be truly equal one with another. Indeed, with Rousseau, the view emerges that egalitarianism, far from being the precondition of political society is in fact the end or goal of political society, a position even more visible in Marx. But whatever permutations and combinations the concept of equality underwent in the course of the two centuries succeeding Hobbes, what is common to them all is the primacy of recognition as a basic human need, if not *the* basic human need. Whether the conflict is seen as one between superior and inferior, master and slave, individual and citizen, lord and serf, capitalist and proletariat; and whether the resolution is Leviathan, the General Will, the dialectic of History, or the victory of the proletariat – the underlying theme is the unrelenting obsession with the regard of another. Surveying the course of political philosophy since Hobbes, one may indeed say with Professor Leo Strauss that the course is marked by the assumption that man as man is thinkable ". . . as a being that is guided by nothing but a desire for recognition."[13]

---

[10] Cf. John H. Schaar, "Some Ways of Thinking About Equality," *Journal of Politics,* Volume 26 (November, 1964), 877–879.

[11] Horace, *Epistles,* Bk. 1, epistle x, 1.24.

We may note, however, two distinct and opposite dispositions toward the desire for recognition. There is, first, as in Hobbes, a concentration upon the deleterious consequences to human life of the necessity to be recognized. In early formulations, the very concept of a state of nature inhabited by solitary human atoms coming together only out of dire necessity suggests the superiority of a condition in which men, having no or no lasting associations with others, are not under any such constraints as the desire for recognition imposes. There men may live free from the anxiety generated by the concern with the regard of another. They are free, moreover, to live their *own* lives, taking as the measure of their life what is peculiarly their own, uncontaminated by any necessity to disguise themselves or their being in order to please others. Even Rousseau's solitary, stupid, unimaginative savage has one basic redeeming feature; his life is an articulation of *amour de soi,* of that proper love of himself that arises from and is attuned to his own needs and to the sentiment of his own being. When conditions force him first to associate with others and then to form with them a civil society, this original, authentic self-love is subverted into *amour-propre,* vanity, that in which he derives his opinion of himself from the opinions of others. The process is ineluctable, there is no going back, but such reflections as these cast a pall over political life. Life together is purchased at the sacrifice of one's own.[14]

A wholly different attitude toward the phenomenon of recognition, one closer to the contemporary insistence upon esteeming another not in terms of his "price" but of his intrinsic worth as a "person," has its origins in the thought of Hegel and Marx. Here recognition is seen not only as a basic human need but also as the primary agency through which the process of becoming human is initiated and carried forward. In Marx, at least, it becomes as well the ultimate goal or quest of the human condition. A condition in which man does not recognize another human being *as* a human being, i.e., as his equal, is still a condition of mere nature, one in which man's true humanity is subordinated to necessity. Marx writes of the history of mankind as an evolutionary development of natural needs into human needs, the latter characterized precisely by and culminating in the recognition of human beings *as* human beings.[15] The delineation of genuine sexual relations in his *Early Writings* is a case in point. Such relations tell us "how far man's needs have become *human* needs, and consequently how far the other person, as a person, has become one of his needs, and to what extent he is in his individual existence at the same time a social being."[16] Thus, far from seeking to subvert or even to rechannel the need for recognition (as Hobbes and Rousseau each in his own way attempts), the philosophical as well as political task is to facilitate and bring to final fruition the human destiny hidden in this peculiarly human need.

---

[12] See Jean Jacques Rousseau, *The Social Contract and Discourses,* trans. G. D. H. Cole (New York: E. P. Dutton, 1950), pp. 18–19, 38–39, 307–308; *Emile,* trans. Barbara Foxley (New York: Everyman, 1966), pp. 7–8. Cf. also John Charvet, "Individual Identity and Social Consciousness in Rousseau's Philosophy," in *Hobbes and Rousseau,* ed. Maurice Cranston and Richard S. Peters (Garden City, N.Y.: Anchor, 1972), pp. 462–483; and John Charvet, "The Idea of Equality as a Substantive Principle of Society," *Political Studies,* Vol. 17, No. 1 (March, 1969), 6–7.

[13] Leo Strauss, *On Tyranny* (Ithaca, Cornell, 1968), p. 205. The full quote is: "Both doctrines [i.e., Hobbes's and Hegel's] construct human society by starting from the untrue assumption that man as man is thinkable as a being that lacks awareness of sacred restraints or as a being that is guided by nothing but a desire for recognition."

[14] There is, of course, an analogue in classical thought to Rousseau's depreciation of political life. But, whereas classical philosophy rejects political life in favor of the philosophic life, Rousseau does so in favor of the self, of the sentiment of one's own existence.

[15] See Nicholas Lobkowicz, *Theory and Practice* (Notre Dame: University of Notre Dame Press, 1967), p. 356, and also, Shlomo Avineri, *The Social and Political Thought of Karl Marx* (Cambridge: Cambridge University Press, 1970), pp. 88–89.

[16] Cited in Avineri, p. 90.

It may be difficult to discern the roots of today's concern with "transparency" and "person-to-person encounter" in Hegel's description of the archetypical struggle to the death between two individuals, each striving to extract from the other the recognition of the value he places upon himself; still, the basic need to be recognized in oneself as a person is common to both. In fulfilling this need, Hegel declares, man achieves that radical break with nature, in particular with his animal nature, that makes possible his humanity.[17] What distinguishes man from all other sentient creatures is his self-consciousness, his awareness of himself as an I, as a *Self*, as something apart from and therefore not identifiable with his natural desires. This arises when and as man desires this or that object not as an animal desires for its own sake an object of warmth, food, or protection, but for the sake of the other "human" desires which have become attached to or embodied in the object. Thus a child may desire to keep himself clean, not for his own sake, but for the sake of experiencing the love of his parent that has become identified with the parental injunction about cleanliness. As Kojève explains: "It is human to desire what others desire, because they desire it. Thus, an object perfectly useless from the biological point of view (such as a medal, or the enemy's flag) can be desired because it is the object of other desires."[18] Only such desires are truly human desires, and action undertaken in fulfillment of such desires constitutes human reality. We may recall Marx's illustration of sexuality. If I desire only the body of another, I remain at the level of animal desire; if, however, I desire the desire of another or the desire embodied in another person, I may be said to love as a human being loves. But to recognize such an embodied desire is to recognize it as emanating from a consciousness like my own; I do not simply respond to it as I might to an object in nature which either satisfies or frustrates my desires. And in "recognizing" the other as human, I confirm myself as human. But lest the other treat me as indistinguishable from any other object in nature, and lest I thus lose the tenuous grasp of my own humanity, I will go to any lengths to extract from him a sign of my humanity.[19] As Kojève goes on to say: "Without this fight to the death for pure prestige, there would never have been human beings on earth. Indeed, the human being is formed only in terms of a Desire directed toward another Desire, that is – finally – in terms of a desire for recognition."[20]

There is a controversy among the interpreters of Hegel about whether the struggle for recognition will eventuate in a condition in which there are no longer any masters or any slaves, no longer any ruling or being ruled, in which all are equal. Certainly the Hegelian analysis affirms at least a kind of abstract equality, that of the reciprocal recognition of two centers of self-consciousness, and, by extension, of all such centers. Ultimately, this is the source of Hegel's affirmation of the equality of men as men: "It is part of education, of thinking as the consciousness of the single in the form of universality, that the ego comes to be apprehended as a universal person in which all are identical. A man counts as a man in virtue of his manhood alone, not because he is a Jew, Catholic, Protestant, German, Italian, etc. . . ."[21] It seems also the case that such equality is by no means to be translated literally into political life:

> Men are made unequal by nature, where inequality is in its element, and in civil society that right of particularity is so far from annulling this natural inequality that it produces it out of mind and raises it to an inequality of skill and resources, and even to one of moral and intellectual attainment. To oppose to this right a demand for equality is a folly of the Understanding which takes as real and rational its abstract equality and its "ought-to-be."[22]

Whether, then, the struggle for recognition will terminate in a homogeneous world state, as Kojève avers, or whether a form of abstract equality (say, of "equal rights") will coexist in any future Hegelian state along with hierarchies of rule and of class division, as I believe, is not a controversy into which we can enter here.[23]

---

[17] In what follows, I am indebted for the exposition of Hegel's section in the *Phenomenology* on Mastery and Slavery to Alexandre Kojève, *Introduction to the Reading of Hegel*, ed. Allan Bloom (New York: Basic Books, 1969), pp. 3–30.

[18] Kojève, p. 6. See also John Plamenatz, *Man and Society* (New York: McGraw-Hill, 1963), Vol. II, p. 152.

[19] Kojève, pp. 9, 11.

[20] Kojève, p. 7.

[21] *Hegel's Philosophy of Right*, trans. Thomas M. Knox (Oxford: Oxford University Press, 1949), No. 209, p. 134; also fn. to No. 270, pp. 168–169.

[22] *Ibid.*, No. 200, p. 130.

[23] See the controversy between Strauss and Kojève in Strauss, *On Tyranny*, pp. 154–156, 183–184, 221–226. Cf. Plamenatz, pp. 155–156; George A. Kelly, "Notes on Hegel's 'Lordship and Bondage,' " in

It is important to note, though, that while contemporary scholars may depart in their analyses from the "heroic" form in which Hegel depicts the battle for recognition, many continue to affirm the absolute primacy of the phenomenon itself. Kojève, for example, maintains that "... the desire to be 'recognized' in one's reality and in one's eminent dignity (by those whom one 'recognizes in return') is actually, as I believe, the ultimate motive of all emulation among men...."[24] Plamenetz, who is himself critical of the Hegelian understanding of the form in which the struggle takes place, nevertheless adds, "I should not quarrel with Hegel for insisting that the desire for recognition is the pre-eminently human desire...."[25] Finally, John Charvet in an analysis of Rousseau rejects the *Social Contract* because Rousseau therein suppresses "that element in man, namely his *amour propre* or desire for the respect of others and of himself, which constitutes his specifically human characteristic...."[26]

As we have already seen, there is no unidirectional inference from recognition to equality, and the opposing pulls – toward equality or inequality – are not reconciled easily, if at all. For some, the logic behind the drive for recognition presses toward inequality, and extreme measures are indicated for the suppression of the drive in order to assure a measure of political equality. For others, suppressing the drive for recognition is equivalent to the suppression of that which is taken to be the hallmark of our humanity, and, moreover, it is precisely with the full realization of recognition that men attain to a genuine sense of equality. But even in regard to the genuine or truly human form of equality, the concern with recognition reflects also a fundamental ambivalence as we sometimes look to the recognition of our common humanity and sometimes to recognition of the concrete actual person that stands before us. As we shall see below, it is this latter demand that assumes for us the status of the primary moral and political obligation.

Value and Basic Human Need

Whether recognition is thus seen as the precondition for or the terminus of civil society, and however ambiguous the inference from recognition to equality, it is challenging that for three centuries it has been characterized as the basic human need, the ultimate motive, the pre-eminently human desire, the specifically human characteristic. Without neglecting the relationship of equality and recognition, it is appropriate to turn now to a closer look at the concept of basic human need. Political philosophy has traditionally been engaged in the search for the genuinely human, for whatever it is that constitutes human nature. It has been guided by the not unreasonable conviction that political society must in some way take its bearings from the genuinely human. It is hard to imagine how it could be otherwise. Among contemporary writers, Christian Bay explicitly calls for a "political science that deals with *basic human needs* as well as overt desires and other observable aspects of behavior."[27] But Bay's proposals nevertheless reveal (as we might expect from one who balks at the inference from "fact" to "value") a curious ambivalence toward these basic human needs. Far from concluding that the identification of such needs determines the legitimate values of political life, Bay remarks instead that "the student of politics, once he has adopted a conception of human needs, should proceed from there to make explicit *his* inferences about political objectives and *his* choice of commitments with the utmost care."[28]

Not everyone, of course, is as hesitant as Bay in concluding value from need. Some, like Professor Edgar Bodenheimer, look upon basic needs as comprising the ontological structure or makeup of man and derive therefrom the values appropriate to human life. In a penetrating and suggestive article, "Philosophical Anthropology and The Law," Bodenheimer argues on the basis of modern psychological evidence and "a look at the contemporary scene" that the most important values that the structure of law is designed to protect and promote are security, liberty, and equality, and seeks to demonstrate that these values "have not been arbitrarily singled out as lodestars of legal regulation but that they have deep ontological roots in the

---

Hegel: A Collection of Critical Essays, ed. Alasdair MacIntyre (Garden City: Anchor, 1972), pp. 193, 214–217; and Sidney Hook, "Hegel Rehabilitated?" in *Hegel's Political Philosophy*, ed. Walter Kaufmann (New York: Atherton, 1970), pp. 60–63.

[24] Alexandre Kojève, "Tyranny and Wisdom," in Strauss, *On Tyranny*, p. 151.

[25] Plamenatz, p. 188.

[26] Charvet, "Equality," p. 8.

[27] Christian Bay, "Politics and Pseudopolitics: A Critical Evaluation of Some Behavioral Literature," in *Behavioralism in Political Science*, ed. Heinz Eulau (Chicago: Aldine-Atherton, 1969), p. 112.

[28] *Ibid.*, p. 115. Italics mine.

constitution of human beings."[29] But in the conclusion to the article, Bodenheimer acknowledges that his argument "depends on the tenability of two basic theses *which are by no means generally accepted today.*"[30] Of these, the first "is the proposition that man has a biological essence, consisting of physiological, psychological, and noetic constituents which cannot without peril be discarded in the legal regulation of human affairs," and, moreover, that "it is assumed . . . that men have certain basic needs, over and above the most pressing physiological needs, which ought to be taken into account by lawmakers in the interest of the health of the social body."[31] The second thesis is the related one "that the basic needs of men form an ontological foundation for the values protected by the legal order."[32] Bodenheimer refers to the well-known list of basic needs identified by Abraham Maslow – physiological needs, the needs for safety, love and belongingness, esteem needs (the locus of our present concern with recognition and equality), the needs for self-actualization, knowledge and understanding, and also aesthetic needs – and clearly shares with Maslow the view that these needs are not "cultural artifacts," nor are they the expression of subjective and emotive responses to reality. The needs described (which form the "link to social and legal goal values") are "empirical traits of men which are not deemed to be subjective and idiosyncratic."[33]

Still, it is notorious that any positive and uniform identification of these empirical traits has escaped most investigators. In his book, *Human Nature in Politics,* James C. Davies treats us to a survey of what psychologists from Freud to Maslow have identified as the basic human needs, sometimes also described as wants, drives, or instincts. We cannot repeat the whole inventory. Suffice it to say that the list progresses from the eros and death instincts of Freud; to the thirty-two identified by William James; the seven major and several minor ones of social psychologist William McDougall; some ten or eleven described by Graham Wallas in *his* work on *Human Nature in Politics* including such exotic ones as the desire for combat and for adventure; the twelve "viscerogenic" and twenty-eight "psychogenic" needs listed by Henry Murray of the Harvard Psychological Clinic; and finally, again, the several categories of needs identified by Maslow.[34] One might conclude from this embarrassment of riches only that the difficulty of the enterprise has so far precluded any final identification of the basic human needs. But while Davies himself admits that "knowledge of human motivation is not yet adequate to establish the psychological equivalent of a periodic table of the elements," it may be that the enterprise itself is thrown into question when he adds, "the garment of culture conceals the needs of the organism."[35]

The difficulty is the familiar one of the observer attempting to shed the garment of his own culture in order to reach those human needs which allegedly underlie his own as well as all other cultures. This difficulty is not insuperable, as any traveler to a foreign clime may testify. Yet perhaps we have already conceded too much. For the unexamined premise here is that there are human needs which have been decorously or otherwise covered over by the cloaks of culture but that the needs themselves persist in some fixed or unaltered form. The premise is only another restatement of the aspiration that lay behind the concept of the state of nature: strip man of his cultural or political garments (hypothesize the condition of man in his precivil state), and he will stand revealed as the naked shivering self he is. Whereas Hobbes and Locke postulated the existence of "natural man" *prior to* his entry into civil society, modern day investigators locate natural man *in* civil society or within any given culture. But this is less of an advance than might appear on the surface. Whether natural man precedes or persists in civil society, he is not in any fundamental sense a creature *of* society. This is not simply to affirm that men are human only *in* a social condition but, more importantly, that there is no being human apart from society and thus what it means to be human is ineluctably already a product; not of man *and* society, but man-society. This is of course Rousseau's criticism of the concept of the state of nature: whatever that creature is which exists prior to civil society, it is not man, not a human being. Man *becomes* human; man is his history.[36]

---

[29] Edgar Bodenheimer, "Philosophical Anthropology and the Law," *California Law Review,* Vol. 59, No. 3 (May, 1971), 662.

[30] *Ibid.*, p. 679. Italics mine.

[31] *Ibid.*, pp. 679–680.

[32] *Ibid.*, p. 680.

[33] *Idem.*, and see also fn. 73.

[34] James C. Davies, *Human Nature in Politics* (New York: John Wiley, 1963), pp. 6–7. Cf. Rokeach, pp. 11, 14, 20.

[35] Davies, p. 7.

[36] See Steven Lukes, *Individualism* (New York: Harper Torchbooks, 1973), pp. 150–152, and also Marvin Zetterbaum, "Human Nature and History," in *Human Nature in Politics,* (NOMOS XVII), ed. J.

If being human has the character just described, the unabating search for basic human needs as if existing in some uncontaminated state must prove futile. Investigators have affirmed the cultural substrate even of the so-called biological drives:

> Physiological disequilibrium, at least in human organisms, is so early and so thoroughly modified by social learning that pure or 'primary' needs do not exist except in theory. . . . At no point in the progression from the generalized tensions which include genital responsiveness, to the complex attitudes of fear, shame, or guilt over sexual behavior, can we isolate 'primary' sex needs from the matrix of social learning within which it develops. Like hunger, thirst, oxygen-deficit, or temperature imbalance, the disequilibrium of sex begins in a social context and is never separated from it.[37]

To put it another way, as Melford Spiro makes us aware, no one would categorize a society in which the activities of the members were centered exclusively around the search for food and mates and the avoidance of enemies as a human society.[38] A society is identifiable as human only when such drives articulate with an awareness of some comprehensive pattern that defines for its members what food is acceptable and what prohibited, who may qualify as a mate, and who is to be regarded as the enemy. Only in such a society does man become human. As Spiro adds: "Society is not only necessary for the survival of man, *qua* organism, it is crucial for the emergence of man, *qua* human. To develop a mind and a self it is necessary that the infant live in a human society and acquire its cultural heritage. . . ."[39] If the locus of becoming human shifts from organismic needs to society, it is no wonder that a search is undertaken for the invariant mechanisms or "structures" by which this is accomplished in every society, no matter what the "content" to which these structures give rise. Thus, for example, every society, it is alleged, is characterized by some comprehensive pattern or "world-view" which specifies not only what needs are to be met, indeed what are to be seen as needs at all, but also the manner in which these needs are to be fulfilled; and the *manner* assumes as much importance as the need itself.

Still, the search for invariant structures does not quite prepare us for the reappearance, in scarcely disguised form, of the need to be "recognized." It is thus surprising to find Spiro describing the process of becoming human as invariably and inevitably involving the awareness of and development of an individual *self*. "Self-awareness plays an enormous role in human behavior, for to most people their own selves are the objects of greatest value . . . [and] regardless of the source, the integrity of the self must be defended against . . . threats to its esteem. . . ."[40] Having been warned against the danger of discussing the individual in isolation from society, we are nevertheless treated, once again, to the central priority of awareness of self to the individual. It seems, indeed, that we cannot escape the notion that the recognition and defense of self is *a* basic human need, if not *the* basic human need. Nevertheless, we cannot help wondering if, in the account just given, the investigator is not himself a prisoner of his own societal world view, one wherein the centrality of the self is sustained by a long historical, religious, and philosophical tradition. If indeed it is the case that the world view of each society is determinative, must it not determine not only the way an individual experiences himself but also whether he experiences himself as a self at all? And if he does not, what could possibly be the meaning for him of a struggle for recognition or of the need to defend himself against threats to his esteem?

If we seek empirical confirmation of the existence of a basic human need for recognition in *all* societies however different their world-views – that is, if the issue is to be resolved by cross-cultural studies – we may be surprised to learn that the very concept of basic human need may have to be jettisoned. Dorothy Lee, for example, reports that in some societies – the Trobrianders, the Lovedus, and the Wintus among others – no sense of superiority or inferiority is experienced because the sense of comparison that makes such judgments possible is altogether lacking. Of course, these cultures have not attained a perfect state of equality of condition, nor are their citizens identical to one another. Differences in functions and achieve-

---

Roland Pennock and John W. Chapman (New York: New York University Press, 1977), pp. 225–249.

[37] Norman Cameron and Ann Margaret, cited in Melford E. Spiro "Human Nature in Its Psychological Dimensions," *American Anthropologist*, 56 (February, 1954), 19–30, at p. 24. On the cultural components of such concepts as time, space, motion, object, and people, see Jules Henry, *Pathways to Madness* (New York: Vintage, 1973), pp. 11–24. Cf. Karl Jaspers, "The Search for Being," in *The Existentialist Tradition*, ed. Nino Langiulli (Garden City, N.Y.: Anchor, 1971), pp. 167–168.

[38] Spiro, p. 25.

[39] *Ibid.*, p. 24. Cf. p. 27.

[40] *Ibid.*, p. 28. Cf. Rokeach, pp. 14–15.

ment exist here as in other cultures, but individuals are not *evaluated* in terms of these roles or accomplishments. Value accrues to the individual simply because of the uniqueness of the person. Lee believes she has found a species of equality in such societies which "derives from the recognition of the right to be different, noncommensurate, unique; from the valuing of sheer being."[41] Paradoxically perhaps, in these societies there is no sense (sometimes not even the word) of a unique "self." If it is difficult for us to conceive of the members of a society acknowledging (recognizing?) the uniqueness and hence value of each of the members of that society without resorting to the concept of self, it may be that, since Hobbes, we have been unable to think of self or even individual except in the context of a sense of self extracted from or wrenched from the unwilling hold of the other. Lee maintains:

> Equality was the solution appropriate to a culture where self is opposed to other, where society had to be protected from the encroachment of the individual; the solution of a society which holds an either-or conception of duality. There are societies, however, where dualism is complementary, so that the terms of the duality are not opposed nor measured against each other, *nor seen as discrete units.*[42]

Lee's argument might be thought thus to undercut the hypothesis of a universal need for recognition in the Hobbesian and Hegelian sense while nevertheless affirming a purely formal (contentless) basic human need for recognition, one whose specific make-up is supplied by each separate culture. But to draw this conclusion would be to misunderstand the force and direction of Lee's own comprehension of the relation of value to need. For, in keeping with a widely held view, she notes that "everywhere, in every society, the culture offers its peculiar codification of reality, its peculiar avenues of experience."[43] Moreover, the value premises of a society are not mere glosses superimposed upon some underlying foundation of invariant basic human needs. In an article significantly entitled, "Are Basic Needs Ultimate?", Lee argues that the concept of basic needs as the cause of behavior is entirely erroneous, that, in fact, needs are derivative, not primary. What are seen or experienced as needs in a given culture are seen or experienced as such because of the overarching value structure of that society, its worldview. ("To the Hopi, corn is not nutrition; it is a totality, a way of life."[44]) Taught in our society to *esteem* individualism, privacy, and sense of self, we generate the need for individualism, privacy, and sense of self, and we structure our familial, social, and political experiences to fulfill these "needs."[45] On this view, the simplistic understanding of human behavior arising in response to needs – that is, to some biologically given, unidirectional impulses, tensions, or deficiencies – is wholly inadequate to the phenomena. Nor will the ever expanding number or categories of needs serve to remedy the defect. Lee concludes that "culture is not . . . 'a response to the total needs of a society'; but rather a system which stems from and expresses something had, the basic values of the society."[46] From this perspective, the Hobbesian-Hegelian need for recognition is but a reflection of the worldview from which Hobbes or Hegel (more accurately, their culture) saw man, one in which the struggle to achieve identity and self-consciousness occupied the very center. Hardly a basic human need at all.

If basic human needs derive from value; if value is not, after all, either derivative from, an expression of, or even a transformation of some constituent human need, then we are compelled to wonder how we may escape the conclusion of an ineluctable moral relativism. For we are forced to wonder about the sources or the status of the comprehensive worldviews or value premises that structure for each culture its view of reality and of human need. Time and space do not permit us to rehearse the arguments said to undermine each of those supports mentioned in the first paragraph of this paper that might provide for value a foundation independent of chance or man's arbitrary will. We are familiar enough with contemporary discussions of the legitimacy of so-called value judgments where recourse is taken, finally, to an unsupported and unsupportable moral postulate. Such recourse has the character of and frankly proclaims itself as a secular act of faith. Justifications for equality as the primary human value often appear in just such a guise. Surveying the support for the concept of the dignity of man from its biblical origins to the present, Steven Lukes concludes: "In general, this idea of the dignity of the individual has the logical status of a moral (or religious) axiom which is basic, ultimate and overriding, offering a general justifying principle in moral argument."[47] As such, it would be beyond any possible verification, serving instead as the source from which moral arguments may proceed. But we could not then avoid inquiring about the primacy of this rather than of some other moral axiom.

---

[41] Dorothy Lee, "Equality of Opportunity as a Cultural Value," in Lee, *Freedom and Culture* (Englewood Cliffs: Prentice-Hall, 1959), p. 44.

[42] *Ibid.*, p. 48. Italics mine. See also in the same volume, "The Conception of Self Among the Wintu Indians," pp. 131–132, 134.

[43] *Ibid.*, p. 46.

[44] Dorothy Lee, "Are Basic Needs Ultimate?", *Journal of Abnormal and Social Psychology*, 43 (July, 1948), 391–395, at p. 393.

[45] *Idem.*

[46] *Ibid.*, p. 394.

We may conclude the present section with just one contemporary instance of this type of moral reasoning. "Equality," Sir Isaiah Berlin tells us, "is one value among many; the degree to which it is compatible with other ends depends on the concrete situation, and cannot be deduced from general laws of any kind; it is neither more nor less rational than any other ultimate principle; indeed, it is difficult to see what is meant by considering it either rational or non-rational."[48] Equality does not flow from any rights inherent in being a man; it is not a necessary logical deduction from Christian theology; it is not an "inalienable" element in the "ultimate structure" of reality.[49] It is however "one of the oldest and deepest elements in liberal thought, and is neither more nor less 'natural' or 'rational' than any other constituent in them. Like all human ends it cannot itself be defended or justified, for it is itself that which justifies other acts – means taken toward its realization."[50] Still, the impoverishment of such a view is evident when, inconsistently, Berlin warns against the thesis that "the commandment to treat all alike in like situations needs no independent argument to support it."[51] For, we must ask, what could possibly constitute the substance of an argument in favor of even this aspect of equality when we have already been told that the belief in equality itself rests neither in God, in nature, nor in reason, and that, in fact, it "cannot itself be defended or justified." About all we have left to stand on is the honor deriving from Joseph Schumpeter's dubious distinction between ourselves and the barbarian: "To realize the relative validity of one's convictions and yet stand for them unflinchingly is what distinguishes a civilized man from a barbarian."[52]

Recognition and Political Life

The point of traversing the maze of the doctrine of recognition, whatever its meanderings from Hobbes to Rousseau to Hegel and Marx, was that it seemed to promise a way of grounding one value, equality, in what was characteristically and undeniably a basic human need. The doctrine of recognition had, moreover, the twin virtues of, on the one hand, not relying upon extrarational sources for its support, and, on the other (once the doctrine was shorn of its original orientation in Hobbes) of avoiding the disposition to reduce human things to the merely natural. What was sought was a *human* value deriving from a *human* need.[53] It promised also at least partially and modestly to meet the requirements that Herbert Spiegelberg set forth for a fully adequate defense of human equality: ". . . a complete study of the human structure, of human values, of human rights and duties, of human dignity and destination, and, moreover, a determination of their relative significance within the framework of human nature."[54] Not only, however, must we concur with Professor Schaar that "our age has been unable to construct a philosophical anthropology, a general answer to the question, what is man"?[55] but also the wreckage that surrounds us forces us to doubt the wisdom of having undertaken the inquiry in the first place. It might not have been fruitless if we had had only to relinquish the search for the basic human need which justifies equality; what is disconcerting is the conclusion that no human value can ever be said to derive from any basic human need.

---

[47] Lukes, p. 51. Cf. Schaar, "Some Ways": "The older, pre-eighteenth century idea of a common humanity, often denied but never forgotten, was a moral assertion based not upon a generalization of experience but upon a postulate of reason and an exercise of faith" (pp. 876–877).

[48] Isaiah Berlin, "Equality as an Ideal," in *Justice and Social Policy*, ed. Frederick A. Olafson (Englewood Cliffs, N.J.: Prentice-Hall, 1961), p. 150. See also *The Sociology of Georg Simmel*, trans. and ed. Kurt H. Wolff (New York: The Free Press, 1950), pp. 73–74; and also Pepita Haezrahi, "The Concept of Man as End-in-Himself," in *Kant: A Collection of Critical Essays*, ed. Robert P. Wolff (Garden City, N.Y.: Anchor, 1967), pp. 302, 311–313.

[49] Berlin, p. 128.

[50] *Ibid.*, p. 150.

[51] *Ibid.*, p. 148.

[52] Joseph Schumpeter, *Capitalism, Socialism and Democracy* (New York: Harper Torchbooks, 1962), p. 243. It is precisely this sentence that Berlin quotes approvingly in his *Four Essays on Liberty* (London: Oxford University Press, 1969), p. 172.

[53] Cf. Schaar, "Some Ways," p. 881.

[54] Herbert Spiegelberg, "A Defense of Human Equality," *The Philosophical Review*, Vol. LIII, No. 2 (March, 1944), 101–124 at p. 106. Cf. Alfred Schutz, "Equality and the Meaning Structure of the Social World," in *Aspects of Human Equality*, ed. Lyman Bryson et al. (New York: Harper, 1956), p. 37.

[55] Schaar, "Some Ways," p. 875.

The sole *positive* conclusion that emerges is, simply: there are "n" ways of becoming human. Indeed, the only defining characteristic of humanity is its infinite capacity to vary the ways of becoming human. Paradoxically, it might be thought that we could elicit from this conclusion a requirement of respect (recognition) for all those who display this characteristic, that is, a respect for human beings as human, and so arrive again at a ground for the value of equality.[56] Despite an understandable weariness over the discovery and proclamation of one more indisputable *cogito* from which our understanding of all human things would flow, there is still an almost irresistible attraction in the comprehensiveness of the conclusion itself. The new *cogito* practically forces itself upon us; it is faithful to the reality of being human; it readily accommodates itself to the phenomena we have been examining; it dissolves the paradoxes and difficulties over the relation of need to value; and, as a quite unexpected bonus, it provides as well (out of the ground of recognition) an objective foundation for the requirement of equality.

We may, however, have stumbled upon fool's gold. Our new *cogito* answers the question "what is man?" with "man is the creature that varies his way of being"; but it must be painfully obvious that this revelation falls far short of what we should wish from a fully developed philosophical anthropology. It is in the first instance spare and appallingly abstract. It directs us to the source of human values but reveals nothing about the means of differentiating among them, and so leaves us wholly in the dark as to our rights and duties. In a formal way it locates for us the special source of our human dignity, but is silent about our destination and about our obligation, if any, to the rest of the cosmos. It is simultaneously an invitation to innovation and revolution and a defense of what has already come into being, for what has come into being is just as much a way of being human as that which seeks to replace it. And, finally, it is either meaningless or self-contradictory, or both, in that it provides no way of distinguishing (or of approving or rejecting) *as human or nonhuman* any of the "n" ways of being human. It is not inconsistent with the *cogito* to live a human life indistinguishable from the animal, or to live in preparation for the divine. It is as human to choose to live among your fellows as it is to live alone. It is as human to live a life of philosophy as of politics; a life of "Peace, Good Will, Mutual Assistance and Preservation" as of "Enmity, Malice, Violence, and Mutual Destruction." It is as human to recognize your fellows as your equals as it is to deny that equal recognition.

Still, our short-lived *cogito* bears a marked family resemblance to other attempts to set forth whatever it is in virtue of which we recognize others as our equals. Respecting others for this capacity to construct a way of life for themselves is a way of looking at equality that has both scriptural and secular, principally Kantian, overtones. The classic account of the struggle for recognition in Hobbes and in Hegel contained, as noted above, a crucial ambiguity. In its archetypical form, the struggle is between two individuals each seeking to extract from the other the recognition of himself not as "human being" but as *this particular person* embodying the *particular* value he places upon himself. But in the development of the theme of recognition from Hobbes to Rousseau and Kant and on through Hegel all particularity is lost sight of as the emphasis shifts from the concrete to the common. The equality that is sought or defended is the equality of everyone as a person, as a human being, entitled thereby to equal treatment before the law, equal participation in the making of law, equality of opportunity, in general, "equal rights." The original drive for recognition is thus transmuted by Kant and Hegel into a search for the universal self or ego or reason or will in virtue of which each individual is a human being. In this process, the concrete unique individualized particular self shrivels in succumbing to the universal self.[57] As Williams remarks of Kant:

> Kant's view not only carries to the limit the notion that moral worth cannot depend on contingencies [i.e., empirical factors] but also emphasizes, in its picture of the Kingdom of Ends, the idea of *respect* which is owed to each man as a rational moral agent – and, since men are equally such agents, is owed equally to all, unlike admiration and similar attitudes, which are commanded unequally by men in propor-

---

[56] See, e.g., Philip Selznick, "Natural Law and Sociology," in *Natural Law and Modern Society* (Cleveland: World Publishing, 1963), pp. 168–169.

[57] Sentiments proclaiming the absolute value of the individual can also be found in Hegel. See *The Philosophy of Hegel*, ed. Carl J. Friedrich (New York: Modern Library, 1954), p. 20.

> tion to their unequal possession of different kinds of natural excellence. . . .[58]

Our *cogito* has thus a quite respectable ancestry. It, too, abstracts from the empirical complexity of life to reach that which is thought to be true of every human being – the capacity to vary one's way of life – and thus does not require admiration for the constructions or ways of life themselves. It recognizes the equality of "personhood" rather than that of *this* or *that person.*

But if our *cogito* can lay claim to a decent lineage, it and all its ancestors are in serious difficulty. Consider the approach Steven Lukes pursues in his recent study, *Individualism.*[59] According to Lukes, when we admire, praise, or reward an individual we do so on the basis of some acknowledged accomplishment or achievement; if, on the other hand, we *respect* another human being, we do so without regard to any specific accomplishment and even in the absence of any.[60] In offering such respect, we are not acknowledging some inner urge for self-confirmation through recognition by the other, but we are, instead, responding to the awareness we have of the characteristics that that individual has in common with all others. What we respect are potentialities or capacities – for reflection, acts of courage or justice, aesthetic sensitivity and the like. Lukes is correct, I believe, in maintaining that once one has accepted this mode of argument and has thus already abstracted some pure inner "core" from the actual content of human experience, questions of degrees of capacity or of actualization are irrelevant.[61] He admits that the accounts of the essences of these common human characteristics have differed widely: "The Christian ground for equal respect is that all are equally children of God, while the Kantian argument is that they all have free and rational wills and are members of the Kingdom of Ends. Some have suggested that there are empirical features which all human beings share which provide grounds for treating them with equal respect. . . ."[62] He himself offers up three such empirical characteristics: "the capacity for autonomous choice and action, the capacity to engage in valued activities and relations that require a private space, and the capacity for self-development. . . ."[63] It is too awful to contemplate beginning our inquiry anew by substituting "capacities" for basic human needs. We have little assurance of any more sanguine results. The inventory Lukes offers partly overlaps that put forward by Bernard Williams in his study of the idea of equality.[64] Williams's list, in turn, has been criticized by Schaar as neither indisputable nor terribly clear, and not even particularly useful.[65] Certainly we have no wish to repeat the litany of capacities as we did that of basic human needs.

It was not, however, my intention to review the list of human capacities proffered by Lukes. Rather, it was to consider, as implied in his preference for "capacities" over, say, "wants," "motives," or "interests," that *other* aspect of the drive for recognition – the need to be recognized in all one's concreteness, not simply as a representative unit of the human species. Lukes joins those who have characterized the philosophers of the eighteenth century as excessively prone to abstraction in their contemplation of humanity.[66] His objections parallel Schaar's criticism of the "liberal and constitutional theorists of the modern era" who created a "theoretical everyman" bearing little if any relevance to real men, to "flesh and blood resident[s] of the world." Schaar writes:

> This theoretical everyman is conceived as having but one, or, at most, a few attributes which are relevant to the construction of a political order. He is a generalized and diminished man, reduced to fear (Hobbes) or will (Kant) or interest (Madison) or the passion for distinction (John Adams) or pleasure-pain (Bentham). He is an elementary stimulus-response mechanism, a quantum of energy which is set into motion under specifiable conditions and directed toward specifiable ends.[67]

Simmel hammers the point home with great force and clarity: ". . . Man in general, man as such, is the central interest of the period; not historically given, particular, differentiated man. Concrete man is reduced to general man: he is the essence of each individual person. . . . In terms of the pure concept of mankind, all men are essentially alike. Compared with this general element, all *differentiated* individuality is something external and accidental."[68]

---

[58] Bernard Williams, "The Idea of Equality," in *Philosophy, Politics, and Society,* (Second Series), ed. Peter Laslett and Walter G. Runciman (Oxford, Blackwell, 1962), p. 115. But see Williams's demurrer, pp. 116–117, and cf. Charvet, "Idea of Equality," pp. 11–13; Haezrahi, p. 294 and fn. 5; Spiegelberg, p. 108; Simmel, pp. 72–73; Pierre Hassner, "Immanuel Kant," in *History of Political Philosophy,* (2nd ed.), ed. Leo Strauss and Joseph Cropsey (Chicago: Rand McNally, 1972), pp. 559, 561.

[59] See fn. 36 above.

[60] Lukes, p. 126.

[61] *Idem.* Cf. Schaar, "Some Ways," p. 880.

[62] Lukes, p. 126.

[63] Lukes, p. 133.

[64] Williams, pp. 112–114.

[65] Schaar, "Some Ways," p. 880.

[66] Lukes, pp. 146ff.

[67] Schaar, "Some Ways," p. 885.

As we shall see below, the objection of these writers is not to the use of abstraction as such, but to a mode of abstraction that is false to the reality of being human. Still, as we know from Burke, a concept's falsity is no bar to its being useful, even essential. So, Lukes acknowledges, ". . . there is no doubt that historically, the abstract conception of the individual represented a major moral advance. It was a decisive step in the direction of a universalist ethics when human beings first came to be regarded as the possessors of certain rights and claims, simply in virtue of being human."[69] Schaar has similarly called attention to the very positive benefits which accrue from considering all men as equal in certain respects and in thus establishing a public realm as "the area of uniform and equal behavior . . . in order that [all men] will be able to express their inequalities in the private realm."[70] The public realm so created demarks a carefully circumscribed arena in which citizens are accountable to the state for their actions; beyond this arena, they are free from governmental oversight. This particular postulate of equality thus legitimates the traditional liberal sanctuary of the private sphere. Moreover, Schaar adds, the postulate of equality allows us to conduct our affairs with all the anonymous others with whom we come in contact without having to consider each individual in his or her own uniqueness.[71] Justified by the abstract principle of equality, we impartially apply a uniform rule.

Still, such benefits are not without their costs. Like Tocqueville before him, Schaar fears that the real, full, many-faceted individual will begin to take on the characteristics of the narrow, abstract, diminished man of the public realm and so become transformed by the very instrument thought essential for his preservation. Taught, say, to consider ourselves in the public realm solely as predatory pursuers of economic interest, we come to behave just this way in the private sphere.[72] There is, additionally, concern for the character of public life itself; it, too, of course, is altered, diminished, by being populated with "abstract men" who precisely because they are abstractions, and insofar as they remain abstractions, cannot matter to us or become the objects of our affection.[73] While Lukes recognizes the contribution the concept of the abstract individual makes to equal treatment before the law and freedom from arbitrary handling, indeed recognizes these as "crucial and indispensable gains," he nevertheless insists that "if we are to take equality and liberty seriously, *they must be transcended.*"[74]

We need not wonder whether the inspiration behind the appeal to abstract man was noble, ideological, or utilitarian. What is finally decisive is that such an appeal is theoretically false. It is false, so the familiar argument goes, because it conceives of human nature as constituted by needs, instincts, drives, motives, whatever, which are in their *essence* unaffected by social and historical change. But such a view, according to Lukes, is emphatically denied by those who see society as fundamentally constitutive of the individual. A "sociological apperception" rejects the theory of the abstract individual whether conceived of as existing in some precivil condition or as persisting within society itself.[75] Instead of abstract man, we must speak of a social self. As may be readily seen, these are also the arguments, noted above, of those who deny the concept of basic human need. Taken together, these arguments render meaningless the search for that "essential self" (by whatever name it goes) that is thought to define for each of us our common humanity, and in virtue of which we affirm the equality of every human being. It is inconceivable that a legitimate justification for the value of equality could arise from a view of man that is demonstrably false, false to the reality of being human, false to the very possibility of being human.[76]

I have argued elsewhere that the concept of the "social self" is not free of its own internal difficulties.[77] In the context of the present inquiry, however, I wish simply to accept the view that the idea of the "abstract" individual is not tenable theoretically and to speculate on what the transcendence of such a view would entail for political man. It is apparent that the direction in which this view is to be transcended is toward that too maligned or neglected other aspect of the drive for recognition: the desire to be seen by the other not as *a* human being but as *this particular individual* here and now, to be recognized by the other not in virtue of any capacity as a moral agent or of the freedom to alter one's way of life but as this specific human being with these specific virtues and these specific faults – as a lover longs to be loved by his beloved for himself alone. It would be erroneous to locate the source of this aspiration solely in the contemporary concern with alienation, our experience of ourselves as a mere cipher among all those anonymous others, our failure to discover and to preserve a sense of self either in our everyday existence or in our public life. Alienation accentuates and makes all the more poignant the human desire to be recognized in one's concrete reality – a desire from which not even the philosopher's quest, for all its alleged self-sufficiency, is, I believe, immune.[78] No claim can possibly be offered here (especially in view of all that has preceded in the present paper) to have established this as a permanent human characteristic, need, or aspiration. Whether that can indeed be done on the basis of a fully articulated philosophical anthropology is something I am not prepared to say. But, as Socrates observed a very long time ago, it is not always necessary to have proved the existence of something before entering upon a discussion of it.[79] In any event, it is this other feature of the drive for recognition, one that balks at the effacement of the specific individual before some concept of the "everyman," that is at the heart of the demand for equality today.

---

[68] Simmel, pp. 67–68. See also, *ibid.*, pp. 58–84.

[69] Lukes, p. 149.

[70] Schaar, "Some Ways," p. 886.

[71] *Ibid.*, p. 887. Cf., *ibid.*, p. 884.

[72] *Ibid.*, p. 895. See also Schaar's description of the political, p. 889. Cf. Marvin Zetterbaum, *Tocqueville and the Problem of Democracy* (Stanford, Calif.: Stanford University Press, 1967), pp. 142–144.

[73] Schaar, "Some Ways," p. 890.

[74] Lukes, pp. 152–153. Italics mine.

[75] *Ibid.*, pp. 146, 151, 152.

[76] Cf. Schaar, "Some Ways," p. 877–879, and the following quotation at p. 893: "The constitutionalists . . . have offered a specifically political way of thinking about equality, a way which owes little to observation or to the moral sense. Their everyman is a construct, not an empirical portrait. . . . We cannot, without vast intellectual confusion and surprising practical consequences, deal with the construct either as an empirical generalization, or as a full guide to the making of public policy, or as an ethical justification for equal treatment." See also Charvet, "Individual Identity," p. 483; Charvet, "Idea of Equality," p. 13.

What is at issue here, it must be noted, is not the venerable problem of the application of a "standing Rule . . . common to every one of that Society" to a particular individual or case, for it is precisely the appropriateness of the standing rule that is in question or, more exactly, the status of the standing rule itself. That we are each entitled to equal rights, that we "should all be treated in a common or average way, so that the minimal conditions of a common life are made available to all. . ."[80] – these propositions rest finally on some claim of a *common* humanity. How will these fare if we are urged now to direct our attention away from the common to the real individual before us?

We need hardly pause to cavil at the prospects of instant bureaucratic intimacy, the counterfeit hello of the checkout clerk, the mass-produced personalized letter, the mandated air of deep personal concern of the local official. No one accepts these as genuine signs of recognition. But we do well to wonder whether the rejection of the common might not become the invitation for subjection to "the inconstant, uncertain, unknown, Arbitrary Will" of another. As Schaar has pointed out, there never has been a public realm populated with "abstract" men – we have always to deal with real men, men with strongly held views, passions, predilections, strengths, and weaknesses.[81] Certainly American political life has as often been a government of men as one of law, if such a distinction is meaningful at all except in theory. But a politics of real men operating within the constraints of a framework of common law is surely very different from such a politics devoid, say, of the safeguard of a constitutional structure. Nor is just any sense of the "common" threatened by the attacks upon abstract individualism – rather, it is precisely that common which promised to secure for each the equal regard of the law. If some maintain the difficulty if not the impossibility of defending equal treatment on the basis of some alleged characteristics we all share,[82] how much more difficult will it now become to treat with minimal decency the lazy as well as the industrious, the foolish as well as the rational, the sinner as well as the saint? For we are assured that the abstracted common which obliged us to treat our fellows as our equals is theoretically untenable. Moreover, as noted above, it is precisely this sense of the common – in the form of the postulate of abstract equality – that has traditionally been the safeguard of that private sphere where individuals may nourish their freedom and uniqueness. What is now to preserve this sanctuary from the onslaught of the "sociological apperception"? While the private sphere remains even for Lukes[83] an indispensable element of genuine individuality, it is the legitimacy of this private sphere that is called into question by the attacks upon abstract individualism.

---

[77] Marvin Zetterbaum, "Self and Political Order," *Interpretation,* Vol. 1, No. 2 (Spring, 1971), 240–246. See also, Lukes, pp. 152–153.

[78] See the controversy on this point between Strauss and Kojève, in Strauss, *On Tyranny,* pp. 165–175, 218–221.

[79] Plato, *Republic,* 472d.

[80] John H. Schaar, "Equality of Opportunity, and Beyond," in *Contemporary Political Theory,* ed. Anthony de Crespigny and Alan Wertheimer (Chicago: Atherton, 1970), p. 151. Cf. Schaar, "Some Ways," p. 877.

[81] Schaar, "Some Ways," pp. 891–892.

[82] *Ibid.,* p. 881. But cf. Schaar, "Equality of Opportunity," p. 152.

According to an ancient tradition, the study of political things appropriately commences, as in the *Politics,* with the family, with marriage and the household. This tradition is continued by Martin Buber. While Buber has of course long been identified with the need for person-to-person encounter or dialogue, for meeting between real individuals, he has also resisted the temptation to confuse such encounter with political life. Buber looks upon marriage, real marriage as an encounter with otherness, one in which the otherness, the particular being, of the partner is affirmed. Marriage thus leads "to insight into the right and legitimacy of otherness and to that vital acknowledgment of many-faced otherness"[84] which is the essential characteristic of social or political life. But while marriage may be a preparation for the otherness of the polity, Buber insists that "there is a difference between the private sphere, to which marriage belongs, and the public sphere of existence." In the private sphere a man may *identify* himself in all his concreteness with the members of his family in all their particularity. When he says, "we" of this family, "he means not merely the whole, but also the single persons recognized and affirmed by him in their particular being." But with respect to the public sphere, identification is qualitatively different, for such identification (this is "my" country; these are "my fellow citizens") cannot "embrace the concrete persons in a concrete way."[85] Buber is even more insistent upon the difference when he comes to develop his concept of the interhuman, of genuine person-to-person relationship:

> We may speak of social phenomenon wherever the life of a number of men, lived with one another, bound up together, brings in its train shared experiences and reactions. But to be thus bound up together means only that each individual existence is enclosed and contained in a group existence. It does not mean that between one member and another of the group there exists any kind of personal relation. They do feel that they belong together in a way that is, so to speak, fundamentally different from every possible belonging together with someone outside the group.... In no case, however, does membership in a group necessarily involve an existential relation between one member and another.[86]

The recognition and affirmation of concrete otherness which may obtain in marriage or friendship, then, is different from the recognition of generalized otherness of political life. To confound the two is to place demands upon the polity which often strain the limits of even the best marriages. It is to confound the polity with an encounter group.

We can legitimately ask whether the political can be understood in any other way than as the locus of the common, of *something* abstracted from each of the individuals encompassed within a given regime. When, in speaking of the transcendence of the concept of abstract individualism, Lukes says that it "can only be achieved on the basis of a view of unabstracted individuals, in their concrete, social specificity...,"[87] he is not, as he is himself aware, abandoning all abstraction. In fact, he contrasts his abstraction, the concept of the *person,* with that of *abstract individualism* which sees the human being as having certain invariant features that determine his behavior.[88] The person, on the other hand, is viewed as having certain capacities, as mentioned above, whose degree and form of realization "is left open for investigation."[89] The concept of the abstract individual is part of a closed system; the concept of the person permits the particularities of the individual to be seen and recognized. But to ground the value of equality in the capacities of every individual for self-development, etc., is of course to abstract such capacities from the individuals concerned. We see individuals differently, then, but we nevertheless see them as having something in common. And we certainly do not affirm the equality of all men with respect precisely to their individual differences.

Perhaps this is only another way of saying that the demand for recognition in public life may take the form of respect, after all, for something understood as common to all – for equality in being, in simple otherness, or in some yet-to-be-discovered characteristic of human beings. But the demand for equality which emerges out of the recognition of concrete individuality is not a legitimate aspiration of political life. If equality cannot be derived from the one aspect of the drive for recognition – the common – it is wholly out of place with respect to the other – the particular. It is of the latter that Schaar has truly noted, "notions of equality have no relevance." "It is easy," he reminds us, "to abstract a part from the whole and define that part in terms that make it commensurable with the same parts abstracted from other whole men. . . . But when it comes to talking about whole men and about man, the concept of equality is mute. Then there is only the mystery of being, the recognition of self and others."[90]

---

[83] Lukes, p. 132.

[84] Martin Buber, *Between Man and Man* (New York: Macmillan, 1965), p. 61.

[85] *Ibid.,* p. 62. Cf. Aristotle, *Politics, 1261b16–1262a14.*

[86] Martin Buber, *The Knowledge of Man* (New York: Harper Torchbooks, 1965), pp. 72–73.

[87] Lukes, p. 153.

[88] *Ibid.,* p. 146.

[89] *Idem.*

[90] Schaar, "Equality of Opportunity," p. 152.

Collective Choice, Separation of Issues and Vote Trading*

THOMAS SCHWARTZ
University of Texas at Austin

The final outcome of a legislative or committee procedure is a *bundle of positions*, one on each of various *issues*. Instead of choosing among such position-bundles directly, legislatures and committees vote on each issue separately.

What effect does this separation of issues have on collective choices?

To investigate this question, I will construct a relatively abstract model of collective choice separated by issues. The main assumption of the model is (roughly) that at least some of the issues voted on separately are *really* separate in the sense that they can be decided independently of one another. Beyond that, the model is quite general. Nothing is assumed, for example, about the procedure used to decide each issue. It might be simple majority rule; then again, it might be something totally outlandish, perverse or undemocratic.

In a way, *vote trading* (log rolling) unseparates separate issues, combining positions on distinct issues to form single legislative packages. I will examine the effects of vote trading and of its absence. Assuming the model, if there exists a feasible outcome not dominated by any other – one to which no other feasible outcome is preferred by a majority, or by whatever counts as a winning coalition – then that outcome will automatically be chosen in the absence of vote trading. If some majorities (or whatever) fail to coalesce to protect the common interests of their members, then it turns out to be rather easy for organized minorities, trading votes, to frustrate the wishes of these disorganized majorities, ensuring the collective choice of an outcome dominated by every other feasible outcome. Otherwise, the chosen outcome must be undominated if any is, and a reasonable choice set or solution concept can be defined for the case where every outcome is dominated.

The assumptions of the model are presupposed (along with much else) by many less abstract models of democratic processes, including spatial models. Somewhat similar models have been investigated by Kadane,[1] Oppenheimer[2] and Bernholz.[3] Mine is considerably more general, and the lessons I draw from its consequences are very different. For example, Oppenheimer and Bernholz have argued that, assuming their models, rational vote trading necessitates a "voting paradox" situation. That is not generally true, although a weaker result can be proved.

The Model

There are n *issues*, $\omega_1, \ldots, \omega_n$, each a set of alternative *positions*. The positions constituting each ω_i are *feasible*, not merely *conceivable*.

---

*I presented earlier drafts of this paper at a Conference on the Foundations of Political Economy, University of Texas at Austin, February 1975, and at the annual meetings of the Public Choice Society, Chicago, April 1975. I profitted from the comments of the discussants, Peter Fishburn, Russel Hardin and Joe Oppenheimer at Austin, and Steven Brams at Chicago.

I am grateful to Joe Oppenheimer for stimulating discussions on the topic on various occasions, and to Martin Baily and Peter Bernholz for discovering a number of important errors. I owe a special debt of gratitude to Bernholz. This paper grew out of an extensive, extremely fruitful correspondence with Prof. Bernholz, occasioned by his paper, "Log-Rolling, Arrow-Paradox and Decision Rules," *Kyklos,* 27 (November, 1974), 49–62.

[1] Joseph B. Kadane, "On Division of the Question," *Public Choice,* 13 (Fall, 1972), 47–54.

[2] Joe A. Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox, or Putting the Fly in the Democratic Pie," paper delivered at the Annual Meeting of the South West Political Science Association, San Antonio, March 30–April 1, 1972.

[3] Peter Bernholz, "Log-Rolling, Arrow-Paradox and Cyclical Majorities," *Public Choice,* 15 (Summer, 1973), 87–102; and Bernholz, "Log-Rolling, Arrow-

In any interesting or realistic example, $n \geqslant 2$ and each ω_i has at least two members. Typically, indeed, each ω_i has *just* two members: *passage* and *defeat* of some motion. While these assumptions hold in all examples used below, there is no need to include them in the model.

Intuitively, a *feasible outcome* is a bundle of positions, one on each of the n issues. Formally, the set Ω of (feasible) outcomes may be defined as the Cartesian product of $\omega_1, \ldots, \omega_n$:

DEFINITION. $\Omega = \overset{n}{\underset{i=1}{\times}} \omega_i$.

Let P be the *collective preference* or *dominance* relation among feasible outcomes. It is a binary relation on Ω. Beyond that, what is it?

We might interpret P as the relation of simple majority preference of some legislature, committee or other voting body. Formally, this means there are, say, v voters (legislators, committee members), with binary preference relations $\rho_1, \ldots, \rho_v$ on Ω, such that:

(1) xPy if there are more than $\frac{v}{2}$ members i of $[1,2, \ldots, v]$ for which $x\rho_i y$.

This is an appropriate way to characterize P when issues are decided by simple majority rule.

Much more generally, we might interpret P as the relation of unanimous preference by any one of certain *winning coalitions* of voters. This means there is a set W such that:

(2) W is a non-empty family of non-empty subsets of $[1,2, \ldots, v]$, and xPy if $x\rho_i y$ holds for some $c \in W$ and for every $i \in c$.

This is an appropriate way to characterize P when every issue is decided by the following *winning coalition rule:* Choose the position voted for by all the members of some winning coalition, if such a position exists; otherwise, choose randomly.

In all examples below, (1) holds, and issues are decided by simple majority rule. But (1) is not one of the assumptions of the model, which is so general that it does not even include (2).

In order to *interpret* the model and its consequences in political terms, however, it is necessary to make some assumption about the interpretation of P. To this end I will assume that P satisfies (2); I will also assume that issues are decided (in effect) according to the winning coalition rule.

The model comprises just two assumptions. The first says P is *separable* or *noncomplementary* with respect to the issues $\omega_1, \ldots, \omega_n$:

ASSUMPTION I. Suppose $1 \leqslant i \leqslant n$, $z,w \in \omega_i$, and $x_j,y_j \in \omega_j$, $j = 1,2, \ldots, n$. Then if

$$(x_1, \ldots, x_{i-1}, z, x_{i+1}, \ldots, x_n)\ P\ (x_1, \ldots, x_{i-1}, w, x_{i+1}, \ldots, x_n),$$

$$(y_1, \ldots, y_{i-1}, z, y_{i+1}, \ldots, y_n)\ P\ (y_1, \ldots, y_{i-1}, w, y_{i+1}, \ldots, y_n).$$

This means, roughly, that if one position on a given issue (say ω_i) is collectively preferred to another, that is independent of the positions taken on the other issues ($\omega_1, \ldots, \omega_{i-1}, \omega_{i+1}, \ldots, \omega_n$). So, besides being voted on separately, $\omega_1, \ldots, \omega_n$ are really separate issues in the sense that they can be decided independently of one another.

Assumption I obviously holds if voter preferences ($\rho_1, \ldots, \rho_v$) are similarly separable and (2) holds.

To be sure, both voter preferences and collective preferences can easily fail to be separable with respect to common issues, witness:

EXAMPLE 1. A committee must vote on two motions:

p – Spend $1,000 on a party.
t – Spend $1,000 on a trip.

So there are two issues, passage vs. defeat of p and passage vs. defeat of t, hence four possible outcomes:

pt – p and t both pass.
$p\bar{t}$ – p passes and t is defeated.
$\bar{p}t$ – p is defeated and t passes.
$\overline{p}\overline{t}$ – p and t are both defeated.

One committee member prefers a $1,000 party to a $1,000 trip, prefers either activity to neither, and prefers having neither activity to spending more than $1,000 (the whole budget) altogether. So she prefers $p\bar{t}$ to $\bar{p}t$, both of these outcomes to $\overline{p}\overline{t}$, and all three of these to pt. Since she prefers $p\bar{t}$ to $\bar{p}t$ but does not prefer pt to $\bar{p}t$, her preferences are not separable. And if a majority share her preferences, then P, interpreted as majority preference, is not separable either.

With $\omega_1, \ldots, \omega_n$ interpreted as *all* the issues voted on, Assumption I is so strong as to be inapplicable to virtually any real-world committee or legislature. But this assumption is quite weak and realistic if $\omega_1, \ldots, \omega_n$ are interpreted simply as *some* n issues *or other* (for some $n \geqslant 2$). Although P is not likely to be

---

Paradox and Decision Rules," cited in acknowledgment note above.

separable with respect to all the issues voted on, it could easily be separable with respect to some two or more of them.

P is a relation among outcomes, not among positions on any issue. But there is a natural way to define, for each issue ω_i, a separate collective preference relation P_i among the alternative positions constituting ω_i:

DEFINITION. For i=1, 2, . . . ,n, xP_iy iff $x,y \in \omega_i$, and there are $z_1, \ldots, z_n$ such that

$$(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n) \, P \, (z_1, \ldots, z_{i-1}, y, z_{i+1}, \ldots, z_n).$$

This equates the following two conditions:

(i) xP_iy.

(ii) $(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n) \, P \, (z_1, \ldots, z_{i-1}, y, z_{i+1}, \ldots, z_n)$ for *some* $z_1, \ldots, z_{i-1}, z_{i+1}, \ldots, z_n$.

It would have been no less natural to equate (i) with:

(iii) $(z_1, \ldots, z_{i-1}, x, z_{i+1}, \ldots, z_n) \, P \, (z_1, \ldots, z_{i-1}, y, z_{i+1}, \ldots, z_n)$ for *all* $z_1, \ldots, z_{i-1} z_{i+1}, \ldots, z_n$ belonging to $\omega_1, \ldots, \omega_{i-1}, \omega_{i+1}, \ldots, \omega_n$, respectively.

Happily, according to Assumption I, (ii) and (iii) are equivalent.

I will sometimes say that one *outcome* (member of Ω) *dominates* another when the first bears P to the second, and that one member of a given *issue*, say ω_i, *dominates* another member of ω_i when the first member bears p_i to the second. Outcomes and positions on issues are *stable* if *undominated.*

Let $m_1, \ldots, m_n$ be the positions that would be taken on the n issues *in the absence of any vote trading* involving $\omega_1, \ldots, \omega_n$.

The second assumption of the model says each m_i directly or indirectly dominates every other position on the i^{th} issue:

ASSUMPTION II. For i=1,2, . . . ,n and for each $x \neq m_i$ in ω_i, there is a finite P_i-chain from m_i to x_i; that is, for some k, there are $x_1, \ldots, x_k$ such that $m_iP_ix_1P_ix_2P_i \ldots x_{k-1} P_ix_k$ and $x=x_k$.

This condition is automatically met when, as is normally the case in real-world committees and legislatures, there are just two positions on each issue – passage and defeat of some motion – and no ties.

I have not assumed that any or every m_i is stable. Consistently with the model, some or every m_i might be dominated. Since every *other* position on every issue is dominated according to Assumption II, if some m_i is dominated as well, then there is no stable position on the i^{th} issue.

Stable Outcomes in the Absence of Vote Trading

Suppose $m_1, \ldots, m_n$ are all individually stable. Then, assuming the model, must Ω contain a stable outcome? If not, *can* Ω contain a stable outcome? If so, can the stable outcomes, when they exist, be characterized in an illuminating way? And can we predict that one of them will be chosen, at least when there is no vote trading?

First, Ω need not contain a stable outcome, even when $m_1, \ldots, m_n$ are all individually stable, witness:

EXAMPLE 2. Three voters, Messrs. 1, 2 and 3, must vote on two motions, a and b, so that there are two issues, passage vs. defeat of a and passage vs. defeat of b, hence four possible outcomes: $a\bar{b}$, $\bar{a}b$, $\overline{ab}$ and ab. Messrs. 1, 2 and 3 rank these outcomes in order of preference as follows:

Mr. 1	**Mr. 2**	**Mr. 3**
$a\bar{b}$	$\bar{a}b$	$\overline{ab}$
ab	ab	$\bar{a}b$
$\overline{ab}$	$\overline{ab}$	$a\bar{b}$
$\bar{a}b$	$a\bar{b}$	ab

As in subsequent examples, voter preferences are linear and separable, and P is the relation of simple majority preference. P has this cyclic form:

$$\begin{tikzcd}
a\bar{b} \arrow[r] & ab \arrow[d] \\
\bar{a}b \arrow[u] \arrow[ur] & \overline{ab} = (m_1 m_2)=(m_1, \ldots, m_n) \arrow[l] \arrow[ul]
\end{tikzcd}$$

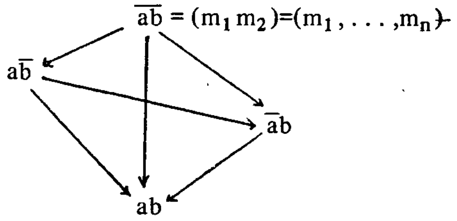
Evidently, there is no stable outcome, although each m_i is stable.

Second, although Ω *need* not contain a stable outcome, it *can* contain one, witness:

EXAMPLE 3. Five voters rank the same four outcomes in order of preference as follows:

Mr. 1	**Mr. 2**	**Mr. 3**	**Mr. 4**	**Mr. 5**
$a\bar{b}$	$a\bar{b}$	$\bar{a}b$	$\bar{a}b$	$\overline{ab}$
ab	$\overline{ab}$	ab	$\overline{ab}$	$a\bar{b}$
$\overline{ab}$	ab	$\overline{ab}$	ab	$\bar{a}b$
$\bar{a}b$	$\bar{a}b$	$a\bar{b}$	$a\bar{b}$	ab

Interpreted again as simple majority preference, P has the form of a linear ordering:



Here, $\overline{ab}$ is the one and only stable outcome.

Notice that the unique stable outcome in this example is also the outcome, $(m_1, \ldots, m_n)$, chosen in the absence of vote trading. This is no accident. In general, when there exists a stable outcome, it must be $(m_1, \ldots, m_n)$, because every other outcome is perforce dominated. Indeed, every other outcome is directly or indirectly dominated by $(m_1, \ldots, m_n)$:

CONSEQUENCE 1. Suppose $x \in \Omega$ and $x \neq (m_1, \ldots, m_n)$.

Then yPx for some y. Indeed, there is a finite P-chain from $(m_1, \ldots, m_n)$ to x; that is, for some k, there are $x_1, \ldots, x_k$ such that $(m_1, \ldots, m_n) \ P x_1 P x_2 \ \ldots \ x_{k-1} P x_k$ and $x_k = x$.[4]

Proof. By induction on the number of dimensions in which x differs from $(m_1, \ldots, m_n)$. Say x differs from $(m_1, \ldots, m_n)$ in the i^{th} dimension (at least). Let x_i be the i^{th} component of x. Then $x_i \neq m_i$. For any $y \in \omega_i$, let x(y) be the result of replacing x_i in x by y. So, unlike x, $x(m_i)$ has the same i^{th} component as $(m_1, \ldots, m_n)$. By inductive hypothesis, there is a finite P-chain from $(m_1, \ldots, m_n)$ to $x(m_i)$. So it suffices to show that there is a finite P-chain from $x(m_i)$ to $x=x(x_i)$. But by Assumption II, there is a finite P_i-chain from m_i to x_i, that is, for some k, there are $y_1, \ldots, y_k \in \omega_i$ such that $y_1=m_i, y_k=mx_i$ and

$$y_1 P_i y_2 P_i \ldots y_{k-1} P_i y_k. \qquad (*)$$

I will show that $x(y_1), x(y_2), \ldots, x(y_k)$ constitute a P-chain from $x(m_i=x(y_1)$ to $x=x(x_i)=x(y_k)$. What must be established, then, is that $x(y_j)Px(y_{j+1})$, $j=1,2,\ldots,k-1$. But by (*) and the definition of P_i, there are $z_1, \ldots, z_n$ such that:

$$(z_1, \ldots, z_{i-1}, y_j z_{i+1}, \ldots, z_n) \ P$$
$$(z_1, \ldots, z_{i-1}, y_{j+1}, z_{i+1}, \ldots, z_n).$$

Hence, by Assumption I, $x(y_j)Px(y_{j+1})$, q.e.d.

So the outcome chosen in the absence of vote trading has got to be stable if any is. This is an interesting result about issue-separated voting under the model, inasmuch as most election rules (plurality, run-off, Borda, etc.) do not guarantee the choice of a stable outcome *even when one exists* (and voters vote sincerely). In fact, the very oldest problem of social choice theory – the principal problem addressed by Borda, Condorcet, Dodgson and Nanson – is precisely to find a voting rule that picks a stable outcome when one exists (and voters vote sincerely).[5]

Stable Outcomes and Vote Trading

Even when $(m_1, \ldots, m_n)$ – the outcome chosen in the absence of vote trading – is uniquely stable, *vote trading* can lead to the choice of a *different* outcome.

In Example 3, $\overline{ab} = (m_1, m_2)$ would be chosen in the absence of vote trading. But suppose Messrs. 1 and 3 and they alone trade votes, Mr. 1 agreeing to vote for b in return for Mr. 3's agreement to vote for a. Then the chosen outcome is ab.

That seems anomalous. Far from being stable, ab is dominated by every other outcome. Yet Messrs. 1 and 3 have evidently behaved rationally in trading votes, bringing about what is for them a better outcome (ab) than would be chosen ($\overline{ab}$) in the absence of vote trading.

What has happened is that an *organized minority* (Messrs. 1 and 3) has frustrated the wishes of the *disorganized majority* (Messrs. 2, 4 and 5). Had Messrs. 2 and 4 also traded votes, Mr. 2 agreeing to vote against a in return for Mr. 4's agreement to vote against b, they would have blocked the effect of the first trade, insuring the choice of $\overline{ab}=(m_1, \ldots, m_n)$, which both of them prefer to ab – as does Mr. 5. If *cooperation costs* are such as to permit Messrs. 1 and 3 to trade votes while preventing Messrs. 2 and 4 from trading votes, then the will of the majority is frustrated in a striking way. It is perhaps an awareness of this type of situation that explains many of the complaints we hear about democratic governments serving special interests rather than the public interest.

The example can be generalized: *If* $(m_1, \ldots, m_n)$ *– the outcome chosen in the absence of vote trading – is stable, and if cooperation costs do not keep winning coali-*

---

[4] More or less this result was proved, in a less general form, by Kadane, "On Division of the Question."

[5] On the early history of social choice theory, see Duncan Black, *The Theory of Committees and Elections* (Cambridge: Cambridge University Press, 1963), Part II.

tions from cooperating, then vote trading cannot block the choice of $(m_1, \ldots, m_n)$.

Proof: Suppose $(m_1, \ldots, m_n)$ is stable and yet there is some chance of bringing about an outcome x, other than $(m_1, \ldots, m_n)$, by vote trading. By Consequence 1, however, there is a finite P-chain from $(m_1, \ldots, m_n)$ to x. For the sake of illustration, say:

$$(m_1, \ldots, m_n)PyPzPx.$$

Then there are winning coalitions Cm, Cy and Cz whose members prefer $(m_1, \ldots, m_n)$ to y, y to z and z to x, respectively.

Since there is a chance of choosing x, the members of Cz have some incentive to agree to vote for the positions constituting z, thereby bringing about the choice of z instead of x. But then, there is a chance of choosing z, and so the members of Cy have an incentive to agree to vote for the positions constituting z, thereby bringing about the choice of z instead of x. But then, there is a chance of choosing z, and so the members of Cy have an incentive to agree to vote for the positions constituting y, thereby bringing about the choice of y instead of z. And this gives the members of Cm as an incentive to agree to vote for $m_1, \ldots, m_n$, thereby bringing about the choice of $(m_1, \ldots, m_n)$ instead of y. Since $(m_1, \ldots, m_n)$ is stable, no winning coalition has any incentive to block the choice of $(m_1 \ldots, m_n)$.

That our ability to predict the choice of a stable $(m_1, \ldots, m_n)$ depends upon cooperation costs being small is a significant limitation. Cooperation costs can be great. They include:

- *Information costs.* These are the costs of making potential cooperators aware of the opportunity to benefit from cooperation.
- *Transaction costs.* These are the costs of working out an agreement.
- *Fidelity costs.* These are the costs of cooperating with partners who are less than fully trustworthy.
- *Opprobrium costs.* These are the psychic costs of being involved in an arrangement one finds (somewhat) distasteful.

Vote Trading and the Paradox of Voting

Peter Bernholz[6] and Joe Oppenheimer[7] have argued that vote trading entails the "paradox of voting," assuming a model like mine but less general. This point has recently been aborated in the exchange among Koehler,[8] Bernholz[9] and Oppenheimer.[10]

What is the "paradox of voting"? The label is sometimes applied to *any nontransitivity* in the collective preference relation (our P) or the corresponding relation of collective indifference – the relation between outcomes not related by P in either direction. Bernholz and Oppenheimer apply the label just to *collective preference cycles* (P-cycles). This makes their thesis much more interesting. The thesis becomes more interesting still if the label "paradox of voting" is applied just to situations in which none of the feasible outcomes (no member of Ω) is stable.[11]

The following seems to be the most interesting interpretation (though not necessarily the most favorable) of the Bernholz-Oppenheimer thesis:

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then it is advan-

---

[6] Bernholz, "Log-Rolling, Arrow-Paradox and Cyclical Majorities," and "Log-Rolling, Arrow-Paradox and Decision Rules."

[7] Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox."

[8] David H. Koehler, "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," *American Political Science Review*, 69 (September, 1975), 954–960; and "Vote Trading and the Voting Paradox: Reply," *American Political Science Review*, 69 (September, 1975).

[9] Peter Bernholz, "Logrolling and the Paradox of Voting: Are They Really Logically Equivalent?" *American Political Science Review*, 69 (September, 1975), 961–962.

[10] Joe A. Oppenheimer, "Some Political Implications of 'Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence,'" *American Political Science Review*, 69 (September, 1975), 963–966.

The thesis stated in this paper and in Bernholz, "Logrolling, Arrow-Paradox and Decision Rules," is more general than that of Oppenheimer, "Relating Coalitions of Minorities to the Voter's Paradox," and Bernholz, "Logrolling, Arrow Paradox and Cyclical Majorities." Koehler in "Vote-Trading and the Voting Paradox: A Proof of Logical Equivalence," restates the argument for the special three-voter example of William H. Riker and Steven J. Brams, "The Paradox of Vote Trading," *American Political Science Review*, 67 (December, 1973), 1235–1247, then makes a questionable attempt to generalize it.

Bernholz, "Logrolling and the Paradox of Voting: Are They Really Logically Equivalent?" points out that Koehler's general formulation is too general and that Koehler falsely assumes there are no P-cycles if there are no *three-element* P-cycles. Koehler, "Vote-Trading and the Voting Paradox: Reply" responds by citing a trivial fact of elementary logic: there are no P-intransitivities if there are no *three-element* P-intransitivities. True. But not every P-*intransitivity* is a P-*cycle*.

[11] If nothing in Ω is stable, there must exist a P-cycle. But not conversely. A P-cycle can coexist with a stable outcome. Of course, if there is a P-cycle, then, even if Ω itself has a stable member, some *subset* of Ω must lack a stable member.

tageous for some voters to trade votes with one another on issues drawn from among $\omega_1, \ldots, \omega_n$, only if Ω contains no stable outcome.

This is simply false. It remains false if we replace the clause "Ω contains no stable outcome" by "P is cyclic" or even "there is some nontransitivity in P or in the collective indifference relation." And the thesis remains false even if we further weaken it by restricting it to particularly simple, realistic cases, in which: each issue comprises just two positions; each voter has a linear, separable preference ordering; P is the relation of simple majority preference; each issue is decided by simple majority rule; and there are no ties on any issue and no collective indifferences between outcomes.

In Example 3, Messrs. 1 and 3 alone might trade votes (to their advantage but to the disadvantage of Messrs. 2 and 4), or Messrs. 2 and 4 might trade votes to block the effect of the first trade, and yet $\overline{ab} = (m_1, \ldots, m_n)$ is stable. Indeed, P is a linear ordering, so that there is no collective nontransitivity of any sort.

To be sure, if Messrs. 1 and 3 alone trade votes, this can only be because cooperation costs have prevented the remaining majority – or at least Messrs. 2 and 4 – from cooperating. But we cannot save the thesis by restricting it further to cases where cooperation costs are zero.

In Example 3, suppose voters are *fully informed* of all relevant features of the situation and *fully rational* in the sense of maximizing expected advantage, and suppose cooperation costs are *zero.* Then Messrs. 2 and 4 (at least) must trade votes, although there are no collective nontransitivities of any sort.

To see why, suppose, on the contrary, that Messrs. 2 and 4 did *not* trade votes. Then they must have either decided definitely not to trade votes or else adopted a mixed strategy involving some likelihood of not trading. Either way, it is then to the expected advantage of Messrs. 1 and 3 to trade votes themselves, inasmuch as cooperation costs are zero. Therefore, since they are fully informed and rational, Messrs. 1 and 3 will certainly trade votes. But then, since cooperation costs are zero, it is to the advantage of Messrs. 2 and 4 to trade votes after all. And so, since they are fully informed and rational, Messrs. 2 and 4 will certainly trade votes.

The situation is depicted in the following payoff matrix, in which vote trading is a dominant strategy for all concerned:

		Messrs. 1 and 3	
		Trade	Don't trade
Messrs. 2 and 4	Trade	0 1 $a\overline{b}$	0 1 $\overline{ab}$
	Don't trade	10 −1 ab	0 1 $\overline{ab}$

Notice that the vote trade involving Messrs. 1 and 3 is *opposed by a majority,* while the trading involving Messrs. 2 and 4 is supported by a majority in order *not to overturn* but *to sustain* the choice of $\overline{ab} = (m_1, \ldots, m_n)$. The Bernholz-Oppenheimer thesis is true when restricted to instances of vote trading supported by majorities – or whatever count as winning coalitions – in order to overturn $(m_1, \ldots, m_n)$:

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then Ω contains no stable outcome if there is a potential vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$ such that the members of some winning coalition all prefer that trade (i.e., the outcome issuing therefrom) to $(m_1, \ldots, m_n)$.[12]

As it happens, the converse also is true. Once correctly stated, the Bernholz-Oppenheimer thesis holds in both directions:

Suppose Assumptions I and II hold, P satisfies (2), and issues are decided according to the winning coalition rule. Then Ω contains no stable outcome if, and only if, there is a potential vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$ and preferred to $(m_1, \ldots, m_n)$ by all the members of some winning coalition.

Proof: Suppose the members of some winning coalition all prefer some vote trade, involving issues drawn from among $\omega_1, \ldots, \omega_n$, to $(m_1, \ldots, m_n)$. Let x be the outcome resulting from that trade. Then $xP(m_1, \ldots, m_n)$. But by Consequence I, every outcome other than $(m_1, \ldots, m_n)$ is dominated. Hence, every outcome is dominated.

Conversely, suppose no outcome is stable. Then $xP(m_1, \ldots, m_n)$ for some x. Therefore, the members of some winning coalition c all prefer x to $(m_1, \ldots, m_n)$. By agreeing to vote for the positions constituting x, the members of c can insure the choice of x rather than $(m_1, \ldots, m_n)$. But $(m_1, \ldots, m_n)$ is the outcome chosen in the absence of any vote trading involving $\omega_1, \ldots, \omega_n$. Consequently, by agreeing to support the positions constituting x, the members of c (some of them, anyway) must have engaged in a vote trade involving issues drawn from among $\omega_1, \ldots, \omega_n$, and so must prefer that vote trade to $(m_1, \ldots, m_n)$.

---

[12] See Thomas Schwartz, "On the Possibility of Rational Policy Evaluation," *Theory and Decision,* 1 (January, 1970), 1–25; and "Rationality and the Myth of the Maximum," *Noûs,* 7 (May, 1972), 97–117.

As you will see in the final section of this paper, Assumptions I and II are essential to the revised thesis. We cannot drop either assumption without making the thesis false. Still, we can *weaken* Assumption II, as follows:

ASSUMPTION II*. For i=1,2, . . . ,n and for each $x \neq m_i$ in ω_i, yP_ix for some $y \in \omega_i$.

With Assumption II replaced by II*, we can no longer deduce Consequence 1. But we can deduce:

CONSEQUENCE 1*. Suppose $x \in \Omega$ and $x \neq (m_1, \ldots, m_n)$. Then yPx for some y.

And this is all I actually used to demonstrate the thesis.

One should be cautious about drawing from this thesis any lessons concerning the nature of vote trading, the nature of stability in collective choice, or the likelihood of there being no stable outcome (of the "paradox of voting"), for three reasons:

First, the thesis is conditional upon Assumptions I and II*.

Second, the thesis concerns only a special type – even if an especially prevalent type – of vote trading.

Third, even among vote trades of this type, only those that involve issues drawn from among $\omega_1, \ldots, \omega_n$ are said to imply the nonexistence of a stable outcome in Ω. The thesis says nothing about vote trades involving issues other than $\omega_1, \ldots, \omega_n$.

True, we can interpret $\omega_1, \ldots, \omega_n$ as *all* the issues voted on, in which case no vote trade can involve other issues. But that would make Assumption I (separability) so strong as to be virtually inapplicable in the real world.

On the other hand, Assumptions I and II (or II*) are plausible if $\omega_1, \ldots, \omega_n$ are interpreted as special interest or local interest issues – issues concerning river and harbor improvements, for example. And it is realistic to suppose there is some vote trading, even of the special type lately remarked, involving such issues.

When No Outcome Is Stable

Given Assumptions I and II, if there is a stable outcome, it must be $(m_1, \ldots, m_n)$, and it must be chosen unless cooperation costs keep the members of some winning coalition from cooperating. But, of course, there need be no stable outcome. Even $(m_1, \ldots, m_n)$ might be dominated. What then happens? This question is partly answered by the following consequence of the model:

CONSEQUENCE 2. If $xP(m_1, \ldots, m_n)$ and $x \neq (m_1, \ldots, m_n)$, there is a unique undominated P-cycle γ – i.e., a unique P-cycle γ such that nothing in $\Omega - \gamma$ bears P to anything in γ – and $(m_1, \ldots, m_n) \in \gamma$.

Proof. By Consequence 1, there is a finite P-chain from $(m_1, \ldots, m_n)$ to x, i.e., for some k, there are $y_1, \ldots, y_k$ such that $(m_1, \ldots, m_n) Py_1Py_2P \ldots y_{k-1}Py_k$ and $y_k = x$. Hence, since $y_kP(m_1, \ldots, m_n)$, the set $[(m_1, \ldots, m_n), y_1, \ldots, y_k]$ is a P-cycle. So there is at least one P-cycle containing $(m_1, \ldots, m_n)$. Since all such P-cycles overlap in at least one element, and since the union of overlapping P-cycles must itself by a P-cycle, the union – call it U – of P-cycles containing $(m_1, \ldots, m_n)$ is also a P-cycle containing $(m_1, \ldots, m_n)$. I will show that U is a unique undominated P-cycle. Since U is a P-cycle, it suffices to show that U is *undominated* and *that no other* P-cycle *is* undominated.

To prove U undominated, suppose yPz for some $z \in U$; I will show that $y \in U$. If $y=(m_1, \ldots, m_n)$, $y \in U$ by construction. Otherwise, by Consequence 1, there is a P-chain from $(m_1, \ldots, m_n)$ to y. But since yPz for some $z \in U$, and U is a P-cycle containing $(m_1, \ldots, m_n)$, there is another P-chain from y back to $(m_1, \ldots, m_n)$. Therefore, there is a P-cycle containing both y and $(m_1, \ldots, m_n)$, and thus, by construction, $y \in U$.

To prove that no other P-cycle is undominated, suppose α is an undominated P-cycle; I will show that α=U. By Consequence 1, there is a P-chain from $(m_1, \ldots, m_n)$ to some member of α. Thus, since α is undominated, $(m_1, \ldots, m_n) \in \alpha$, so that α is a P-cycle containing $(m_1, \ldots, m_n)$, whence, by construction, $\alpha \subseteq U$, and it suffices to show that $U \subseteq \alpha$. Suppose not. Then since $\alpha \subsetneq U$ and U is a P-cycle, something in $U - \alpha$ must bear P to something in α, contrary to the undominated character of α. Hence, $U \subseteq \alpha$, q.e.d.

Suppose cooperation costs do not prevent winning coalitions from cooperating, and suppose $(m_1, \ldots, m_n)$ is dominated. What can we predict about the chosen outcome? We can certainly predict that *the chosen outcome will belong to the unique undominated P-cycle* –

call it γ – of whose existence we are assured by Consequence 2.

For suppose, on the contrary, there were some chance of choosing an outcome x foreign to γ. By Consequence 1, there is a finite P-chain from $(m_1, \ldots, m_n)$ to x. For the sake of illustration, say:

$$(m_1, \ldots, m_n)PyPx.$$

Then there are winning coalitions Cm and Cy whose members prefer $(m_1, \ldots, m_n)$ to y and y to x, respectively. Since there is some chance of choosing x, and the members of Cy can afford to cooperate to get a better outcome, the members of Cy have an incentive to agree to vote so as to bring about the choice of y rather than x. But then, there is a chance of choosing y, and so the members of Cm have an incentive to support $(m_1, \ldots, m_n)$.

Because $(m_1, \ldots, m_n)$ is dominated, the chain does not stop here. There is a winning coalition whose members have an incentive to block the choice of $(m_1, \ldots, m_n)$ in favor, say, of z; another winning coalition whose members have an incentive to block the choice of z in turn; and so on.

Because γ is a cycle, the chain never ends, and it is hard to make any certain prediction about the final choice among members of γ. But because γ is undominated, the chain never leads back outside γ, and so we can certainly predict that the chosen outcome will belong to γ.

This suggests defining the choice set or solution set from Ω as follows:

$$\text{DEFINITION. } \Gamma = \begin{cases} [(m_1, \ldots, m_n)], & \text{if nothing bears P to } (m_1, \ldots, m_n); \\ \text{the unique undominated P-cycle,} & \text{otherwise.} \end{cases}$$

Example 3 casts doubt on the possibility of saying anything very interesting about the final choice when cooperation costs prevent some winning coalitions from cooperating. Otherwise, the final choice must certainly belong to Γ.

There are various ways this prediction might be refined, depending on circumstances. I examine some refinements later. But I know of no decisive reason for generalizing any of these refinements to all relevant situations, i.e., for postulating any of them as part of a *general* model of collective choice separated by issues.

Comparison with Known Solution Concepts

According to one well-known proposal, the choice set or solution set from Ω is the set $C(\Omega)$ defined by this condition:

BINARY CHOICE PROPERTY (BICH). $C(\Omega) \equiv [x \in \Omega \,|, yPx \text{ for no } y \in \Omega\}$.

This equates $C(\Omega)$ with the set of stable outcomes – with the *core,* in game theoretic terms. The trouble is, there may be no stable outcome, as Example 2 illustrates. Even $(m_1, \ldots, m_n)$ might be dominated.

To get round this difficulty, I have proposed elsewhere[12] that we 'generalize' BICH as follows:

GENERAL OPTIMAL CHOICE AXIOM (GOCHA). $C(\Omega) \equiv$ the union of minimum undominated subsets of Ω,

where:

> an *undominated subset* of α is any non-empty subset β of α for which nothing in $\alpha - \beta$ bears P to anything in β

and:

> a *minimum* undominated subset of α is one that is not a proper superset of another.

Like BICH, GOCHA defines $C(\Omega)$ in terms of P. Like BICH, GOCHA equates $C(\Omega)$ with the set of stable outcomes when P is acyclic. Unlike BICH, GOCHA guarantees the *non-emptiness* of $C(\Omega)$, independently of any properties of P – hence even if no outcome is stable.

It can be shown that $C(\Omega)$ must satisfy GOCHA if it is to represent any of certain important types of choice process. For example, assuming certain rather mild conditions, it is known that $C(\Omega)$ must satisfy GOCHA if $C(\Omega)$ comprises just those elements of Ω that can be chosen from Ω by any of a wide range of common (and not so common) *serial* (sequential) choice processes.[13]

The significance of GOCHA for present purposes is this: GOCHA implies that $C(\Omega)=\Gamma$. In other words, the suggested choice set Γ, defined earlier, is also the set defined by GOCHA. This follows from the fact that Γ is the one and only minimum undominated subset of Ω, which is trivial to prove.

---

[13] Thomas Schwartz, "Serial Collective Choice," paper presented at the annual meeting of the Public Choice Society, New Haven, April 1974.

Is Γ Too Inclusive?

Some would reject Γ as an appropriate choice set on the ground that it is *too inclusive* when ($m_1, \ldots, m_n$) is dominated. Take Example 2. There, ($m_1, \ldots, m_n$) is dominated, and P is the whole set Ω of feasible outcomes.

But the mere fact that Γ can sometimes encompass all of Ω is not a decisive reason for rejecting Γ as an appropriate choice set. It may just happen, in some cases, that every feasible outcome has some chance of being chosen, even apart from cooperation costs.

Nor is it *necessary* that Γ exhaust Ω. If ($m_1, \ldots, m_n$) is stable, Γ is the unit set [($m_1, \ldots, m_n$)]. And even if there is no stable outcome, Γ still need not comprise all of Ω, witness:

EXAMPLE 4. Three voters have the following preferences among eight outcomes:

Mr. 1	**Mr. 2**	**Mr. 3**
$a\bar{b}c$	$ab\bar{c}$	$\bar{a}bc$
$\overline{ab}c$	$\bar{a}b\bar{c}$	$\overline{ab}c$
abc	$a\overline{bc}$	abc=($m_1, \ldots, m_n$)
$\bar{a}bc$	$\overline{abc}$	$a\bar{b}c$
$a\overline{bc}$	abc	$\bar{a}b\bar{c}$
$\overline{abc}$	$\bar{a}bc$	$ab\bar{c}$
$ab\bar{c}$	$a\overline{bc}$	$\overline{abc}$
$\overline{ab}c$	$a\bar{b}c$	$a\overline{bc}$

Here, $\Gamma \neq [(m_1, \ldots, m_n)]$ and $\Gamma \neq \Omega$. Rather, $\Gamma = [\overline{ab}c, \bar{a}bc, abc, a\bar{b}c]$.

What will doubtless distress many theorists is that Γ can contain *Pareto inefficient* outcomes – outcomes to which other outcomes are unanimously preferred – as in this example:

EXAMPLE 5. Three voters have the following preferences among eight outcomes:

Mr. 1	**Mr. 2**	**Mr. 3**
$a\bar{b}c$	$\bar{a}bc$	$\overline{abc}$-($m_1, \ldots, m_n$)
$a\overline{bc}$	$\bar{a}b\bar{c}$	$\overline{ab}c$
abc	abc	$a\bar{b}c$
$ab\bar{c}$	$ab\bar{c}$	$a\overline{bc}$
$\overline{ab}c$	$\overline{ab}c$	$\bar{a}bc$
$\overline{abc}$	$\overline{abc}$	$\bar{a}b\bar{c}$
$\bar{a}bc$	$a\bar{b}c$	abc
$\bar{a}b\bar{c}$	$a\overline{bc}$	$ab\bar{c}$

Here, Ω is one big P-cycle. So Γ = Ω. But fully four of the eight outcomes in Γ = Ω are Pareto inefficient: $a\overline{bc}$, $ab\bar{c}$, $\overline{abc}$ and $\bar{a}b\bar{c}$. In fact, one of these outcomes, $\overline{abc}$, is *Pareto dominated* by (i.e., unanimously dispreferred to) none other than $\overline{abc}$=($m_1, \ldots, m_n$).

It is not obvious to me, however, that we should automatically predict the rejection of Pareto inefficient outcomes, hence not obvious that they should be banished from our solution set. They are, in a sense, unanimously opposed. But not in the sense that voters can unanimously agree on an alternative. Nor even in the sense that voters can unanimously agree to reject them. It would be unwise for Mr. 1 to agree to reject $\overline{abc}$, for example, unless his preference for $a\bar{b}c$ over $a\overline{bc}$ were comparatively very intense or $a\bar{b}c$ were especially likely to be chosen after the rejection. And if unanimous opposition is enough to reject an outcome, why isn't opposition by *any winning coalition* enough? After all, a winning coalition has the power to enforce any choice. When Γ is cyclic, however, to reject every member of Ω opposed by some winning coalition is to reject every member of Ω.

There are rationales for predicting the rejection of Pareto inefficient outcomes. I discuss some in the next section. None, I think, is applicable in all legislative situations.

Tullock[14] and Riker and Brams[15] have argued, more positively, that vote trading can indeed lead to an outcome Pareto dominated by ($m_1, \ldots, m_n$), making every voter worse off than he would be in the absence of vote trading. They offer examples in which, although there are many issues, voters trade votes on relatively few issues at a time, thereby allowing themselves to incur external costs from other trades by other voters on other issues. Because these external costs outweigh the benefits of trading *for each voter,* the outcome achieved is Pareto dominated by ($m_1, \ldots, m_n$).[16]

Besides cases in which an outcome in Γ is Pareto dominated by ($m_1, \ldots, m_n$), there also are cases in which ($m_1, \ldots, m_n$) itself is Pareto dominated by another outcome in Γ, achievable by vote trading. So vote trading can make every voter *better* off than he would be in the absence of vote trading.

---

[14] Gordon Tullock, "Problems of Majority Voting," *Journal of Political Economy,* 67 (December, 1959), 571–579, reprinted in *Freedom and Authority: An Introduction to Social and Political Philosophy,* ed. Thomas Schwartz (Encino, Ca.: Dickenson, 1973), pp. 295–303.

[15] Riker and Brams, "The Paradox of Vote Trading."

[16] Tullock's argument is criticized by Anthony Downs, "In Defense of Majority Voting," *Journal of Political Economy,* 69 (April, 1961), 192–199, reprinted in *Freedom and Authority,* ed. Schwartz, pp. 304–313, and defended against Downs's criticism by Gordon Tullock, "Reply to a Traditionalist," *Journal of Political Economy,* 69 (April, 1961), 200–203.

EXAMPLE 6. Our old friends, Messrs. 1, 2 and 3, must vote on three measures – a, b and c – to which they assign the following dollar values:

	Mr. 1	Mr. 2	Mr. 3
a	500	500	−1000
b	−1000	500	400
c	400	−1000	500

Observe that each measure benefits two voters at a considerable cost to the third. In each case, the net cost to the nonbeneficiary, measured in dollars, is greater than the combined net benefit to the two beneficiaries – owing, perhaps, to transfer costs. Each voter ranks the eight feasible outcomes according to their total dollar values for him:

Mr. 1	Mr. 2	Mr. 3
$a\bar{b}c$	$ab\bar{c}$	$\bar{a}bc$
$a\bar{b}\bar{c}$	$\bar{a}b\bar{c}$	$\bar{a}\bar{b}c$
$\bar{a}\bar{b}c$	$a\bar{b}\bar{c}$	$\bar{a}b\bar{c}$
$\overline{abc}$	$\overline{abc}$	$\overline{abc}$
abc	abc	$abc=(m_1,\ldots,m_n)$
$ab\bar{c}$	$\bar{a}bc$	$a\bar{b}c$
$\bar{a}bc$	$a\bar{b}c$	$ab\bar{c}$
$\bar{a}b\bar{c}$	$\bar{a}\bar{b}c$	$a\bar{b}\bar{c}$

Here, $\Gamma = \Omega$, and $(m_1,\ldots,m_n)=abc$ is Pareto dominated by $\overline{abc}$. In fact, abc is the *only* Pareto-inefficient outcome. But if Messrs. 1, 2 and 3 trade votes, each agreeing to vote against all three measures, they can ensure the choice of $\overline{abc}$, *making every voter better off than he would be in the absence of vote trading.* (Of course, other outcomes also can be achieved by vote trading.)

Paring Down Γ

Since Γ can be distressingly large, one might try to pare it down by deleting outcomes fulfilling certain exclusion conditions, such as Pareto inefficiency, when Γ is a P-cycle.

An attractive proposal is to delete those outcomes opposed by the largest winning coalitions, or those outcomes dominated by the largest number of other outcomes, or those outcomes with the largest combined voter-opposition. That is, one might propose paring down Γ by deleting those outcomes x that fulfill one of the following three conditions (provided, of course, that this does not exclude *all* members of Γ):

(XC_1) There exists a $c \epsilon W$ such that $y\rho_i x$ for some $y \epsilon \Gamma$ and every $i \epsilon c$, and such that, for all $c' \epsilon W$, if $z\rho_i w$ for some z, $w \epsilon \Gamma$ and all $i \epsilon c'$, then c is no smaller than c'.

(XC_2) For every $y \epsilon \Gamma$, $[z \mid zPx]$ is no smaller than $[z \mid zPy]$.

(XC_3) For every $y \epsilon \Gamma$, $[c \epsilon W \mid$ for some z, $z\rho_i x$ for every $i \epsilon c]$ is no smaller than $[c \epsilon W \mid$ for some z, $z\rho_i y$ for every $i \epsilon c]$.

Notice that (XC_1) and (XC_3) are automatically satisfied by *Pareto inefficient* outcomes.

Why predict the rejection of outcomes fulfilling one of these maximum opposition conditions? Maybe because the voters in question are elected legislators who wish to minimize opposition within the larger electorate by supporting minimally opposed (or maximally popular) platforms.

But even if the legislature were a fair microcosm of the larger electorate, size of opposition within the legislature would not necessarily reflect size of opposition within most legislative constituencies. Nor is it likely that all legislative behavior can be explained by the very simple goal of minimizing constituent-opposition. Besides, *small* winning coalitions sometimes reflect small but *very well organized* blocs of constituents, making outcomes they oppose unattractive to legislators concerned to keep their jobs, hence no less reasonable candidates for exclusion from the solution set. Anyway, the "voters" in our model might not be elected legislators.

My point is not that opposition-minimization (or support-maximization) models of legislative behavior are, in any sense, Bad Things. It is just that they are inapplicable in many collective choice situations characterized by Assumptions I and II.

More interesting, I think, is the proposal that we banish from Γ those outcomes x preferred to others only by winning coalitions that prefer yet other outcomes to x:

(XC_4) For every $c \epsilon W$, if there is a $y \epsilon \Gamma$ such that $x\rho_i y$ for all $i \epsilon c$, then there is a $z \epsilon \Gamma$ such that $z\rho_i x$ for all $i \epsilon c$.

Suppose Γ is a P-cycle and an element x of Γ satisfies (XC_4). Why couldn't x be the chosen outcome? Since Γ is a P-cycle, xPy for some $y \epsilon \Gamma$, and so the members of some winning coalition c all prefer x to y. Why couldn't the members of c support x to block the choice of y? One might argue that it would be irrational for the members of c to support x, for this reason: Because x satisfies (XC_4), there is a third outcome, z, which the members of c all prefer to x, and hence to y as well. So, in agreeing to support x, the members of c would be making a less advantageous agreement than they might have made. They would be better off agreeing to z. That is why one might predict the rejection of x.

A nice feature of (XC_4) is that it is satisfied by all *Pareto inefficient* members of Γ, so that the deletion from Γ of outcomes satisfying (XC_4) insures the deletion of Pareto inefficient outcomes. To prove this, suppose x is a Pareto inefficient member of Γ, i.e., $x \in \Gamma$ and some $y \in \Gamma$ is unanimously preferred to x. Let c by any winning coalition whose members all prefer x to another outcome. It suffices to show that there is yet a third outcome preferred by all the members of c to x. But y is such an outcome.

Can we predict the rejection of outcomes satisfying (XC_4)? Upon what model of voter behavior might such a prediction be based?

The above argument that it would be irrational for the members of any winning coalition to support an outcome satisfying (XC_4) seems to rest on the following model of rational voter behavior: *A rational voter will not accept less than the best voting agreement he can get; he will not make any agreement if a better agreement is available.*

But if winning coalitions always overlap somewhat (as, for example, majorities always do), then this model precludes *any* rational agreement by *any* winning coalition to support *any* outcome, when $(m_1, \ldots, m_n)$ is dominated. To see why, let x be any outcome, assume the above model of rational voter behavior, and assume that winning coalitions always overlap somewhat; I will show that no winning coalition Cx can rationally support x if $(m_1, \ldots, m_n)$ is dominated. By Consequence 1, if $(m_1, \ldots, m_n)$ is dominated, then every outcome is dominated, whence yPx for some y. So the members of some winning coalition Cy all prefer y to x. If the members of Cx all agree to vote for x, some of them would be accepting less than the best agreement they could get, contrary to the assumed model: Those members of Cx who also belonged to Cy could make a better agreement by agreeing with the other members of Cy to support y rather than x. Hence, there is no winning coalition whose members can rationally agree to support x.

On the other hand, one might reasonably predict the rejection of outcomes satisfying (XC_4) in circumstances in which *voters have a very strong incentive to make binding agreements and stick to them* – even at the cost of foregoing a more desired outcome. For suppose the members of some winning coalition c make a binding agreement to support x. And suppose x satisfies (XC_4). Then the members of c all prefer another outcome, y, to x. While their binding agreement prevents them from breaking faith with one another, it does *not* prevent them from deviating from the agreement in order to support y, since they would presumably *all consent* to such a deviation. To *deviate* from an agreement is not to *violate* it if all parties consent. The most binding of agreements allow deviation by mutual consent. Consequently, y will probably be supported in place of x. In general, a binding agreement to support an outcome is likely to be deviated from when, and only when, the outcome satisfies (XC_4).

Circumstances might make it possible to pare down Γ in any of a number of ways. And instead of refining our prediction about the final choice by paring down Γ, we might do it by means of a nicely humped probability distribution over Γ. Either way, appropriate refinements seem to depend on circumstances. I do not think they can be incorporated in a widely applicable model of collective choice separated by issues.

The Desirability of Vote Trading; Relaxing the Model

I conclude on a normative note. Assume the model we have been examining. In the absence of vote trading, $(m_1, \ldots, m_n)$ is the chosen outcome. If vote trading occurs, the chosen outcome might still be $(m_1, \ldots, m_n)$ – neither an advantage nor a disadvantage. It might also be some other member of Γ – no disadvantage from *my* point of view, though no advantage either. But vote trading could instead lead to an outcome foreign to Γ, as in Example 3, and that is surely a serious *disadvantage* of vote trading.

Before concluding that vote trading is an undesirable practice, bear in mind three things:

Thing 1. Although vote-trading can lead to a Pareto inefficient outcome in Γ (which does not distress me), so can the *failure* to trade votes: $(m_1, \ldots, m_n)$, the outcome chosen in the absence of vote trading, can be Pareto dominated by another outcome, achievable through vote-trading, as in Example 6.

Thing 2. It would be extremely costly, perhaps impossible, to *prevent* vote trading. It is a well-entrenched practice. It is hard to detect. You can engage in it in the privacy of your bedroom. It is sometimes tacit, or implicit.

On the other hand, since the worst anomalies occur when cooperation costs prevent some winning coalitions from trading votes, the cheapest way to preclude them may simply be to minimize such costs: to make vote trading easier rather than harder, to facilitate the practice rather than penalize it.

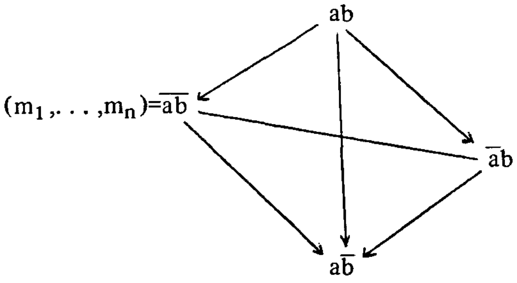
Thing 3. The argument that vote trading is never socially advantageous and sometimes socially disadvantageous rested on Assumptions I and II. But these are strong assumptions. The real utility of vote trading becomes apparent only when these assumptions are relaxed.

Suppose we drop Assumption I, allowing nonseparable collective preferences, and therewith nonseparable voter preferences. Then vote trading might actually be necessary to prevent a patently anomalous collective choice, as in this example:

EXAMPLE 7. Three voters have the following preferences among four outcomes:

Mr. 1	**Mr. 2**	**Mr. 3**
$\overline{ab}$	$a\bar{b}$	$\bar{a}b$
ab	ab	ab
$\bar{a}b$	$\overline{ab}$	$\overline{ab}$
$a\bar{b}$	$\bar{a}b$	$a\bar{b}$

Interpreted as simple majority preference, P is a linear ordering, but *nonseparable* – contrary to Assumption I. It looks like this:



I would contend that ab – which dominates every other outcome – should be the final choice. In the absence of vote trading, however, Messrs. 1 and 2 would doubtless vote against b, while Messrs. 1 and 3 voted against a, insuring the choice of $\overline{ab}$ rather than ab. But if Messrs. 2 and 3 *traded votes*, Mr. 2 agreeing to vote for b in return for Mr. 3's agreement to vote for a, the result would be the "socially best" ab, which Messrs. 2 and 3 both prefer to ab.

Now let us retain Assumption I but drop Assumption II. As in all previous examples, let us make the realistic assumption that there are just two positions on each issue: passage and defeat of some motion. Then the force of Assumption II – as well as II* – is that there is *no tie* on any issue. Allowing ties, vote trading might again be needed to prevent anomalous results, witness:

EXAMPLE 8. Two equal-size blocs of voters, I and II, have the following preferences among four outcomes:

Bloc I	**Bloc II**
$a\bar{b}$	$\bar{a}b$
ab	ab
$\overline{ab}$	$\overline{ab}=(m_1,\ldots,m_n)$
$\bar{a}b$	$a\bar{b}$

A motion is automatically defeated unless a majority favor it, hence defeated in the event of a *tie.* With P interpreted as simple majority preference, the only collective preference among the four outcomes is that of ab to $\overline{ab}$. Indeed, ab is unanimously preferred to $\overline{ab}$. So, if any outcome should not be chosen, it is $\overline{ab}$. But $\overline{ab}$ is precisely the chosen outcome in the absence of vote trading. If, however, the two blocs traded votes, Bloc I agreeing to vote for b in return for Bloc II's agreement to vote for a, then the chosen outcome would be ab.

These examples prove, by the way, that Assumptions I and II (or II*) are essential to Consequence 1 and to the Bernholz-Oppenheimer thesis (as formulated here). In each example, there is a stable outcome other than $(m_1,\ldots,m_n)$ (which itself is dominated), contrary to Consequence 1. In each example, a majority prefer some vote trade to $(m_1,\ldots,m_n)$; and yet, contrary to the conclusion of the Bernholz-Oppenheimer thesis, Ω contains a stable outcome – indeed, P is transitive.

Demographic, Social-Psychological, and Political Factors in the Politics of Aging: A Foundation for Research in "Political Gerontology"*

NEAL E. CUTLER
University of Southern California

A prediction of the role that political activity among the aged will play in the future political system may be based upon a number of factors which are already known. This article explores three sets of factors as supportive of the argument that the American political system should expect increasing demand pressure articulated by the older segments of the population.

The first section will consider current and future demographic aspects of the older population: the size of future cohorts of older people, their structural relationship in the population to younger people, and their changing demographic composition. These factors provide insights into the magnitude of age-oriented issues in the future political system. The second section critically examines the disengagement theory of social and political participation which predicts that the processes of aging precipitate a withdrawal from such participation. The discussion will evaluate disengagement theory with data from studies of political attitudes and behavior. The third section considers the issue of whether or not political conservatism is in fact an inherent characteristic of older persons – a popular belief in American political mythology.

These sets of factors are particularly relevant for the near future of American politics. The "baby boom" of the 1940s represents a "bulge" in the flow of population groups in contemporary American society. Surviving members of this age cohort, consequently, will represent a "gerontology boom" in the first decades of the next century. The demographic, social-psychological, and political factors explored here bear upon the question of whether this large group of citizens is likely to represent, in the aggregate, a significant actor in the future of American political conflict.

Current and Projected Demographic Aspects of the Older Population

A primary factor in anticipating the future of an age-based politics is that of population numbers. One of the most important sociopolitical facts documented by the 1970 population census is the continuing increase in the size of the older population in the United States (typically defined as people aged 65 and older). Data on the age distribution of the national population are available since the year 1870, at which time the 65+ group was about 1.2 million, representing only 2.9 per cent of the population. This proportion has exhibited an uninterrupted increase, being 4.1 per cent in 1900, 6.8 per cent in 1940, and 9.9 per cent in 1970. By 1970 the national population had reached more than 203 million with an older population of 20 million. "Thus, a hundred years of variable growth produced by 1970 a total population five times as large, a middle-aged population nine times as large, and an older population an unprecedented 17 times as large."[1]

---

*The author holds a joint appointment in the Department of Political Science, and as Laboratory Chief, Social Policy Laboratory, in the University's Andrus Gerontology Center. The research project of which this paper is a part has been supported by Administration on Aging Grant 93–P–5762/9. The author wishes to extend his thanks to the following for their comments on an earlier draft: James Birren, James Davies, Frederick Eisele, Daniel Fritz, Kenneth Janda, and K. Warner Schaie, with special gratitude to Vern Bengtson for his multifaceted colleagueship in the socialization of a gerontologist.

[1] Herman B. Brotman, "The Older Population Revisited: First Results of the 1970 Census," *Facts and Figures on Older Americans*, Number 2, Administration on Aging, Washington, D.C., SRS-AOA Publication No. 182, 1971, p. 1.

A detailed analysis of the last three decennial censuses further indicates the expanding relative growth rate of the older population.[2] The percentage growth rates for the population under 65 in 1950–1960 and 1960–1970 were, respectively, 17.1 per cent and 12.5 per cent. For the population over 65 the growth rates were 34.7 per cent and 21.1 per cent. Thus, even in the context of a general decline in the growth rate for the total population, the growth rates for the 65+ group remained almost *twice as large* as the growth rates for the under-65 population.

Table 1 presents the growth in the percentage of the 65+ population group, as compared with the 18–64 and under-18 groups. In terms of absolute numbers, the increase between 1960 and 1970 in the older (65+) population was 3.5 million, from 16.5 million to 20 million. Yet this *net* increase severely understates the growth of the older population in this ten-year period. The increase in the actual number of persons who joined the ranks of the elderly is a better indicator of the "social policy" issue represented by aging, and anticipates the potential political impact of age-related issues on the political system. Between the 1960 and 1970 censuses, approximately 14.2 million persons passed their 65th birthday. When this is combined with the 16.5 million older persons previously enumerated in the 1960 census, the total number of older persons alive sometime during the 1960–1970 decade was almost 31 million. Thus the total magnitude of the social and political issues represented by the older population (e.g., the number of older persons various policies and programs deal with) is greater than that illustrated by net growth figures.

In addition, older people are living slightly longer. In 1929 a person of age 65 could expect to live, on the average, 12.2 more years, a figure which increased to 14.4 by 1959. By 1971, a person who had reached age 65 could expect to live an additional 15.1 years.

Thus, these data suggest (a) that there are more older persons in the national population, (b) that this group represents an increasing proportion of the total population, and (c) that this increasing number of individuals is spending a longer part of its life-cycle in old age. Consequently, to the degree that one considers the evolving role of age factors in politics, recognition should be given to the various changes in the age structure of the population.

When portrayed in yet a different form, these demographic data suggest that age distributions have the potential of precipitating political conflict between the work force and those population groups who are economically dependent. Demographers employ the "dependency ratio" as a summary index describing the statistical relationship between population groups.[3] Simply stated, the dependency ratio is the number of people in a "dependent" age category divided by the number of people in the "supporting" age category. In much demographic analysis, the 18–64 age group is defined as the work-force population, and those

Table 1. Basic U.S. Census Data for Three Age Groups, 1930–2050

	1930	1940	1950	1960	1970	2000	2020	2050
(Number given in thousands)								
Young (under 18)[a]	47,609	45,306	47,079	64,202	69,644	72,429	78,767	79,166
Work force (18–64)	68,533	77,344	91,953	98,560	113,502	163,159	188,374	191,633
Old (65+)	6,633	9,019	12,294	16,560	20,065	28,842	40,261	49,295
Total	122,775	131,669	151,326	179,323	203,212	264,430	307,402	320,094
(percentages)								
Young (under 18)	38.8	34.4	31.1	35.8	34.3	27.4	25.6	24.7
Work force (18–64)	55.8	58.7	60.8	55.0	55.9	61.7	61.3	59.9
Old (65+)	5.4	6.9	8.1	9.2	9.9	10.9	13.1	15.4

[a] For 1930 and 1940, the data are for the age groups 0–19, 20–64, and 65+.

Sources: (1930–1940) U.S. Bureau of the Census, *U.S. Census of Population: 1940. Characteristics of the Population,* Part I, Table 7, p. 26. (1950–1970), U.S. Bureau of the Census, *Statistical Abstract of the United States: 1972,* Table 37, p. 32. (2000) (based on Series E projections) Herman B. Brotman, "Projections of the Population to the Year 2000," Statistical Memo #25; Department of Health, Education, and Welfare, Administration on Aging, June 1973, p. 3. (2020, 2050) (based on Series E and Series W respectively) prepared by Dr. David M. Heer, Population Research Laboratory, University of Southern California, February 1974.

---

[2] *Ibid.*

[3] Henry S. Shryock and Jacob S. Siegel, *The Materials and Methods of Demography,* Rev. ed. (Washington, D.C.: U.S. Government Printing Office, 1975), p. 235.

under 18 and over 65 are considered to be the dependent population. Of course, everyone in the 18–64 group is not actually working, just as everyone in the 65+ group is not in fact dependent. Nonetheless, for the purposes of estimating those future conditions for which a dependency ratio is useful, the index can be computed directly from the published census age distributions. The logic of the dependency ratio can be used to compute alternative index measures, depending on whether the numerator contains just the young, just the old, or both groups. In this discussion, the old-age dependency ratio (i.e., 65+/18–64) is used.

Table 2 presents the trend in the dependency ratio for the years 1930 through 1970, and includes the estimated dependency ratios through the year 2050. These data indicate substantial increases in the proportion of the older population as compared to the supportive working population. The dynamics of the dependency ratio depend, of course, on the relative sizes of population cohorts born at different times. The baby boom of the 1940s becomes the gerontology boom of the period 2000–2010. During the time that the proportionally large baby boom cohort is in its productive working years, especially when older people during these years constitute a relatively smaller cohort, the relationship between supports and demands may not produce conflict. But when the social and political systems encounter the gerontology boom at a time when the working population has relatively decreased, age-based allocation claims may easily become the focal point for substantial political conflict.

The dependency ratios based on the projected population estimates emphasize the potential salience of issues of social and financial allocation likely to arise in the future. The decreases in the birth rate that have occurred recently will mean that a continually diminishing work force will be responsible for supporting a continually increasing older population. As the data in Table 1 and Table 2 indicate, the dramatic change comes not in the period ending in the year 2000, but in 2020 and 2050. The population estimates for these latter years are based on zero population growth birthrates, or the "replacement" fertility rate of 2.1 babies per woman. Given these population projections for the years 2020 and 2050, the dependency ratios based upon the projections are .213 and .257, as compared to the ratio of .177 for 1970.[4]

Dependency ratios such as these become increasingly important as one considers the demands which the older population is likely to press upon the rest of society. Medical care, transportation, housing, and basic economic well-being all represent needs which the supportive sectors of society will be called upon to finance. The example of the Social Security system serves to further illustrate the significance of these dependency ratios. As the system moves more completely from a pay-as-you-go deferred-wages program to a revolving-fund, retirement-income provision program financed out of the general revenue funds, the burden upon the active labor pool of supporting the expanding retired population will increase.[5]

The magnitude of the potential conflict

Table 2. Old Age Dependency Ratios,[a] 1930–2050

1930	1940	1950	1960	1970	2000	2020	2050
.097	.118	.133	.167	.177	.177	.213	.257

[a] Dependency Ratio = 65+/18–64; based on demographic data in Table 1.

---

[4] Although all population projections are estimates subject to error, certain elements of these projections suggest their likely stability. For example, all people who will be in the 65+ age group in 2000 and 2020 are already born. Estimates of future births may be made either on the assumption of high fertility or low fertility. The United States is, and recently has been, witnessing decreased fertility, a trend that is expected to be maintained for the next ten to twenty years. Consequently, the estimates used here are based upon the low fertility assumption of 2.10 births per woman – also known as replacement rate – i.e., the assumption on which the U.S. Census Bureau's Series E projections are based. See: Neal E. Cutler and Robert A. Harootyan, "Demography of the Aged," in *Aging: Scientific Perspectives and Social Issues,* ed. Diana S. Woodruff and James E. Birren (New York: Van Nostrand Company, 1975), pp. 31–69. A nationally distributed news story reported that a recent Census Bureau study found the fertility of American women in 1976 at a historical low. "The fertility rate now stands at 1.8 children per woman – exactly half the 1957 level." The report also found that there are now "7.6 million fewer American children aged 13 and under than there were in 1970, even though there are more women of childbearing age." United Press International, "U.S. Fertility Rate Drops to Record Low," *Los Angeles Times,* February 11, 1977, p. 23. The continuation of this pattern would mean that Tables 1 and 2 underestimate the proportion of older persons in the population.

[5] For discussion of the "economics of the *intergenerational relationship,*" see James N. Morgan,

suggested by the dependency ratios, however, is increased if changes evolve in the age definition of the dependent population. All of the population estimates on which the dependency ratios in Table 2 have been computed are based on a retirement age of 65. Yet there is evidence[6] that the average retirement age of the population is declining and will become even lower as regulations are changed by the Social Security system and other pension systems, and by the personal selection of early retirement options by increasing numbers of workers.

The potential impact of changes in retirement age is indicated by a re-examination of the 1970 census data using age 60+ as defining "old" rather than age 65+. The 1970 dependency ratio would increase from .177 to .273, the latter ratio being more extreme than the ratio indicated in Table 2 for the year 2050 using the 65+ population. Consequently, the social, economic, and political consequences become greater as one considers (a) the increase in the numbers and proportions of older people, (b) their increasing longevity, and (c) a lowering of the age of retirement or dependency.

To this point we have considered the possible political impact of the changing age structure of the population only in terms of absolute numbers and relative proportions. A significant aspect of demographic analysis which affects political considerations, however, is that of the *composition* of various population groups. Of particular importance to the anticipation of political activity among the aged is the trend in educational level which characterizes successive age cohorts in the United States, since education has been seen to be a major predictor of political activity.[7]

It is well known that the average educational level of the American public has been steadily rising over the past fifty years. This general rise is "caused" by the addition to the population of new cohorts of people who are more highly educated than the cohorts which preceded them, and the dying off of older cohorts who were, as a group, less educated than the cohorts now entering the population system. The implications of these cohort changes for the politics of the aged may be drawn from the age distribution of education presented in Table 3.

In this table each of six national presidential election samples has been divided into high and low education groups, the definition of "high education" being the completion of high school. The data for the 65+ group reveal substantial changes. The older group in 1952 was educated at the beginning of the century when mass public education was not yet widely available. As more educated cohorts enter the system and mature, the average educational level of older persons continuously increases, as is seen by contrasting the 65+ groups in 1952 and 1972.

Finally, we can estimate what the educa-

Table 3. Educational Composition of Selected Age Groups

	1952		1956		1960		1964		1968		1972	
Age Group	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
21–24	45	55	33	67	39	61	30	70	19	81	14	86
25–34	47	53	34	66	36	64	29	71	21	79	22	78
35–44	58	42	48	52	36	64	34	66	34	66	26	74
45–54	67	33	59	42	47	53	50	50	39	61	40	60
55–64	76	24	65	35	68	32	62	38	55	45	55	45
65+	81	19	78	22	71	29	67	33	71	29	70	30
Total	62	38	51	49	48	52	45	55	41	59	38	62

"Low" education is defined as no education through incomplete high school; "High" education is complete high school (or equivalence) and higher.

Source: Each year was taken from the University of Michigan Center for Political Studies presidential election national survey. The data were provided by the Inter-University Consortium for Political and Social Research through the USC Political and Social Data Laboratory.

"Economic Problems of the Aging and Their Policy Implications," paper prepared for the Gerontological Society Conference on Public Policy Assessment of the Conditions and Status of the Elderly (Santa Barbara: February, 1975); Juanita Kreps, "The Economics of Intergenerational Relationships," in *Social Structure and the Family: Generational Relations*, ed. Ethel Shanas and Gordon Streib (Englewood Cliffs: Prentice-Hall, 1965), pp. 267–289.

[6] Robin Jane Walther, "Economics and the Older Population," in Woodruff and Birren, pp. 336–351.

[7] For example, see Seymour Martin Lipset, *Political Man: The Social Bases of Politics* (New York: Doubleday & Company, 1960), Chapter 6; Lester W.Milbrath and M. L. Goel, *Political Participation*, 2nd ed. (Chicago: Rand McNally, 1977), Chapter 4; Sidney Verba and Norman H. Nie, *Participation in America* (New York: Harper and Row, 1972), Chapter 6.

tional level of old people will be at the beginning of the next century. The youngest age group in the 1972 sample will all be at least 65 years old by the year 2016, so that by the year 2020 the older population will be a much better educated group than older people of yesterday and today. Whereas only 19 per cent of the 65+ group in 1952 had a "high" level of educational attainment, by 2020 86 per cent of the 65+ group will have a high education – a rather substantial reversal. While not all educated people will participate in politics, the potential for involvement and activity will certainly be greater for tomorrow's older people than it has been in the past.[8]

Participation vs. Disengagement and the Future Politics of Aging

Despite the evidence of substantial growth in the older population, the question remains whether this large and increasingly well-educated population group will in fact participate in politics. One of the widely considered theories in social gerontology argues that as individuals grow old they gradually disengage from active social participation. If in fact older people disengage from, among other things, political activity, then the demographic factors discussed above are not as predictive of the political involvement of old people as might otherwise be expected.

Formulated in the early 1960s, disengagement theory was based upon the observation of a gradual behavioral and psychological withdrawal by older individuals from the various activities which they had pursued in the younger and middle years. As proposed by Cumming and Henry,[9] the theory argues that the older person naturally or inevitably withdraws his or her emotional and actual involvement from the neighborhood, from the family, and from the world of politics. The disengagement process was considered to be natural or inherent because of the inevitability of death and the onset of physiological inabilities. As the individual experienced, or perceived himself to experience, a decline in energy and ability, the withdrawal of emotional involvement in the external world became a means of increasing psychological well-being or morale.[10] Furthermore, since the gradual processes of disengagement affect the individual's peripheral involvements first, and then his more personal and central involvements, disengagement, for most people, would affect politics early in the process.

The main criticism of the theory centers on the inevitability of disengagement. Do *all* individuals in fact wish to disengage from social and political activity? Is such disengagement really a reflection of internal processes, or do social, environmental factors which are not a part of the individual's own aging play a part? The disengagement hypothesis has in fact been severely questioned on both empirical and theoretical grounds. There is evidence, for example, that for some individuals high morale is associated with activity rather than being correlated with withdrawal as was predicted by disengagement theory. Theoretically, several gerontologists have noted that the disengagement hypothesis does not sufficiently consider the external social environment in which the aging individual finds himself – a young-oriented environment that is often hostile to old people and to the symbols of aging.[11]

Consequently, new theories of aging are being proposed. Atchley,[12] for example, proposes "continuity theory" as a more general approach to the understanding of the many ways in which people develop in their older years. Continuity theory argues simply that there is a continuity of personality and social orientations between the middle years and old age. Individuals who were highly engaged in society may continue to be highly engaged; those who have had less active patterns of social activity in their earlier life would be expected to evidence less activity in old age.

Recent psychological research in human development has also cast doubt on the scientific validity of any "natural" psychosocial changes associated with the processes of aging. The documentation for many such age-related cognitive and personality gradients is found in cross-sectional studies. In his "General Developmental Model," however, Schaie demonstrates that cross-sectional studies of personality do not accurately describe ontogenetic processes.[13] Indeed, the application of his *cross-sequential* research designs to a range of psychological variables has revealed the existence and explanatory importance of the generational cohort dimension of personality patterns within the population.[14] Thus we may conclude that the apparent age-related changes described for various social and psychological variables are in some cases the artifactual consequence of the inappropriate use of cross-sectional research designs.

---

[8] Bernice L. Neugarten, "Age Groups in American Society and the Rise of the Young-Old," *Annals of the American Academy of Political and Social Science,* 415 (September, 1974), 187–198; Bernice L. Neugarten, "The Future and the Young-Old," *The Gerontologist,* 15 (February, 1975), Part II, 4–9.

[9] Elaine Cumming and William Henry, *Growing Old* (New York: Basic Books, 1961).

[10] Vern L. Bengtson, *The Social Psychology of Aging* (New York: Bobbs-Merrill, 1973), p. 44.

[11] George Maddox, "Activity and Morale: A Longitudinal Study of Selected Elderly Subjects," *Social Forces,* 42 (December, 1963), 195–204. A theoretical rebuttal to the inevitability of disengagement is presented in: Arnold M. Rose, "A Current Theoretical Issue in Gerontology," *The Gerontologist,* 4 (March, 1964), 46–50.

[12] Robert C. Atchley, *The Social Forces in Later Life* (Belmont, Calif.: Wadsworth Publishing Company, 1972), p. 35.

As disengagement theory and its presumed behavioral consequences have been studied, discussed, and criticized, an important area of inquiry has concerned the distinction between "personal disengagement" and "societal disengagement." The word *disengagement* implies that a relationship between two actors is being dissolved. Typically, however, disengagement research has examined only the individual, and not the society from which he is ostensibly disengaging. Yet investigation of society's role in this process has revealed that in fact it is at least as true that society is withdrawing from the older person.

Indeed, Kuypers and Bengtson argue that it is precisely the "social labeling" of symbols of aging by society which accounts for the negative image which many old people have of themselves, and the subsequent withdrawal of old people from social activity. They note that in the course of normal aging, the older person experiences various events which withdraw the usual sources of feedback supportive of an individual's self-image – the loss of role, the loss of spouse, the ambiguity of social information about aging, and the weakening of reference group support. "This feedback vacuum creates a *vulnerability to,* and *dependence on,* external sources of self labeling, many of which communicate a stereotyped negative message of the elderly as useless and obsolete."[15]

Thus, the individual's apparent withdrawal from social participation may reflect not so much inevitable psychodynamic processes, but more simply a realistic response to society's constriction of the "opportunity structure" for participation. Some studies indicate that when the opportunity structure is opened to participation, older persons do not necessarily disengage. Carp, for example, studied a group of older persons as they moved from one residential facility to another.[16] The respondents' desire for engagement in three kinds of activity was measured: paid work, volunteer work, and leisure activity. The opportunities for each of these was limited in the first residential environment, but were expanded in the second residence. The results indicated that in the second environment the older respondents did engage in those activities which fit with their predispositions and expressed a high degree of satisfaction and happiness in the situation of increased engagement.

A second example focused on engagement-disengagement patterns among retired professors. Academic life is one of the few areas of livelihood which has an institutionalized post-retirement role – that of the emeritus professor – thus providing an opportunity structure for continued activity and engagement beyond the official retirement age. In their study of retired professors, Roman and Taietz found the following: 41 per cent were engaged in work at the same university; 13 per cent were professionally engaged elsewhere; 24 per cent were in ill health; and 22 per cent had disengaged from both university and profession.[17] When the percentages are recomputed deleting those in ill health who could not make a decision about engagement, the results of the study show that 71 per cent of the professors were engaged in their profession and only 29 per cent were disengaged. As Atchley comments on the study: "These data suggest that the frequency of disengagement is very much a product of the opportunity for continued engagement.... This finding is all the more revealing since almost half of Roman and Taietz's sample was over seventy-five years old."[18]

---

[13] K. Warner Schaie, "A General Model for the Study of Developmental Problems," *Psychological Bulletin,* 64 (August, 1965), 92–107.

[14] Paul B. Baltes, "Longitudinal and Cross-Sectional Sequences in the Study of Age and Generation Effects," *Human Development,* 11 (No. 3, 1968), 145–171; K. Warner Schaie and Gisela V. Labouvie-Vief, "Generational vs. Ontogenetic Components of Change in Adult Cognitive Behavior: A Fourteen-Year Cross-Sequential Study," *Developmental Psychology,* 10 (May, 1974), 305–320; K. Warner Schaie and Iris A. Parham, "Stability of Adult Personality Traits: Fact or Fable?", *Journal of Personality and Social Psychology,* 34 (1976), 146–158; K. Warner Schaie and Charles R. Strother, "A Cross-Sequential Study of Age Changes in Cognitive Behavior," *Psychological Bulletin,* 70 (December, 1968), 671–680; Diana S. Woodruff and James E. Birren, "Age Changes and Cohort Differences in Personality," *Developmental Psychology,* 6 (March, 1972), 252–259.

[15] Joseph A. Kuypers and Vern L. Bengtson, "Social Breakdown and Competence: A Model of Normal Aging," *Human Development,* 16 (No. 3, 1973), 182.

[16] Frances M. Carp, "Person-Situation Congruence in Engagement," *The Gerontologist,* 8 (Autumn, 1968), 184–188.

[17] Paul Roman and Philip Taietz, "Organizational Structure and Disengagement: The Emeritus Professor," *The Gerontologist,* 7 (September, 1967), 147–152.

Research on political behavior is especially useful in evaluating any connection between aging and disengagement. Unlike other areas of social activity, no formal or legal rules require people to withdraw from political activity at a given age. In fact, legislatures and courts are often cited as prime examples of institutions in which age offers no functional barrier.[19] Data on political orientations and behavior also suggest that disengagement is not an inevitable consequence of the aging process.

Table 4 presents an age distribution of voter participation by sex and education. The patterns disclose a general *increase* in participation associated with age, with little evidence of disengagement from this form of political activity among the males. Whether the decline in voting by females is due to "aging" per se, to widowhood, or to patterns of generational difference in female political activity is not ascertainable from these data. Nonetheless, the old-age decrements in voter participation are not as significant as are the differences associated with sex and education. Indeed, Verba and Nie's study of political participation in the United States demonstrates that when statistically precise controls for education and income are employed, there is no noticeable decrease in age patterns of voting or general participation.[20]

A second example concerns age patterns in self-reporting of political interest. Table 5, which presents data describing those who indicated a "great deal" of interest in politics, reveals quite the opposite of disengagement from politics among older respondents. As the authors of the study conclude, "the highest reported interest is consistently at age 60 and higher, and the difference between the middle-aged and the elderly is pronounced at most educational levels for both sexes."[21]

As a final example, Table 6 presents an age distribution of self-reports of political awareness, aggregated across ten national Gallup surveys in the 1950s and 1960s.[22] As in the previous examples, these data yield no evidence of substantial disengagement associated with age. Sex and educational differences are greater than age differences, and although some reduction in awareness is reported for the total samples in the 60+ group, these decrements are not substantial.

Table 4. Voting in the 1964 Presidential Election, by Age and Education (Percentage voting)

	Years of Education						
	Elementary		High School		College		
Age	0–7 Years	8 Years	1–3 Years	4 Years	1–3 Years	4 Years or More	Total
Males							
21–24	14	30	34	56	70	80	53
25–44	43	58	65	76	82	87	71
45–64	63	79	81	88	88	93	79
65+	66	79	80	88	91	88	74
Total 21+	58	73	69	80	82	88	73
Females							
21–24	22	21	33	55	69	78	52
25–44	34	52	58	76	85	87	68
45–64	52	69	75	84	87	92	74
65+	46	63	72	76	82	93	61
Total 21+	45	63	63	76	83	89	68

Source: Anne Foner, "The Polity," in *Aging and Society, Volume III: A Sociology of Age Stratification,* ed. Matilda White Riley, Marilyn Johnson, and Anne Foner (New York: Russell Sage, 1972), pp. 115–159, Table 4.1, as adapted from Current Population Reports, 1965, P–20, No. 143, pp. 16–19.

---

[18] Atchley, *The Social Forces in Later Life,* p. 223.

[19] For example, see the discussion in Neal E. Cutler and John R. Schmidhauser, "Age and Political Behavior," in Woodruff and Birren, pp. 374–406.

[20] Verba and Nie, *Participation in America,* Chapter 9.

[21] Norval D. Glenn and Michael Grimes, "Aging, Voting, and Political Interest," *American Sociological Review,* 33 (August, 1968), p. 570.

[22] Norval D. Glenn, "Aging, Disengagement, and Opinionation," *Public Opinion Quarterly,* 33 (Spring, 1969), 17–33.

Table 5. Self-Reported Interest in Politics
(Percentage reporting "great deal" of interest)

Years of School Completed	Ages 21–39	 40–59	 60 and Up
Males			
8	18.2 (33)	15.2 (112)	26.8 (97)
9–11	20.8 (72)	21.2 (99)	31.1 (45)
12	23.1 (173)	26.6 (139)	28.6 (42)
All educational levels	27.4 (456)	24.9 (502)	29.5 (298)
Females			
8	5.3 (38)	14.4 (111)	21.2 (104)
9–11	18.5 (130)	21.1 (90)	26.2 (42)
12	16.1 (192)	25.5 (165)	48.2 (56)
All educational levels	19.0 (496)	22.7 (520)	34.5 (330)

Glenn and Grimes report that they inflated the sample by about 100 per cent through a weighting procedure used to increase representativeness. Therefore, the reported N's (in parentheses) are about twice the number of respondents represented.

The data are from Gallup Survey 637 (1960); data are for white respondents.

Source: Norval D. Glenn and Michael Grimes, "Aging, Voting, and Political Interest," *American Sociological Review*, 33 (August, 1968), 570, Table 3.

In sum, the examples drawn from the realm of political behavior do not support the disengagement theorists. Clearly, it should not be expected that all individuals inevitably withdraw their emotional and behavioral support of social activity as a function of age. Recent research demonstrates that the opportunity structure of a particular domain of activity is a factor which substantially influences whether or not older persons who are predisposed to continued activity will in fact manifest sustained activity. Since there are no formal or legal age barriers to political activity, engagement or disengagement becomes a matter of individual choice. These data suggest that political activity appears to *increase* rather than decrease with age, and that this general pattern continues at least into the sixth decade of life.

Age, Conservatism, and Political Demands

When we add the open opportunity structure provided by politics to the increasing numbers and changing educational levels of successive birth cohorts there is reason to expect substantial political activity from the older groups within the population. Yet the question arises whether or not older people are likely to make demands upon the government for the allocation of resources. One of the most

Table 6. Average Percentage of "Yes" Responses to Eleven "Have-You-Heard-or-Read" Questions, by Age, Sex, and Education

Years of School Completed	Age 21–39	 40–59	 60 and Up
Males			
7–8	56.2	57.8	61.4
1–3 of high school	61.5	70.2	70.8
4 of high school	70.5	72.8	77.3
All educational levels	69.0	68.3	65.6
Females			
7–8	41.8	45.7	51.1
1–3 of high school	52.5	54.6	55.6
4 of high school	59.1	65.2	62.6
All educational levels	57.9	57.8	53.0

Examples of the questions are: "Have you heard or read anything about the trouble in the Formosa area?" (1955) and "Have you heard or read about the Black Muslims?" (1963). The ten surveys from which the questions come are Gallup Surveys 521, 524, 532, 543, 582, 588, 649, 673, 675, and 701; data represent white respondents only.

Source: Norval D. Glenn, "Aging, Disengagement, and Opinionation," *Public Opinion Quarterly*, 33 (Spring, 1969), 27, Table 5.

popular myths in American political folklore is that aging brings about a conversion to conservatism.[23] In terms of the dominant issues of domestic American politics in the past forty years, the conservative-liberal dimension has generally been linked to the question of federal vs. local responsibility in political matters, or governmental vs. private responsibility for the welfare of the individual. If in fact aging produces political conservatism, then perhaps there is no need to be concerned with the impact of older people on politics, since the conservative may not press demands upon the national political system for increased dollars and services.

There is, of course, some basis in social psychological studies to argue that the processes of maturation yield a less open view of the world, that psychological rigidity and conservatism in personal values are characteristic of the orientations of the aged. Experiments have demonstrated that older persons may be less open to attitude change, and more set in their ways of problem solving.[24] Interestingly, studies of political attitudes and values are often cited by psychologists as additional evidence of the general age-related conservatism.

At least three factors associated with the onset of old age can provide the frustrations and threats to the individual which may be causes of rigid and conservative behavior patterns.[25] First, age brings significant losses in personal status; while new statuses may be obtained, they cannot really replace those which had been the basis of a lifetime of social gratification. Second, although biological losses do occur with age, the actual biological losses are not as important as are the individual's *perception* of such losses. Third, with old age comes stability of life style, occupation, career, residential location, and family choices. The latitude for new areas of choice may quite realistically be limited, and the older person may thus feel a loss of personal freedom.

These social and psychological manifestations of old age create a situation in which the aging individual is quite susceptible to real and imagined threats.[26] Since the individual is facing losses in status, in perceived biological performance, and in personal freedom, new situations may be seen as threatening. Consequently, older persons, like persons of all ages, may respond to threatening situations through the application of their own personally-validated stereotypes.[27]

Thus, there is evidence of a tendency toward conservatism which may come with advanced age, and there is at least some social psychological theory on which to base a general claim for such conservatism as a response to changing and threatening situations. Yet even the gerontological evidence is not perfectly clear; studies typically use different conceptual and operational measures of conservatism, and the age differences are not sharp: ". . . seldom is the trend strong enough to put the majority of the young on one side of an issue and the majority of the old on the other side."[28]

An examination of three areas of attitude data pertaining to the conservatism issue in American domestic politics yields little support for the idea that older people are, or become, substantially more conservative than the rest of the population. As a first example, Table 7 presents some of the earliest public opinion data available on this topic – Hinshaw's presentation of Gallup poll data from the late 1930s.[29] A number of issues germane to the liberal-conservative dimension as defined in terms of the scope of federal activity are included here. In some cases, for example on the matters of federal ownership of railroads, electric companies, and radio stations, there are no patterns of age differentiation. Where there is some evidence of a more conservative position taken by the older group these differences are rather small, as in the questions pertaining to federal regulation of business, and they certainly do not put the old on the conservative side of the issue and the young on the liberal side. In fact, on the issue concerning the limitation of private fortunes, the older respondents took a *more* liberal position than the younger respondents.

---

[23] A recent formulation of this view is included in: Robert R. Hudson and Robert H. Binstock, "Political Systems and Aging," in *Handbook of Aging and The Social Sciences,* ed. Robert H. Binstock and Ethel Shanas (New York: Van Nostrand, 1976), pp. 369–400.

[24] Sidney L. Pressey and Raymond G. Kuhlen, *Psychological Development Through the Life Span* (New York: Harper & Row, 1957), pp. 436–505; Leonard Z. Breen, "The Aging Individual," in *Handbook of Social Gerontology,* ed. Clark Tibbitts (Chicago: University of Chicago Press, 1960), pp. 145–162; H. J. Heglin, "Problem Solving Set in Different Age Groups," *Journal of Gerontology,* 11 (July, 1956), 310–316.

[25] Raymond G. Kuhlen, "Changing Personal Adjustment During Adult Years," in *Psychological Aspects of Aging,* ed. John Anderson (New York: American Psychological Association, 1956), pp. 21–35.

[26] Kuypers and Bengtson, "Social Breakdown and Competence."

[27] Breen, "The Aging Individual," p. 154.

[28] Raymond G. Kuhlen, "Aging and Life Adjustment," in *Handbook of Aging and the Individual,* ed. James E. Birren (Chicago: University of Chicago Press, 1959), p. 882.

[29] Robert Hinshaw, *The Relationship of Attitudes and Opinions to Age,* Ph.D. dissertation, Department of Psychology, Princeton University, 1944.

A second example concerns the relationship between aging and identification with the Republican party in the United States, since Republican identification has been used as one measure of political conservatism. At least in terms of general public perceptions there is an association between old people, conservatism, and the Republican party: "The fact that the *older generation* of the electorate is most heavily identified with the Republican Party and most unswerving in its devotion to it conforms to the general perception of older people as the most conservative element of the body politic."[30]

Use of the phrase *older generation* in referring to older people in the contemporary electorate points to what has been an interesting controversy in recent political research. It has been observed that in the 1950s and 1960s, in any given election or any given year most older persons were Republicans while most younger people were Democrats. Do we conclude from such data that the aging process has

Table 7. Early Gallup Data on the Scope of Federal Power

1. Do you think the Federal Government should buy, own and operate the railroads? 7–2–38

	Yes	No	No Opinion	N
Young[a]	27%	59%	14%	925
Medium	28	58	14	1160
Old	26	59	15	970

2. Do you think the Government should own the electric companies? 7–31 –40

	Yes	No	No Opinion	N
Young	30%	47%	23%	450
Medium	32	45	23	541
Old	28	48	24	609

3. Would you favor Federal Government ownership and control of all radio broadcasting stations? 5–20–38

	Yes	No	No Opinion	N
Young	15%	70%	15%	990
Medium	17	64	19	1202
Old	16	64	20	960

4. During recent years the trend has been to give the Federal Government greater power to regulate business, industry, and agriculture. Are you in favor of this trend towards centralizing the power in Washington? 8–9–37

	Yes	No	No Opinion	N
Young	45%	39%	16%	992
Medium	40	44	16	1035
Old	36	48	16	915

5. During the next four years, do you think there should be more regulation or less regulation of business by the Federal Government than at present? 10–24–40

	More	Same	Less	No Opinion	N
Young	25%	21%	33%	21%	781
Medium	22	18	42	17	1136
Old	20	19	42	19	1235

6. Do you think the size of private fortunes should be limited? 6–7–37

	Yes	No	No Opinion	N
Young	40%	51%	9%	1021
Medium	43	50	7	988
Old	48	42	9	812

[a] Young = 18–30; medium = 31–47; old = 48+.

Source: Robert Hinshaw, *The Relationship of Attitudes and Opinions to Age* (Ph.D. dissertation, Department of Psychology, Princeton University, 1944), pp. 21–22, Table III.

---

[30] Angus Campbell, "Politics Through the Life Cycle," *The Gerontologist,* 11 (Summer, 1971), 114, emphasis added.

brought about a change in the conservative direction, or that the conservatism or Republicanism of contemporary older voters is simply a generational cohort phenomenon?

A number of studies have investigated these alternatives. The authors of *The American Voter* suggested on the basis of the accumulated cross-sectional evidence of the 1952 through 1958 election surveys that the New Deal period had created a generational hold on young people who were voting for the first time during that period.[31] One study which argued that aging produces a *conversion* to political conservatism focused on an analysis of the relationship between age and Republican party identification, analyzed in a sequence of Gallup polls taken at four-year intervals from 1946 through 1958. While it was argued that the data demonstrated a linear change to the conservative alternative, a re-analysis of some of the data argued that the conclusion connecting increasing Republicanism to aging was not supportable when the data were analyzed using the technique of cohort analysis.[32]

In a follow-up study Glenn and Hefner employed a data base including surveys taken every four years between 1945 and 1959 in which they employed formal cohort analysis procedures, included controls for sex and education, and statistically corrected the data for the underrepresentation of lower-class respondents in the earlier Gallup surveys.[33] Thus, the study brought to the analysis of the relationship between aging and conservatism an expanded data base and much methodological experience and sophistication. The authors concluded that "this study should rather conclusively lay to rest the once prevalent belief that the aging process has been an important influence for Republicanism in the United States."[34]

As a final example of the lack of any inherent social policy conservatism on the part of older people, we may look to analyses of the attitude measures of federal involvement in society included in the election studies conducted by the University of Michigan Center for Political Studies. As reported by Campbell, an acceptable Guttman scale was formed using items on federal involvement in employment guarantees, school construction, the delivery of medical services, fair treatment of blacks in jobs and housing, and the question of whether electricity and housing should be left totally to private enterprise.[35] The scale behaved as expected, with Republicans exhibiting more conservative orientations than Democrats, high income people being more conservative than middle- or low-income people, and the highly educated being more conservative than the less educated. Campbell reports the surprise of the analysts, however, when it turned out that the young were more conservative than the old.

Decomposition of the scale into its component attitudes quickly indicated the source of this reversal. While for most people in the electorate the five issues were part of an overall attitudinal orientation, the older people were more likely to endorse certain items but not others. Specifically, on questions concerning governmental support of medical programs and guarantees of full employment – issues of particular relevance to the welfare of the aged – the older respondents were substantially *less conservative* than the younger members of the electorate, a conclusion supported also by more recent analyses of the Michigan data.[36]

That older members of the electorate are not conservative on certain issues and are likely to make demands upon government for the expansion of programs aimed at their own well-being is predictable on the basis of previous analyses of American political orientations. In *The American Voter* the Michigan analysts classified all voters as to the level of issue content expressed in their attitudes. The four general categories employed were: ideology or near-ideology, group benefits, nature of the times, and no issue content at all. The modal category for the electorate was the "group benefits" orientation, a category into which 45 per cent of the electorate was classified.[37] While the analysis did not present any information on age patterns in these classifications, there is no reason to believe that older voters are different in this regard from the rest of the electorate. On the contrary, as older persons increasingly recognize the particular needs which accompany aging, especially in the context of low levels of retirement and pension income, they may be more likely to take a group-benefits orientation toward political issues.[38]

---

[31] Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York: Wiley, 1960), pp. 153–156.

[32] John A. Crittenden, "Aging and Party Affiliation," *Public Opinion Quarterly*, 26 (Winter, 1962), 648–657; Neal E. Cutler, "Generation, Maturation, and Party Affiliation: A Cohort Analysis," *Public Opinion Quarterly*, 33 (Winter, 1970), 583–588. This exchange is reprinted in *Quantitative Analysis of Political Data*, ed. Samuel A. Kirkpatrick (Columbus, Ohio: Charles E. Merrill, 1974), Chapter 6, pp. 465–482.

[33] Norval D. Glenn and Ted Hefner, "Further Evidence on Aging and Party Identification," *Public Opinion Quarterly*, 36 (Spring, 1972), 31–47.

[34] *Ibid.*, p. 47. Despite the "conclusiveness" of this statement, the issue is alive and well in political science. A sharp exchange can be followed in: Paul R. Abramson, *Generational Change in American Politics* (Lexington, Mass.: D.C. Heath, 1975); Philip E. Converse, *The Dynamics of Party Support: Cohort Analyzing Party Identification* (Beverly Hills: Sage Publications, 1976); Paul R. Abramson, "Developing Party Identification: A Further Examination of Life-Cycle, Generational, and Period Effects," paper prepared for presentation at the Annual Meeting of the American Political Science Association, Washington, D.C., September, 1977.

[35] Angus Campbell, "Social and Psychological Determinants of Voting Behavior," in *Politics of Age*, ed. Wilma Donahue and Clark Tibbitts (Ann Arbor: University of Michigan, 1962), pp. 87–100.

[36] Campbell, "Politics Through the Life Cycle"; E. M. Schreiber and Lorna R. Marsden, "Age and Opinions on a Government Program of Medical Aid," *Journal of Gerontology*, 27 (January, 1972), 95–101; Jerry L. Weaver, "The Elderly as a Political Community: The Case of National Health Policy," *Western Political Quarterly*, 29 (December, 1976), pp. 610–649.

In summary, then, this discussion demonstrates that despite the popular image of old people as being the most conservative age group politically, there is little evidence of an age-related political conservatism on the basic domestic political issue concerning the role of the federal government in the areas of economics and social welfare. As a result of such factors as the group-benefits orientation and differences between generational cohorts, we should not uncritically expect older members of the electorate in the future to be conservative on the issue of making increasing demands upon the federal government for the allocation of scarce resources.

Summary and Conclusion: Research Questions for "Political Gerontology"

The focus of this analysis has been to emphasize the basic factors relevant to the future role of age and the aged in American politics, factors which provide a foundation for more specific research in "political gerontology." First, population trends are producing larger numbers and proportions of older persons. But more directly germane to political conflicts, dependency ratios demonstrate that the baby boom cohorts of the 1940s will become a "gerontology boom" at a time when the lowered birth rates of the 1970s will have produced a relatively smaller number of income-producing workers. At the same time, tomorrow's older population will reflect the education, political experience, and activist life-styles which characterize the younger adults of the 'sixties and 'seventies.[39]

Second, recent gerontological evidence indicates that older people do not necessarily "disengage" from social activity. Indeed, the greater the opportunity for participation, such as that provided by the political arena, the more likely that continuity will characterize the behavior patterns of older persons.

The third factor focuses on a basic myth of contemporary political folklore: that people *become* politically conservative as an inevitable consequence of the processes of aging. At least two factors strongly suggest that future groups of older persons will not necessarily be basically conservative. First, several analyses conclude that the conservatism observed in recent years among older persons is generational in nature, and not related to the aging process. Second, a group-benefits orientation in American politics typically motivates individuals to support programs which will yield benefits for their group. Given the increased needs on the part of the aged for medical care, the rising costs of medical care, and widespread poverty among the aged, it is no surprise that older people have been found to be even more favorable toward federal medical care programs than are the traditionally less conservative younger voters.

The thrust of these three factors – demographic, social-psychological, and political – is that the political system will be increasingly affected by the politics of age in future years, and substantially so by the beginning of the next century. Of course, we do not mean to suggest that these three factors by themselves will determine the future influence of older people upon American politics. Indeed, some might argue that the politics of the year 2000 cannot be foreseen, and that events of the next twenty-five years may completely reorient American politics. In the absence of such a reorientation, however, the factors discussed here do predict the increasing salience of the age factor in politics. To the degree that this prediction is considered plausible, then a number of additional questions for political researchers requires investigation. We shall conclude this overview by suggesting three broad areas of research in "political gerontology."

---

[37] Campbell et al., *The American Voter*, p. 249.

[38] A generational cohort analysis of attitudes toward federal responsibility in the provision of medical care, based upon the Michigan presidential election surveys, quite clearly revealed that cohorts observed aging from 60–64 to 65–68 years of age were substantially more supportive of the liberal (federal) position than were either the youngest cohorts in the electorate or the electorate as a whole. Furthermore, this pattern was found for each pair of successive elections over the 1952–1972 period. See: Vern L. Bengtson and Neal E. Cutler, "Generations and Intergenerational Relations: Perspectives on Age Groups and Social Change," in Binstock and Shanas, pp. 130–159.

[39] In a recent paper Ragan suggests that the beginnings of a "backlash" against the increasing benefits now being allocated for the elderly can already be detected in both the popular and the academic press. While most of the reviewed articles and statements did not "view with alarm" the current state of affairs, many comments pointed to the conflict-prone situation summarized by the dependency ratios. Pauline K. Ragan, "Another Look at the Politicizing of Old Age: Can We Expect a Backlash Effect?", paper prepared for presentation at the Annual Meeting of Society for the Study of Social Problems, New York, August, 1976.

The first is the issue of *generational differences in political orientations.* Much of our knowledge about the political behavior of the mass public is based on cross-sectional inquiries. Yet as our analysis of the issue of conservatism has illustrated, generalizations which may be descriptively valid for the polity at midcentury are not necessarily valid for the year 2000. The concept of "generations" has been part of social analysis for dozens of years,[40] and has recently become popular in the study of youth and political socialization.[41]

Although the concept of generations has been useful in the analysis of successive cohorts of adolescents, a substantial and growing literature in the area of developmental psychology suggests that the generational concept is appropriate for the study of adults as well. Psychologists, perhaps even more so than political scientists, have traditionally been willing to accept cross-sectional age-correlations as evidence of "inherent" maturational or ontogenetic human processes. Yet, as described in the second section of this article, an increasing number of psychological studies demonstrates that a variety of "basic" personality attributes are distributed within the population as a function of generational differences. And since the chronological age of a respondent in an interview study or subject in an experiment is an index of both developmental age and generational location, the generational dynamic may rival the traditional developmental explanation for a range of human attributes.

Political scientists and sociologists have also begun to consider the generational explanation of age-related phenomena. Indeed, one recent sociological analysis argues that generational trends in retirement programs will alter society's very definition of "old age," so that by the year 2000 the "young old" will be a large group of people aged 55–75, who will be healthier, more educated, and more politically involved than generations of older persons in the past.[42] And within political science, several studies have analyzed such political phenomena as partisanship, voter turnout, alienation and tolerance, attitudes toward domestic and foreign policy attitudes, and political ideology, in the context of generational-versus-maturational explanations.[43]

In studying the response of political systems to historical patterns of public demands, therefore, political scientists will increasingly have to recognize that evidence of generational trends may invalidate many of our most strongly held generalizations. And since data bases and methodologies appropriate for trend and generational analysis are becoming increasingly available,[44] the generational analysis of political variables should receive continuing and substantial attention.

---

[40] Karl Mannheim, "The Problem of Generations," in *Essay on the Sociology of Knowledge,* ed. and transl. Paul Kecskemeti (London: Routledge and Kegan Paul [1928], 1952), pp. 276–322; Julian Mariás, "Generations – The Concept," in *International Encyclopedia of the Social Sciences,* Volume VI (New York: The Free Press, 1968), pp. 88–92.

[41] Vern L. Bengtson, "The Generation Gap: A Review and Typology of Social-Psychological Perspectives," *Youth and Society,* 2 (September, 1970), 7–31; Neal E. Cutler, "Toward a Political Generations Conception of Political Socialization," in *New Directions in Political Socialization,* ed. David C. Schwartz and Sandra Kenyon Schwartz (New York: The Free Press, 1975), pp. 363–409; M. Kent Jennings and Richard Niemi, "Continuity and Change in Political Orientations: A Longitudinal Study of Two Generations," *American Political Science Review,* 69 (December, 1975), 1316–1335.

[42] Neugarten, "Age Groups in American Society and the Rise of the Young-Old," and Neugarten, "The Future and the Young-Old."

[43] Summaries and reviews of much of this literature may be found in: Neal E. Cutler, "Generational Analysis and Political Socialization," in *Handbook of Political Socialization: Theory and Research,* ed. Stanley A. Renshon (New York: Free Press, 1977); Neal E. Cutler, "Generational Approaches to Political Socialization," *Youth and Society,* 8 (December, 1976), 175–207.

[44] For a general discussion see Herbert H. Hyman, *Secondary Analysis of Sample Surveys: Principles, Procedures, and Potentialities* (New York: Wiley, 1972), Chapter 7. See also: William M. Evan, "Cohort Analysis of Survey Data: A Procedure for Studying Longterm Opinion Change," *Public Opinion Quarterly,* 23 (Spring, 1959), 63–72; Neal E. Cutler, *The Alternative Effects of Generations and Aging Upon Political Behavior: A Cohort Analysis of Attitudes Toward Foreign Policy, 1946–1966* (Oak Ridge: Oak Ridge National Laboratory, 1968); Karen Mason, William Mason, H. H. Winsborough, and W. Kenneth Poole, "Some Methodological Issues in Cohort Analysis of Archival Data," *American Sociological Review,* 38 (April, 1973), 242–258; Matilda White Riley, "Aging and Cohort Succession: Interpretations and Misinterpretations," *Public Opinion Quarterly,* 37 (Spring, 1973), 35–49; Norval D. Glenn, "Cohort Analysts' Futile Quest: Statistical Attempts to Separate Age, Period, and Cohort Effects," *American*

A second area of needed research concerns *the connection between age consciousness and political attitudes.* Social gerontologists have long studied the social-psychological correlates of age consciousness. In the view of one sociologist, age consciousness is similar to class consciousness: while everybody objectively has an age and a class, all people do not identify with that age or class.[45] Yet to the degree to which old people become aware of themselves as old, they may take on the politically relevant characteristics of a minority group or subculture.[46] Several studies have shown age consciousness to be related to such phenomena as activity, morale, adjustment, and happiness.[47]

In one recent study the chronologically old were separated into those who identified with old age and those who did not, and a number of interesting political differences were observed.[48] The subjectively old were found to be more liberal on issues of the desired scope of federal activity; more conservative with respect to such contemporary social controversies as legalization of marijuana, abortion, school bussing; and more pessimistic about both their own and the nation's economic future. The important point for political researchers is the recognition that age identification, like class consciousness, partisan identification, ethnic identification, and sex identification, is potentially a potent force in the organization of citizens and the mobilization of political activity. And when combined with the demographic changes described previously, age consciousness represents a particularly important topic for political research.

A third area of needed political research concerns *the connection between social organization and the impact of age issues and the aged upon politics.* Unlike age-based organizations of the past, modern organizations of the elderly are likely to be relatively permanent and to have a greater potential impact on politics. Such organizations as the American Association of Retired Persons and the National Council of Senior Citizens have funding sources outside of their own memberships, are bureaucratically rationalized and professionally staffed, have geographically and politically heterogeneous memberships, and are interested in a wide range of political issues.[49]

The study of the political role of age-based organizations should focus on three kinds of behavior. First, many of these organizations have large offices in Washington as well as in selected state capitals, and thus formally participate in the political process. They employ professional lobbyists, testify at hearings – especially those of the Senate Special Committee on Aging and the House Select Committee on Aging – and often draft legislation. Indeed, such national programs as HEW's network of county-based Area Agencies on Aging specifically require the local participation of organizations of the elderly in decision making.[50]

Second, many age-based organizations establish formal coalition ties with other organizations. Thus the potential political impact of the Gray Panthers and the National Caucus of the Black Aged, for example, is enhanced by their collaborative political efforts with organizations (and resources) of the non-aged.[51]

Third, a major product of age-based organizations is their work in raising the age con-

---

Sociological Review, 41 (October, 1976), 900–908; Norval D. Glenn, *Cohort Analysis* (Beverly Hills: Sage Publications, 1977).

[45] Matilda White Riley, "Social Gerontology and the Age Stratification of Society," *The Gerontologist,* 11 (Spring, 1971), Part I, 79–87.

[46] Arnold M. Rose, "The Subculture of the Aging: A Framework for Research in Social Gerontology," in *Older People and Their Social World,* ed. Arnold Rose and Warren Peterson (Philadelphia: F. A. Davis, 1965), pp. 3–16; Gordon F. Streib, "Are the Aged a Minority Group?", in *Applied Sociology,* ed. Alvin W. Gouldner and S. M. Miller (New York: The Free Press, 1965).

[47] George R. Peters, "Self-Conceptions of the Aged, Age Identification, and Aging," *The Gerontologist,* 11 (Winter, 1971), Part II, 69–73; Warren A. Peterson, "Research Priorities on Perceptions and Orientations Toward Aging and Toward Older People," *The Gerontologist,* 11 (Winter, 1971), Part II, 60–63; Donald G. McTavish, "Perceptions of Old People: A Review of Research Methodologies and Findings," *The Gerontologist,* 11 (Winter, 1971), Part II, 90–101.

[48] Neal E. Cutler, "The Aging Population and Social Policy," in *Aging: Prospects and Issues,* ed. Richard H. Davis (Los Angeles: University of Southern California Press, 1976), pp. 102–126.

[49] Henry J. Pratt, "Old Age Associations in National Politics," *Annals of the American Academy of Political and Social Science,* 415 (September, 1974), 106–119; Henry J. Pratt, *The Gray Lobby* (Chicago: University of Chicago Press, 1976).

[50] Robert B. Hudson, "Rational Planning and Organizational Imperatives: Prospects for Area Planning in Aging," *Annals of the American Academy of Political and Social Science,* 415 (September, 1974), pp. 41–54; Raymond M. Steinberg, *A Longitudinal Analysis of 97 Area Agencies on Aging,* Research Reports of the Social Policy Laboratory, Andrus Gerontology Center, University of Southern California, May, 1976.

[51] Jacquelyne Johnson Jackson, "NCBA, Black Aged and Politics," *Annals of the American Academy of Political and Social Science,* 415 (September, 1974), 138–159.

sciousness of old people. The advertisements and publications of several of these organizations specifically aim at educating their constituency about the older person's special position in society, and the potential political advantage in pursuing "senior advocacy" through organizational strategies.[52] Indeed, in recent years one need only note the civil rights movement and the women's movement as examples of the role which organizations play in translating category membership into group consciousness and group consciousness into political action.

Some observers have suggested that the aged historically have not been successfully involved in politics on their own behalf; for example, in the passage of Social Security in 1935 and Medicare thirty years later, the organized elderly were less involved and influential than were several other sectors of society.[53] Consequently, such analysis concludes, the aged are unlikely to become a substantial political force in future politics.[54] Yet evidence presented in this article suggests that past may not be prologue to the role of older people in American politics. We are beginning to see evidence that the current cohort of older persons is setting the stage for age consciousness and age-based political action. Political scientists should begin now to formulate research questions in order to see if and how over the next twenty-five years the baby boom cohort of the 1940s – socialized in the intense political atmosphere of the 1960s and 1970s and aging in a period of increasing age consciousness – will become an activist gerontology boom of the year 2000, press demands of the aged upon the political system, and thus establish new patterns for the politics of old age. As this article has endeavored to demonstrate, it is the multidisciplinary study of these emerging political patterns that provides a research guide for "political gerontology."

---

[52] See the several articles in *Advocacy and Age*, ed. Paul A. Kerschner (Los Angeles: University of Southern California Press, 1976).

[53] Robert H. Binstock, "Interest-Group Liberalism and the Politics of Aging," *The Gerontologist*, 12 (Autumn, 1972), 265–280; Dale Vinyard, "Rediscovery of the Aged: 'Senior Power' and Public Policy," paper prepared for delivery at the Annual Meeting of the American Political Science Association, Chicago, September, 1976.

[54] Binstock in particular takes the position that the elderly are most unlikely to wield substantial political influence on their own behalf either through electoral politics or interest group politics. "If government responds positively to the challenge of expanding services and facilities [for the elderly], it will be because of pervasive ideological considerations and not because it is pushed to do so by senior power." Robert H. Binstock, "Aging and the Future of American Politics," *The Annals of the American Academy of Political and Social Science*, 415 (September, 1974), 199–212.

Economic Power and Political Influence: The Impact of Industry Structure on Public Policy

LESTER M. SALAMON
Duke University

and

JOHN J. SIEGFRIED
Vanderbilt University

Few questions are as important to an understanding of American democracy as the relationship between economic power and political influence.[1] Yet few questions have received so many disparate answers from scholars and pundits alike. While few students of the subject would deny that corporations have unusual resources for political action, there is considerable disagreement over the extent of their success in bringing these resources to bear in the policy process. Depending on which examples one chooses, in fact, it is possible to conclude that American corporations are either politically impotent or politically omnipotent, with infinite variations in between. Some writers see in the extensive interchanges between corporate and governmental elites, the frequent success of corporate lobbying in both legislative and administrative realms, and the widespread pattern of private expropriation of public authority sufficient evidence to support Woodrow Wilson's charge that "the masters of the government in the United States are the combined capitalists and manufacturers of the United States."[2] But others stress the alternative sources of power in the political process and the nonpolitical activities that can absorb the energies and attention of the economic elite.[3]

While the literature on the subject contains a view of the relationship between economic power and political influence to suit almost every taste, however, there are surprisingly few systematic, empirical analyses capable of confirming or denying general propositions. In addition, there is a marked tendency to treat the entire corporate sector as a uniform entity and thus to ignore the potential variations in corporate political influence flowing from variations in the structure of particular industries. Is an industry with a few large firms more powerful politically than an industry of the same size with many smaller firms? Is it true, as former U.S. Senator Fred Harris recently wrote, that "reducing economic concentration by breaking up shared monopolies would also help reduce the political power of big business"?[4] Or would this merely increase the number of access points different industries have to government? Regrettably, the existing literature provides few signposts by which to judge the answers to these and other similar questions.

---

[1] This point is argued convincingly in: Henry Kariel, *The Decline of American Pluralism* (Stanford: Stanford University Press, 1961); Grant McConnell, *Private Power and American Democracy* (New York: Alfred A. Knopf, 1966); Theodore Lowi, *The End of Liberalism* (New York: W. W. Norton and Company, Inc., 1969); Robert A. Dahl, "Business and Politics: A Critical Appraisal of Political Science," in Robert A. Dahl, Mason Haire, and Paul F. Lazarsfeld, *Social Science Research on Business: Product and Potential* (New York: Columbia University Press, 1959), pp. 3, 44; Robert A. Dahl, *After the Revolution* (New Haven, Conn.: Yale University Press, 1970); Edwin M. Epstein, *The Corporation in American Politics* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969).

[2] Woodrow Wilson, *The New Freedom* (New York: Doubleday, 1913), p. 57. See, for example: C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956); G. William Domhoff, *Who Rules America?* (Englewood Cliffs, N.J.: Prentice-Hall, 1967); Floyd Hunter, *Community Power Structure* (Chapel Hill: University of North Carolina Press, 1953); Robert S. Lynd and Helen M. Lynd, *Middletown in Transition* (New York: Harcourt, Brace, Jovanovich, 1937); Morton Mintz and Jerry Cohen, *America, Inc.* (New York: The Dial Press, 1971); Philip M. Stern, *The Great Treasury Raid* (New York: Random House, 1967); Mark Green, *The Closed Enterprise System*, Ralph Nader's Study Report on Antitrust Enforcement (New York: Grossman Publishers, 1972); Fred R. Harris, "The Politics of Corporate Power," in *Corporate Power in America*, ed. Ralph Nader and Mark J. Green (New York: Grossman Publishers, 1973), pp. 25–44; Theodore Lowi, *The End of Liberalism*.

[3] See, for example: Robert A. Dahl, *Who Governs?* (New Haven: Yale University Press, 1961); Raymond Wolfinger, *The Politics of Progress* (Englewood Cliffs, N.J.: Prentice-Hall, 1974); David B. Truman, *The Governmental Process: Political Interests and Public Opinion*, Second Edition (New York: Alfred A. Knopf, 1971). Raymond Bauer, Ithiel de Sola Poole, and Lewis Dexter, *American Business and Public Policy* (New York: Atherton Press, 1963).

One obvious reason for this lack of research findings is the difficulty of measuring the actual extent of corporate political activity and then evaluating its success. Confronted with this difficulty, students of the subject have been forced back to second-best and third-best methodological approaches. The most common of these is reliance on illustrative examples and juicy case studies, with little effort to be comprehensive or systematic. This has led, as Andrew Scott and Margaret Hunt noted in 1960, to an image of business's political influence studded with success stories.[5] In more recent years, several more rigorous approaches have been developed, but the three best known – the reputational approach, the positional approach, and the decision-making approach – have themselves been subjected to telling methodological criticisms.[6] A fourth approach, embodied in the research seeking to account for variations in public policies among American states in terms of state political and economic characteristics, has escaped some of the methodological problems of the earlier approaches, but has so far utilized indicators of economic life that focus on general *levels* of economic activity (per capita income, degree of industrialization, etc.) rather than on the politically more relevant phenomenon of economic structure.[7]

---

[4] Fred Harris, "The Politics of Corporate Power," in Nader and Green, *Corporate Power in America*, p. 39.

[5] Andrew M. Scott and Margaret A. Hunt, *Congress and Lobbies* (Chapel Hill, N.C.: University of North Carolina Press, 1960), p. 6. Similar criticisms can be found in: Earl Latham, "The Body Politic of the Corporation," in *The Corporation in Modern Society*, ed. E. S. Mason (New York: Atheneum, 1966); Bernard Cohen, *The Influence of Non-Governmental Groups on Foreign Policy Making* (Boston: World Peace Foundation, 1959), pp. 218–236; Theodore J. Lowi, "American Business, Public Policy, Case Studies and Political Theory," *World Politics*, 16 (July, 1964), 677–715; and Samuel J. Eldersveld, "American Interest Groups: A Survey of Research and Some Implications for Theory and Method," in *Interest Groups on Four Continents*, ed. Henry Ehrmann (Pittsburgh: University of Pittsburgh Press, 1960).

[6] Each of these three approaches seeks to assess the relationship between economic power and political influence. The "reputational approach," which proceeds by collecting votes on community influentials from a panel of experts, has been criticized for focusing on presumed rather than actual power, and for beginning with a self-fulfilling prophecy in positing the existence of a power structure at the outset. The "positional approach," which focuses on the backgrounds and characteristics of the persons who hold the positions purported to be the most powerful in the society, has been faulted for confusing formal authority with the actual exercise of power. And the "decision-making approach," which concentrates on the actors involved in the making of important decisions, has been called into question for its case specific nature and its neglect of those dimensions of power involved in keeping subjects off the agenda of political decision making altogether.

For classic examples of these three approaches, see, respectively: Floyd Hunter, *Community Power Structure: A Study of Decision Makers* (Chapel Hill, N.C.: University of North Carolina Press, 1953); C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956); G. William Domhoff, *Who Rules America?* (Englewood Cliffs, N.J.: Prentice-Hall, 1967); and Robert Dahl, *Who Governs?* (New Haven: Yale University Press, 1961).

For a discussion of these approaches, see: Robert Dahl, "A Critique of the Ruling Elite Model," *The American Political Science Review*, 52 (June, 1958), 463–469; Raymond Wolfinger, "Reputation and Reality in the Study of Community Power," *American Sociological Review*, 25 (October, 1960), 636–644; Nelson Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); Peter Bachrach and Morton Baratz, "The Two Faces of Power," *American Political Science Review*, 56 (December, 1962), 947–952; Raymond Wolfinger, "Nondecisions and the Study of Local Politics," *American Political Science Review*, 65 (December, 1971), 1063–1080; Richard Merelman, "On the Neo-Elitist Critique of Community Power," *American Political Science Review*, 62 (June, 1968), 451–460.

[7] Thomas R. Dye, *Politics, Economics, and the Public: Policy Outcomes in the American States* (Chicago: Rand McNally, 1966); Richard Dawson and James A. Robinson, "Inter-Party Competition, Economic Variables, and Welfare Policies in the American States," *Journal of Politics*, 25 (May, 1963), 265–289; Charles F. Cnudde and Donald J. McCrone, "Party Competition and Welfare Policies in the American States," *American Political Science Review*, 63 (September, 1969), 959–966; Ira Sharkansky and Richard Hofferbert, "Dimensions of State Policy," in *Politics in the American States: A Comparative Analysis*, Second Edition, ed. Herbert Jacob and Kenneth N. Vines (Boston: Little, Brown and Company, 1971). Sharkansky and Hofferbert have been most comprehensive in their treatment of economic variables, examining no fewer than 15 separate variables. Though they include items as diverse as value added

What is needed, clearly, is an approach to the study of corporate political power that avoids the shortcomings of the anecdotal and case study approaches through reliance on systematic empirical techniques, but without sacrificing attention to the politically meaningful dimensions of economic structure. At a minimum, this requires work at two levels: first, at the theoretical level, to analyze the concept of economic structure and identify those aspects most likely to affect the exercise of political influence; and second, at the empirical level, to formulate ways to test hypotheses about the relationship between various aspects of economic structure and the successful exercise of political power.

This article seeks to take at least a modest step toward this goal by formulating a set of hypotheses about the likely impact of various aspects of economic structure on corporate political power, and then testing these hypotheses by assessing how well they are able to account for variations in effective federal corporate income tax rates and state excise tax rates. Central to the approach elaborated here is the notion that whatever relationship exists between economic structure and political influence in general should be evident in *variations among industries* in securing public policies favorable to them. Two basic questions thus define the main thrust of our inquiry: first, are variations among industries in securing favorable public policies systematically related to the economic structure of the industries? And second, what proportion of the variation in policy can be so explained?

To answer these questions, the analysis is divided into two parts. In the first section, we examine the features of the political system that seem to create opportunities to translate economic power into political influence, and then identify five basic aspects of economic structure that we hypothesize will affect the success an industry has in taking advantage of these opportunities. In the second section, we undertake two empirical tests of these hypotheses, one focusing on federal corporate tax rates and the other on variations in state excise tax rates, especially motor fuel excise rates.

In undertaking a systematic, empirical test of the economic structure/political influence hypothesis using cross-sectional data and regression techniques, we do not intend to dismiss the existing case study material or deny its importance. To the contrary, as will become readily apparent, this material has provided the crucial insights upon which our hypotheses are based. The contention here, therefore, is not that the case study literature is wrong or irrelevant, but rather that this approach has taken us about as far as it can, and that the field is now ripe for more rigorous hypothesis testing. While we make no claim that the tests we have devised are in any sense comprehensive or conclusive, we believe that they throw some interesting, new empirical light on a question that has been treated far less systematically in the past; and that, in the process, they may suggest some fruitful new ways to explore the important question of the political implications of economic structure more generally.

Industry Structure and Political Influence: The Theory

In general terms, two broad sets of factors shape the impact that an economic sector has on public policy: first, the nature of the political system; and second, the structure of the economic sector itself. The first of these helps define the political system's susceptibility to economic influences; the second helps determine the ability of a particular industry to take advantage of that susceptibility. While other factors – like skill, luck, timing, and leadership – also play important roles, these two set the broad parameters of the relationship between economic and political power, and therefore offer the most fruitful subjects for systematic analysis. Let us examine each in turn.

The Political Process. Perhaps the defining characteristic of the American political system is its permeability to outside pressures. Through voting, congressional testimony, campaign finance, and a host of additional means, the political system offers relatively easy access by citizens to the central policy-making process on a regular basis. While this relative permeability raises the possibility of broad public control of governmental policy *à la* pure democratic theory, however, it also raises the paradoxical possibility of translating disproportionate economic power into disproportionate political influence in a way that can frustrate broad public control. Three aspects of the political system in particular facilitate this translation.

The first of these concerns the maldistribution of incentives for political action, what

---

by manufacturing and the number of telephones per 1,000 population, however, they do not include variables measuring the structure of different industries that would make it possible to test hypotheses about the impact industry structure (e.g., firm size, market concentration) has on political influence. Yet these are the hypotheses most often generated by students of the economic aspects of political power.

might be termed the "free rider problem." As numerous democratic theorists have noted, political involvement is, like any other investment, contingent upon the expectation of a reasonable rate of return on invested resources. But since each individual consumer-taxpayer (or small firm) typically bears only a small portion of the costs and enjoys only a small share of the benefits from public programs, he rarely has the incentive to spend the energy, time, and resources needed to influence the policy. For other political actors, like large corporations, however, the potential benefits and costs of government action are sizeable enough to make political involvement a rational investment. The consequence is a gross disparity in the incentives for political involvement that works to induce the citizen-taxpayer toward passivity while stimulating the large corporation toward political activism. In his essay on *The Logic of Collective Action,* Mancur Olson provides a general theory of political activism in just these terms, arguing that groups with larger numbers can be expected to be less effective in securing collective goods than those with fewer members because

> a collective good is, by definition, such that other individuals in the group cannot be kept from consuming it once any individual in the group has provided it for himself. Since an individual member thus gets only part of the benefit of any expenditure he makes to obtain more of the collective good, he will discontinue his purchase of the collective good before the optimal amount for the group as a whole has been obtained. In addition, the amounts of the collective good that a member of the group receives free from other members will further reduce his incentive to provide more of that good at his own expense. Accordingly, *the larger the group, the farther it will fall short of providing an optimal amount of a collective good* [emphasis in original].[8]

Not only does Olson's theory suggest a difference in incentives for political activism between consumer-taxpayers and the corporate sector generally, however, it also suggests a basis for expecting variations in incentives *within* the corporate sector itself because of differences in the economic structure of different industries. It is this aspect of the free rider concept we will test below.

Not only are the *incentives* for political involvement grossly disparate, but so is the distribution of politically relevant *resources.* Large-scale corporate enterprises have important political advantages by virtue of their control over sizeable quantities of several crucial political resources: money, expertise, and access to government officials. These resources are frequently direct outgrowths of the enterprises' regular economic activities, which, in the age of the "new industrial state," frequently put the enterprise in intimate contact with scores of government officials on a day-to-day basis. By contrast, the typical consumer-taxpayer has only one resource regularly at his command: the vote. While the electoral process theoretically gives the mass of unorganized citizens a mechanism to remedy whatever inequities arise from the struggle of organized groups without incurring the costs associated with more direct political action, things rarely work out so smoothly in practice. Indeed, as Dye and Zeigler point out, the tendency to obscure policy issues in campaigns, the difficulty of reading majority policy preferences from elections, and the problem voters have holding elected officials accountable rob the vote of its utility as a determiner of policy.[9] As in his role of consumer in the corporate-controlled marketplace, so in his role as voter, the individual citizen must generally take what the prevailing structure of political interests serves up for him, although even the most powerful oligopolist will take care not to push his luck too far.

What makes this maldistribution of *incentives* and *resources* for political action so important is the highly fragmented character of the policy process in American government, which plays directly into the hands of those with organized resources focused on particular policy issues. Part of this fragmentation is dictated by constitutional provisions establishing a government of "separated institutions sharing powers."[10] But part of it grows out of the more informal arrangements these institutions have developed to process their workload. The classic example of this fragmentation is the pervasive decentralization of power in the U.S. Congress. Long ago legislators discovered that they could hold their own in the interinstitutional donnybrook that is the American political system only by channeling legislative business through specialized committees run by politically independent legislators with long tenure in the Congress. In pursuit of institutional viability, therefore, decision-making authority was fractionated into numerous relatively autonomous chunks.

---

[8] Mancur Olson, *The Logic of Collective Action. Public Goods and the Theory of Groups* (Cambridge Mass.: Harvard University Press, 1965), p. 35.

[9] Thomas R. Dye and Harmon Zeigler, *The Irony of Democracy* (Belmont, Calif.: Wadsworth Publishing Co., Inc., 1970), p. 149.

[10] Richard Neustadt, *Presidential Power,* Signet Edition (New York: The New American Library, 1964), p. 42.

What is not so clearly understood, however, is that the same fragmentation of authority that frustrates effective overall policy direction in Congress operates as well in the executive branch. Despite the deceptive symbolism of a single Chief Executive giving direction to the executive establishment, the federal bureaucracy is really a bewildering smorgasbord of institutional types with varying degrees of autonomy. Rather than unified administrative structures, Cabinet-level departments frequently resemble loose collections of warring fiefdoms, only nominally subservient to a common sovereign. Add the numerous government corporations, the quasi-independent regulatory agencies, the special commissions, the advisory committees, the singleheaded and multiheaded agencies, the institutions and institutes, and the interagency committees, and the impression of unity and consistency disappears like fog under the morning sun.[11]

What produces this administrative fragmentation is the bureaucrat's need for succor in a political system that looks somewhat askance at his very existence.[12] Since Congress is the ultimate source of agency funds and authority, it is only natural that agencies should turn first to Congress for support. But since power is fragmented in Congress, it quickly becomes so in the agencies because committee chairmen typically arrange agency structure to facilitate their own control.[13]

Agency reliance on congressional committees and their chairmen is supplemented, however, by agency dependence on client groups, particularly those with influence on the congressional committees. As one long-time government official recently noted: "Private bureaucracies in Washington now almost completely parallel the public bureaucracies in those program areas where the federal government contracts for services, regulates private enterprise, or provides some form of financial assistance."[14]

The consequences of this pattern of governmental decision making are profound. Instead of a single integrated policy process, what emerges is a series of individual, and largely independent, "policy subsystems" linking portions of the bureaucracy, their related congressional committees, and organized clientele groups in a symbiotic state of equilibrium.[15] The key to policy-making power, therefore, is access not to the political system generally but to these well-insulated and highly structured subsystems. Voter-taxpayers can occasionally gain access to the overall political system. Effective access to the subsystems, however, is typically confined to those with the expertise, resources, and influence to provide stable sources of support to key subsystem actors over extended periods of time.

Further complicating the job of the activist citizen is the predominance of administrative over legislative decision making in modern government. The theory of separation of powers notwithstanding, the bureaucracy performs crucial legislative and judicial as well as administrative functions in American government. The vast bulk of legislation considered by Congress originates in the bureaucracy, and it is the bureaucracy that makes the most potent input into the legislative process thanks to its control of information. In addition, statute law (law passed by Congress) is overwhelmed by the far greater volume of administrative law produced by the bureaucracy in the course of interpreting congressional intent. As a former commissioner of the Federal Communications Commission has put it:

> While the Courts handle thousands of cases each year and Congress produces hundreds of laws each year, the administrative agencies handle hundreds of thousands of matters annually. The administrative agencies are engaged in the mass production of law, in contrast to the Courts [and Congress], which are engaged in the handicraft production of law.[16]

This predominance of administrative rule-making puts a premium on technical expertise and on the ability to follow the complex evolution of administrative decision making. Far from expanding presidential power *per se*, the expansion of administrative rule making really contributes to the influence of the private subsystem actors who alone have the resources to retain Washington legal counsels and research staffs to analyze relevant agency decisions and respond to them effectively. The very structure of governmental decision making, like the maldistribution of incentives and resources, thus facilitates the translation of economic power into political influence.

---

[11] For an excellent analysis of the diversity and political consequences of federal administrative fragmentation see Harold Seidman, *Politics, Position and Power* (New York: Oxford University Press, 1970).

[12] Lester M. Salamon and Gary L. Wamsley, "The Federal Bureaucracy: Responsive to Whom?" in *People vs. Government: The Responsiveness of American Institutions*, ed. Leroy Riselbach (Bloomington: Indiana University Press, 1975), pp. 151–188.

[13] Seidman, *Politics, Position, Power*, pp. 37, 40.

[14] *Ibid.*, p. 18.

[15] On the concept of "policy subsystems," see J. Leiper Freeman, *The Political Process* (New York: Random House, 1965); Lee J. Fritschler, *Smoking and Politics* (New York: Appleton-Century Crofts, 1969).

[16] Quoted in Lee J. Fritschler, *Smoking*, p. 94.

Economic Structure and the Political Process. If the maldistribution of incentives and resources for political action, and the structure of the policy process facilitate the translation of economic power into political influence, the economic structure of particular industries helps determine which ones are in the best position to take advantage of this opportunity. But which aspects of economic structure are most germane? What types of differences among industries are most relevant in explaining variations in their levels of political influence?

To answer these questions, it is necessary to turn to the literature on industrial organization and to evaluate this literature in the light of political science findings about the effectiveness of various corporate pressure group tactics.[17] What emerges most clearly from such an evaluation is the hypothesis that *firm size,* the average size of firms in an industry, is the most important aspect of economic structure so far as the implications for political influence are concerned. Much of the concern about conglomerates and rising aggregate concentration in the American economy, in fact, grows out of the fear that large firms wield not just disproportionate economic power, but also disproportionate political influence,[18] for a variety of reasons. In the first place, to the extent that public policies deliver benefits proportional to the size of the firm (which is the case, for example, with the income tax system), then the larger the firm the greater the incentive to participate in politics, for reasons suggested by Olson. But larger firms not only have greater incentives to participate in politics, they also have larger resources with which to do so. Kaysen[19] and Edwards[20] have both suggested that economic power is an important source of political power, either directly, through campaign finance, or indirectly, through the purchase of expertise to generate information instrumental in the policy process and liaison agents to transmit this information to the relevant policy makers. A National Industrial Conference Board study found that large firms tended to represent their legislative views more frequently than did smaller firms.[21] By the same token, Bauer, Poole, and Dexter found that firm size is an important determinant of the political activity of executives, since the executives of large firms could afford the luxury of hiring staffs and taking the time to inform themselves about policy issues.[22]

What makes the absolute size of available resources, and hence firm size, so important politically is the fact that political involvement has certain fixed costs attached to it (we will call this the "threshold problem"). Like a single pain-reliever commercial, corporate political activity is likely to produce little payoff unless sustained over a period of time and spread over a variety of policy-making arenas. Since small firms can rarely sustain these high fixed costs, they are generally constrained to channel their political involvement more extensively through trade associations, with all the intra-organizational differences, lack of control, and consequent weakening of influence that carries with it.[23]

Access to economic resources is not the only political asset arising from larger firm size. Also important is the prestige typically accorded individuals who reach the command posts of the larger corporate enterprises, a prestige that is transferable into political influence through the numerous advisory committees and personal contacts that bind the worlds of business and government together. Equally important is the larger pool of expertise potentially available to the larger firms, and, on some occasions, the larger number of employees and stockholders who can be mobilized (or threatened to be mobilized) for political action. Other things being equal, therefore, we hypothesize that an industry containing large firms will have greater political influence than an industry of the same size but composed of more numerous small firms. At the same time, we expect this influence to be mitigated somewhat by the visibility of large firms and the consequent dangers they face of attracting adverse public attention.

---

[17] We are indebted here to Edwin Epstein, *The Corporation in American Politics* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1969), particularly pp. 67–111 for an excellent review of patterns of corporate political involvement.

[18] See, for example, Carl Kaysen, "The Corporation: How Much Power? What Scope?" in *The Corporation in Modern Society,* ed. E. S. Mason (Cambridge, Mass.: Harvard University Press, 1959), p. 99; Kaysen asserts: "The market power which large absolute and relative size gives to the giant corporation is the basis not only of economic power but also of considerable political and social power of a broader sort." According to Senator Fred Harris: "In any area, the size of a big corporation can be translated into political strength." Harris, "Politics of Corporate Power," p. 39. Ronald H. Coase writes that "if policy is to be based on 'fear of size,' it is surely desirable to discover what is really feared, whether it results from size, and whether this comes about in all circumstances or only in some" [*Congressional Record* (June 16, 1969)].

[19] Kaysen, "The Corporation."

[20] Corwin D. Edwards, "Conglomerate Bigness as a Source of Power," *Business Concentration and Price Policy* (Princeton, N.J.: Princeton University Press, 1955), pp. 331–352.

[21] National Industrial Conference Board, *The Role of Business in Public Affairs,* Studies in Public Affairs, No. 2 (New York: National Industrial Conference Board, Inc., 1968), p. 8.

[22] Bauer, Poole, and Dexter, *American Business and Public Policy,* p. 227.

[23] Commenting on the relative advantages of having a full-time firm representative in Washington and working through a trade association, Corwin Edwards notes: "While some smaller business interests make a comparable showing through associations set up for the purpose, the experience of a Washington official is that small companies generally find out what is happening too late and prepare their case too scantily to be fully effective where their interests conflict with those of large companies." Edwards, "Conglomerate Bigness," p. 347.

To assess the impact of firm size on political power, however, it is necessary to control for the effects of *industry size,* for the obvious reason that the size of the firm may really be a function of the overall scope of the industry. But industry size also probably affects political power in its own right as well, independently of the size of firms within the industry. If the average firm size is held constant and the overall industry size increases, the number of firms in an industry must increase. Under Olson's thesis, this should be associated with a reduction in the political effectiveness of the industry, since Olson argues that the incentives for any particular actor to work to secure collective benefits decline as the number of members in the group increases. This would lead us to hypothesize an inverse relationship between industry size and political influence, other things being equal.

A competing thesis can also be found in the literature, however. This thesis holds that large industries, whatever the average size of firms comprising them, will have greater political influence because they will have larger pools of money, talent, client support, contacts, and related resources than smaller industries. Nevertheless, on balance we subscribe to the Olson "free rider" interpretation as a working hypothesis and therefore posit a negative relationship between industry size and political influence.

If firm size and industry size are related to each other, both in turn are likely to be related to a third aspect of economic structure: *market concentration,* usually defined as the proportion of an industry's sales or assets accounted for by its four (or eight) largest firms. For a given average firm size, an increase in industry size would most likely be associated with a decline in market concentration. By the same token, for a given industry size, an increase in typical firm size will likely be accompanied by an increase in market concentration. Our firm size and industry size variables could thus inadvertently be measuring market concentration. This is important because there is reason to expect that market concentration also affects political influence independently of the effects of firm size and industry size. In the first place, market concentration typically yields higher profits[24] that can be channeled into political activity. A more competitive industry of roughly the same size would be likely to earn fewer profits and hence lack some politically relevant resources that might be available to it if it were more highly concentrated. More concentrated industries also have the advantage of avoiding the debilitating and time-consuming process of negotiating industry positions on political matters among numerous competing firms, a process that has frequently been cited as a major obstacle to trade association political effectiveness.[25] In eliminating competitors in the economic marketplace, highly concentrated industries also eliminate competition in framing industry positions in the political marketplace. In addition, the more concentrated the industry, the less severe the "free rider problem" it encounters, since firms that dominate an industry receive a larger share of whatever benefits investment in political activity brings. Hence they have a greater incentive to make this investment.

Partially counter-balancing these political advantages of economic concentration, however, are some liabilities. For example, concentrated power, particularly when combined with large firm size, is likely to enhance corporate visibility. As Bauer, Poole, and Dexter put it: "A business can be too big to be politically effective along some lines . . . nowadays many really big corporations are not eager to dance among the chickens. . . ."[26] Closely related to the visibility problem is the "wear out your welcome" problem. When a few firms must carry the entire burden of representing an industry in the public policy-making arena, they run the risk of overdrawing on their account of goodwill with key policymakers, particularly since most policymakers can scant afford the appearance of subservience to a small group of firms. Nevertheless, on balance, we believe the presumptive evidence supporting a *direct* relationship between market concentration and political power is the more persuasive for hypothesis-framing purposes.

---

[24] For an excellent survey of the evidence relating market concentration to profit rates see Leonard W. Weiss, "Quantitative Studies of Industrial Organization," in *Frontiers of Quantitative Economics,* ed. M. D. Intrilligator (Amsterdam: North-Holland Publishing Company, 1971), pp. 362–379. Weiss concludes that "practically all observers are now convinced that there is something to the traditional hypothesis. . . . Almost all of the 32 concentration-profits studies except Stigler's have yielded significant relationships for years of prosperity or recessions, though they have depended on a wide variety of data and methods" (p. 371).

[25] See, for example, Bauer, Poole, and Dexter, *American Business and Public Policy,* pp. 332–340; and David Truman, *The Governmental Process,* pp. 156–187.

To test the independent effect of market concentration on political influence, however, it is also important to control for the effects of *profit rate* differentials given the evidence we have just cited about the relationship between profit rates and concentration. But profit rate is also likely to affect political influence independently of market concentration because of the link between profitability (whether caused by concentration or not) and the availability of resources necessary for political effectiveness. What is more, highly profitable firms have more to gain from efforts to alter policies such as the federal income tax which place a larger absolute burden on high-profit firms.

At the same time, however, the political efforts to achieve these favorable policies may produce adverse consequences that make them not worth the price. For example, firms earning higher than average profits may shy away from political action if they fear that such action would attract public attention to the monopoly position that yields them these large profits. By the same token, extensive political pressure tactics could hinder pursuit of the kinds of corporate goals stressed by the "new theories of the firm" – goals that go beyond profit maximization to focus on managerial prestige, sales revenue maximization, or "satisficing" behavior.[27] On balance, we favor the first of these two sets of explanations and thus expect a positive relationship between profit rate and political influence.[28]

A fifth aspect of economic structure that seems likely to influence the level of political success of an industry is its *degree of geographical dispersion.* With respect to this aspect of economic structure too, however, opinions differ markedly about the consequences for political impact. If we assume that each member of Congress has many constituent groups in his district and that he devotes attention to their desires in proportion to their likely impact on his re-election, then it follows that an industry must be sufficiently large in order to attract the attention of its legislative representatives. For a given firm size and industry size, industries will be more successful in attracting this attention to the extent that they are geographically concentrated. As one participant in a Brookings Institution round table put it: "I can't think of anyone who has more of an impact on a congressman than a representative of a corporation having a big plant in his area."[29] Presumably, if the same industry's plants were more dispersed, and hence smaller, its ability to secure the support and attention of its local representatives would be reduced.

A competing hypothesis with respect to geographical concentration is that highly concentrated industries lack the broad-based political support necessary to pass legislation and have it implemented. As Senator Hart has noted, "when a major corporation from a state wants to discuss something with its political representatives, you can be sure he [sic] will be heard. When that same company operates in thirty states, it will be heard by thirty times as many representatives."[30] Given this reasoning, dispersed industries would be more effective than concentrated ones. On balance, however, we find the first argument more convincing, and posit an inverse relationship between geographical dispersion and political influence once we have controlled for firm size and industry size.

---

[26] Bauer, Poole, and Dexter, *American Business and Public Policy,* p. 266.

[27] It has been proposed that firms sometimes do not maximize the attainment of some well-defined objective function, but rather "satisfice." That is, they seek choices which satisfy at least minimum levels of aspiration with respect to a variety of objectives. For example, management may set a target rate of return on invested capital as its profit objective, and if that target is achieved, it turns to the satisfaction of other nonprofit objectives. See Herbert Simon, "Theories of Decision Making in Economics and Behavioral Science," *American Economic Review,* 49 (June, 1959), 253–283; Robert F. Lanzillotti, "Pricing Objectives in Large Companies," *American Economic Review,* 48 (December, 1958), 921–940; William Baumol, *Business Behavior, Value and Growth,* rev. ed. (New York: Harcourt, Brace and World, Inc., 1967); Oliver Williamson, *The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964).

[28] The introduction of a control for profit rate will also probably change the expected direction of the relationship between market concentration and political influence. This is so because the positive political impact attributed to concentration was based largely on the expected higher profits of concentrated industries. With this factor controlled for, the visibility and "wear out your welcome" problems may dominate the relationship. Under these circumstances, we should expect an inverse relationship between market concentration and political influence.

[29] Paul W. Cherington and Ralph L. Gillen, *The Business Representative in Washington* (Washington, D.C.: The Brookings Institution, 1962), p. 55.

[30] Senator Phillip Hart (D-Michigan), Speech to the Lawyers Club in Ann Arbor, Michigan, April 8, 1968,

Altogether, therefore, we have identified five dimensions of economic structure that appear, from the available literature and *a priori* reasoning, to be likely to affect an industry's level of political influence. Other dimensions of economic structure – such as level of unionization of firms, capital-output ratio, etc. – could also be considered, but we believe these five are the most promising to explore. In the case of at least four of them, moreover, they are significantly interrelated. Table 1 below depicts this fact, showing the correlations among these five dimensions of economic structure for the one hundred and ten industries identified in the Internal Revenue Service's "minor industry" classification of mining and manufacturing firms.[31]

The crucial question to consider, however, is not whether these dimensions of economic structure are related to each other, but whether they are related to the successful exercise of political influence, both independently and as a group. To answer this question, we must formulate suitable indices of the successful exercise of political influence, translate our economic structure variables into measurable form, and test empirically for the impact of the latter on the former. It is to these tasks that we now turn.

Empirical Tests

To date, the few efforts to analyze the relationship between economics and public policy have either neglected to consider economic structure as a variable[32] or have treated this variable through essentially case specific decision-making approaches.[33] While we have built on these approaches to develop our hypotheses, we have found them inadequate for hypothesis testing, the first because we believe that economic structure is an important variable and that there are ways to measure its politically relevant dimensions; and the second

Table 1. Correlations Among Five Dimensions of Economic Structure for 110 IRS "Minor Industries" in Mining and Manufacturing

Variable[a]	F	I	C	R	D
Firm Size (F)	1.000				
Industry Size (I)	.276	1.000			
Concentration (C)	.448	–.280	1.000		
Profit Rate (R)	.419	.107	.310	1.000	
Geographical Disperson (D)	.120	–.172	.493	.069	1.000

[a] Units of measurement for each variable are discussed in the text below and summarized in the "measurements" column of Table 2.

---

cited in Mark J. Green, *The Closed Enterprise System,* p. 19. Senator Fred Harris concurs, noting in a recent article that: "A company with plants in ten congressional districts can expect more congressional champions than a company with plants in only five districts." Fred Harris, "The Politics of Corporate Power," p. 39.

[31] The IRS classifies all economic activity into "major industry" groups. Within each group are further subdivisions, called "minor industries." Firms are classified into "minor industries" on the basis of their predominant product. Of the 129 "minor industries" in mining and manufacturing identified by the IRS, fifteen are "not allocable" categories in the major industry groups. The firms in these categories usually do not provide sufficient information to permit the IRS to classify them into a substantive minor industry classification. In general, these industries are small relative to the other "minor industries" in the sample, and the firms in these industries are not likely to have homogeneous political interests. In addition, four other industries were omitted for various reasons that prohibited calculation of their effective average corporation income tax rates, one of the major dependent variables used in the tests below. Accordingly, the correlations reported here relate to 110 of the 129 minor industries identified.

[32] See footnote 7 above.

[33] In his study of New Haven, for example, Robert Dahl evaluates the influence of economic notables by studying a collection of decisions in four key policy arenas in New Haven over a ten-year span. Since these notables participated infrequently in decisions in almost all of the four policy areas, Dahl concludes that economic power is not translated effectively into political power in New Haven. Similarly, Bauer, Poole, and Dexter, in assessing business influence on U.S. trade policy, scrutinized the degree of business participation in the actual policy-making arenas during consideration of trade legislation. In addition, they carefully analyzed the flow of information within the business community and its trade associations concerning foreign trade matters. On both counts, they concluded that business influence was marginal, despite what many would take to be high economic stakes in the issue. Businessmen generally had little knowledge about trade matters, and their lobby

because we believe, with Bachrach and Baratz, that the decision-making approach fails to take account of the numerous items that economic notables keep from becoming issues in the first place, and because we feel it is not suited to broad-gauged empirical testing.

The approach we have adopted, therefore, is to formulate a measure of the successful exercise of political power that can be used to generate an index of political effectiveness for *all* industries, and that can be related to indices of economic structure. Accordingly, we measure power, in Bertrand Russell's terms, as "the production of intended consequences."[34] In other words, what we propose to do is to measure the success with which an industry exerts political influence in terms of the extent to which policies emerge that benefit it.

This approach is similar to the one used in economic analyses of firm performance, where relationships between firm structure and performance are measured without examining the intervening variable of firm conduct. This approach is used by economists because of the near-impossibility of quantifying the conduct variable.[35] Clearly, the same problem obtains in dealing with the intermediate variables in the political influence process. Measuring the actual exercise of political influence – lobbying, campaign contributions, informal contacts, and so forth – across numerous industries is highly sensitive to the gross imperfections in the information reporting requirements and the resulting glaring gaps in data. Under the circumstances, systematic empirical testing of the economic structure/political influence hypothesis can only occur through reliance on the structure/performance paradigm of economic analysis. At the very least, it should be fruitful to explore what contributions this paradigm can make to political analysis as well.

In choosing which policy outputs to employ as our measures of the successful exercise of power, we searched for those that were not only substantively important but also susceptible to empirical treatment and broad in their coverage. Tax policies thus emerged as the leading contenders. In particular, we focused on federal income tax policy and state excise tax policy. Both of these are universal in their application yet varied in their impact, thus offering an opportunity for systematic analysis. What is more, both are cases of what Theodore Lowi has termed "redistributive" policy arenas, which typically involve substantial class conflict and opposition to economic elite dominance.[36] To the extent that we identify a link between economic structure and the successful exercise of power in these arenas, therefore, we will have established an even more dramatic confirmation of the economic structure/political influence hypothesis than would be the case if we were looking only at a "regulatory" arena. Even so, readers should be cautioned that our findings in these two arenas may not be generalizeable to other policies and that we make no claims about the representativeness of our dependent variables. With these caveats in mind, we can now turn to the empirical tests themselves.

Federal Corporate Income Tax Rates: Empirical Test I. Under the federal income tax laws in effect at the time for which we collected our data, all firms were required to pay 52 per cent of their taxable income in taxes. However, the Internal Revenue Code includes numerous "special" tax provisions (such as the investment credit, the special capital gains rate, the surtax exemption for income under $25,000, the depletion allowance, etc.) that reduce the effective tax rate below this 52 per cent level. For each of the 110 "minor industries" for which tax data were available, we computed the effective average tax rate produced by these special tax provisions.[37] Our dependent variable, then, is the deviation of this effective tax rate from the 52 per cent rate. This "tax avoidance rate" measures the success with

---

organizations proved incapable of seriously influencing consideration of the policy.

[34] Bertrand Russell, *Power: A New Social Analysis* (London: Allen & Unwin, 1962), p. 25.

[35] For a clear statement of this approach, and of the argument for it, see Joe S. Bain, *Industrial Organization* (New York: John Wiley and Sons, Inc., 1968), p. 329.

[36] Actually, Lowi is inconsistent in characterizing the appropriate category for tax policy, referring to disputes over taxes at one point as classic examples of "distributive politics," and at another point as classic examples of "redistributive politics." Our data may provide an empirical basis for deciding which of these views is most accurate, for to the extent that we can explain variations in tax impact in terms of industry economic structure, we will have discredited the view that tax advantages are distributed among industries on something resembling the first-come-first-served basis of a distributive arena. See: Lowi, "American Business, Public Policy," pp. 705–707.

[37] The effective average tax rate is the ratio of actual tax liabilities to estimated "true" accounting net income, which is the industry's reported income plus the amount it was able to exclude from its reported income because of the special provisions in the Internal Revenue Code. For a detailed description of the effective average tax rate see John J. Siegfried, "Effective Average U.S. Corporation Income Tax Rates," *National Tax Journal*, 27 (June, 1974), 245–259.

which an industry has reduced its tax burden below the standard 52 per cent rate thanks to the operation of the "special provisions" in the code. As shown in Appendix Table 1, the tax avoidance rate varied from a high of 35.8 per cent for the Miscellaneous Nonmetallic Minerals industry to a low of 3.9 per cent for the Newspaper industry.

Based on our earlier discussion, we hypothesize that the size of the Tax Avoidance Rate is a function of an industry's economic structure, as reflected in its firm size, industry size, level of market concentration, profit rate, and degree of geographical concentration. The equation below expresses this model succinctly:

$$TAR = 0.52 - t = f(F,I,C,R,D)$$

where

TAR = tax avoidance rate
t = the effective average corporation income tax rate
F = firm size (millions of dollars)
I = industry size (number of employees)
C = market seller concentration (percentage points)
R = the rate of "true" accounting profit plus interest on total assets (percentage points)
D = geographical concentration (index number based on employment)

In positing a relationship between economic structure and the Tax Avoidance Rate, of course, we are not claiming that this is the only explanation of variations in tax avoidance rates among industries. It is conceivable that many of the "special" tax provisions were instituted on the basis of sound public policy, independent of the influence of interested parties. Alternatively, the effect of some of the laws and rules on particular industries may be "flukes," instituted either in error or as unnoticed side-effects of other well-designed policy decisions. Neither of these explanations is ruled out by our model. What our model will tell us, however, is whether any portion of the variation in Tax Avoidance Rates can be attributed to variations in political influence flowing from variations in economic structure, and, if so, which aspects of economic structure are most important.[38]

To test the relationship between economic structure and Tax Avoidance Rates, we employed multiple regression techniques.[39] Throughout the analysis, we rely on the Internal Revenue Service's *Statistics of Income* and the Bureau of the Census's *Census of Manufactures* for 1963 to measure both our independent and dependent variables. The year 1963 was used because of the unavailability of data to measure some of the independent variables in later years. For example, the 1967 *Census of Manufactures* failed to provide sufficient detail on the location of manufacturing plants to compute the geographical dispersion variable, and the necessary 1972 data were not yet published at the time we conducted our research. In addition, certain of the computations that went into the derivation of the effective average corporate tax rate for each industry relied on Treasury studies completed in the 1950s, and few of these have been updated. For all these reasons, 1963 data were most appropriate. Since we are more interested in the existence of the relationships hypothesized than in their size at some particular point in time, moreover, this use of 1963 data is less serious than it might otherwise be, especially given our expectation that these relationships are likely to be fairly durable.

To test our hypotheses about the political impact of variations in economic structure, we converted each of our dimensions of economic structure into measurable terms and tested its impact on the Tax Avoidance Rate, using multiple regression techniques to control for the impact of the other factors in the model. Table 2 below summarizes the results of this analysis, along with the measurement used for each variable, and the relationship hypothesized on the basis of our theory. (Appendix Table 2 records the simple correlation between each of our independent variables and the Tax Avoidance Rate.)

Firm Size: The first thing that emerges from this table is that the hypothesized relationship between firm size and political influence, which formed the heart of our inquiry, is supported by the data. As noted in the table, firm size was measured by taking the asset size of that firm which accounts for the median asset dollar in an industry. When this factor was tested for its effect on the corporate Tax Avoidance Rate, while controlling for the effects of our four other measures of economic structure, the results were statistically significant and in the expected direction. A one per cent increase in firm size was found to be associated with a .08 per cent increase in the Tax Avoidance Rate. Even though it is difficult to evaluate the practical importance of this degree of change, it is nevertheless significant to have validated this hypothesis empirically across so large a number of observations.

---

[38] These opposing arguments about the causes of variations in tax policy are nicely summarized by Louis Eisenstein, who characterized the tax policy process as "a continuing struggle among contending interests for the privilege of paying the least." According to Eisenstein, to view the tax policy process in any other way is just a "pleasing illusion." Louis Eisenstein, *The Ideologies of Taxation* (New York: The Ronald Press Company, 1961), pp. 3–4. See also Stanley S. Surrey, "The Congress and the Tax Lobbyist: How Special Tax Provisions Get Enacted," *Harvard Law Review*, 70 (May, 1957), 1145–1182.

[39] To determine the proper functional form of the hypothesized relationship, we employed the Box-Cox maximum likelihood method, which suggested that a multiplicative (nonlinear) form of our equation was most appropriate. For further discussion of this technique, see Paul G. Hoel, *Introduction to Mathematical Statistics* (London: John Wiley and Sons, Inc., 1962), pp. 220–225. In a multiplicative model, the reported coefficients are interpreted as the percentage change in the dependent variable produced by a one per cent increase in the value of the independent variable.

Industry Size: Unlike the firm size variable, the industry size variable seems to have little significant impact on the Tax Avoidance Rate. Industry size was measured in terms of total employment, which allowed us to tap the political influence represented by employee dependence on the industries that employ them. As noted, we had theoretical reasons to expect a positive relationship between industry size and tax avoidance, on grounds that larger industries had more resources with which to overcome the threshold problem and greater numbers of potential supporters among voters. But we also had reason to expect a negative relationship, on the grounds that larger industries would encounter the "free rider" problem more severely. Therefore, we subjected this hypothesis to a more rigorous, two-tailed significance test. The results suggest that the "free rider" hypothesis is the more accurate one – a finding that makes sense in light of the many competing interests that vie for the voter patronage of industrial workers. On tax matters in particular, it seems more likely that employees will support a position contrary to that of management, especially if they are unionized, since unions recognize that taxes not paid by business will result either in a reduction of public services or an increase in other taxes. But the observed negative relationship, though consistent with our earlier hunch, is too weak to give us more than 90 per cent confidence that it did not occur merely by chance.

Market Concentration: In the case of *market concentration*, the results show a significant impact on the Tax Avoidance Rate, but in a direction opposite to what we originally expected. Market concentration was calculated on the basis of the proportion of all industry shipments accounted for by the four largest firms in the industry.[40] We hypothesized that

Table 2. Relationship Between Tax Avoidance Rate and Five Aspects of Economic Structure, 110 IRS "Minor Industries"

Variable	Measurement	Hypothesized Sign[a]	Regression Coefficient	t-ratio
Firm Size	Asset size of firm which accounts for the median asset dollar in an industry	+	.084**	2.42
Industry Size	Employees	+/–	–.095	–1.72
Market Concentration	Per cent of sales accounted for by four largest firms	+/–	–.512**	–2.94
Profit Rate	Net income plus interest as % of assets	+/–	–.301*	–2.46
Geographical Concentration	Index based on distribution of employees across states; higher values indicate more concentration	+/–	–.017	–0.15

[a] A positive sign indicates that we hypothesize a direct relationship between the independent and dependent variables. A positive and negative sign signifies that we have found theoretical reasons to expect either a direct or indirect relationship. In such cases, we subject our results to a two-tailed significance test.

**Statistically significant at the .01 level.
*Statistically significant at the .05 level.
R^2 (coefficient of determination) = .158
Fisher's F statistic = 3.892*
Standard error of the estimate = .465
Number of observations = 110

---

[40] To compute this variable for each "minor industry," we employed the concentration ratios provided for the Census Bureau's more narrowly defined Standard Industrial Classification of firms. These concentration ratios for the SIC industries were then aggregated, using a weighted average based on employ-

market concentration would be associated with a high level of political influence, since concentrated industries could expect to avoid the "free rider" problem more easily than nonconcentrated industries. At the same time, we noted that highly concentrated industries would face some disabilities in exercising political influence as a result of their fears about arousing public attention and what we called the "wear-out-your-welcome" problem. As is apparent from Table 2, this latter argument is the one that finds support in the evidence. A one per cent increase in market concentration is associated with a .512 per cent decrease in Tax Avoidance Rates. One reason for this finding could be that our measure of market concentration does not capture the "free rider" problem as accurately as a simple measure of the number of firms in an industry, which is already partially accounted for in the industry size variable. Indeed, with the "free rider" and higher profit rate aspects of market concentration factored out (which is what our controls for firm size, industry size, and profit rate essentially do), it is understandable that the independent effect of market concentration on the successful exercise of political power is negative.

Profit Rate: The profit rate variable also turns out to be negatively related to success at tax avoidance. Profit rate is defined here as the ratio of net income plus interest to total industry assets, where net income is calculated by taking reported taxable income and adding to it the items excluded from reported taxable income thanks to the special tax provisions. On the basis of the available literature, we suggested that industries with higher profits would be most effective politically because of their access to more substantial resources and the greater incentive they would have to reduce taxes on their profits. Yet, we also noted that such industries had a strong incentive to abstain from efforts to lower their effective average tax rates for fear that extensive political involvement might draw attention to the favorable market environment responsible for their high returns and thus invite government regulation. In addition, we suggested that such firms, having achieved their profit objectives, might be pursuing other objectives that are inconsistent with the pursuit of political influence.

The significant negative relationship between profit rate and Tax Avoidance Rate recorded in Table 2 suggests that these latter arguments are more accurate. Profit rate does have a significant impact on the successful exercise of political influence as measured here, but for reasons associated with the favorable-market-environment-more-to-lose hypothesis, not the higher-profits-more-resources-more-to-gain hypothesis.

Geographical Concentration: Of all the measures of economic structure used here the geographical concentration factor turns out to have the least discernible impact. Geographical concentration was measured by an index based on the distribution across states of employment in each of the 110 Census-defined industries.[41] The more concentrated the employment of the industry in a few states, the larger the concentration index.

We hypothesized that since a highly concentrated industry would have the greatest success in attracting the attention of at least one set of legislators, it could be assured of having its interests represented in the political process. Here, too, however, an alternative hypothesis also made sense, namely that the more dispersed an industry the more access it could expect to have in the political process. The data reported in Table 2 do not provide sufficient evidence to favor either argument. However, it could be that both factors are at work, but that their approximately equivalent impacts cancel out in our empirical test.

Taken together, the five dimensions of economic structure incorporated in our model explain 16 per cent of the variation in corporate tax avoidance rates, and do so at a confidence level exceeding 95 per cent. For a model seeking to explain so complex a phenomenon over so large a cross-section of observations, this is an encouraging result. What it suggests is that variations in economic structure do affect the success of various industries in securing public benefits (or avoiding public costs), even though such variations explain only a portion of this success. Beyond this, our findings suggest the feasibility of testing the widely hypothesized link between economic structure and political influence in a more systematic, empirical fashion than has been done in the past. In this regard, our discovery

---

ment, to derive the concentration ratios for the "minor industries" which the SIC firms comprise.

[41] This index consists of the sum (across states) of the squared share of total industry employment located in each state in 1963. An index of this type is sensitive to both the number of states in which an industry has employees and the distribution of employees among these states. Fewer states or a more unequal distribution will cause the index to rise. Squaring the shares weights the large employment states (and hence the more important states) more heavily in the index.

of a positive relationship between firm size and Tax Avoidance Rates is particularly significant, since it suggests an empirical base for the long-standing argument that antitrust policy is necessary to avoid not just undue concentrations of economic power but also threatening concentrations of political power.

This latter conclusion is further substantiated, moreover, when we take the analysis one step further and examine variations in tax avoidance *within* industries, not just variations among them. For this purpose, we examined the petroleum refining industry in particular, calculating the tax avoidance rates for categories of firms within this industry grouped by asset size.[42] Table 3 below records the results of this analysis. What it shows is that the very largest oil refiners, in particular the integrated "majors," have been best able to take advantage of the special tax provisions that affect business in general and the petroleum industry in particular.[43] This finding supports the results of our cross-industry analysis reported above. Not only do industries with larger firm sizes have larger tax avoidance rates, but also the larger firms within those industries do better than the smaller ones.

Table 3. Tax Avoidance Rates (TAR) for Petroleum Refining Industry, 1967

Asset Size Class ($1,000,000)	TAR_1	TAR_2
5–10	14.3	14.8
10–25	0.2	8.2
25–50	14.5	14.5
50–100	13.0	13.2
100–250	34.8	35.4
Over 250	27.9	42.3

$$TAR_1 = 0.52 - \left(\frac{\text{tax liabilities}}{\text{"true" accounting profits}}\right),$$

treating foreign taxes entirely as a substitute for U.S. taxes; foreign tax credit not a "special" provision.

$$TAR_2 = 0.52 - \left(\frac{\text{tax liabilities} - \text{foreign tax credit}}{\text{"true" accounting profits}}\right),$$

treating the foreign tax credit as a "special" tax provision.

Before we can have confidence in any of these tentative conclusions, however, it is necessary to determine whether they are peculiar to the federal income tax arena. Accordingly, we turn our attention to an aspect of state policy to determine whether the economic structure/political influence hypothesis finds empirical support there as well.

State Gasoline Excise Tax Rates: Empirical Test II. In assessing the impact of economic structure on state politics and policy, it is necessary to alter the mode of analysis somewhat. Instead of focusing on industry-by-industry variations in policy impact, the focus here must be on state-by-state variations. What we have done, therefore, is to identify a range of policy that exists in all the states, and then examine state variations in that policy in terms of state-by-state variations in the strength and structure of the economic sectors most affected by it.

The motor fuel excise tax is an excellent candidate for this kind of scrutiny. Every state has such a tax, and the tax rate varies significantly from state to state. In twenty-eight of the states, motor fuel excise tax receipts are "dedicated" to highway construction by constitutional amendment. Not surprisingly, major oil firms have not opposed increases in motor fuel excise tax rates in these states, since these taxes construct highways that ultimately help to increase the demand for gasoline. However, in twenty-two of the states, where motor fuel tax receipts are not dedicated to highway construction, the oil companies have generally lobbied against such increases.[44]

Since state policy making has traditionally been considered even more vulnerable than federal policy making to private interest pressure,[45] we expect significant relationships between state-by-state variations in petroleum industry economic structure and state motor fuel excise tax policies, both the policies on "dedication" and the policies on tax *rates*. Of particular concern are the impacts of two dimensions of state petroleum industry structure: first, the overall size of this industry in the state, measured as a share of state personal income; and second, the extent to which large firms dominate this industry within the state. Based on earlier hypotheses about the impact of economic structure on political influence, we hypothesize, first, that states in which the petroleum industry is large will be more likely to dedicate their motor fuel excise taxes to highway construction than is the case for states without large petroleum industries; and second, that among nondedication states – those that do not dedicate motor fuel excise taxes to highways – the motor fuel excise tax rate will vary indirectly with both the overall size of the petroleum industry in the state and the dominance of large firms within the industry. This latter hypothesis, which suggests that motor fuel excise tax rates will be lower where the petroleum industry is both large and dominated by large firms, reflects the industry's opposition to special taxes on its products when the resulting revenue is not tied to highway construction. In the case of the dedication states, where the petroleum industry has taken a generally neutral stance towards proposed increases in motor fuel excise tax rates (because these taxes are earmarked for further highway expenditures that can be expected to boost gasoline sales), we hypothesize little relationship between our two measures of industry structure and motor fuel excise tax rates.

---

[42] Those firms having less than five million dollars of assets were excluded because many of them showed deficits, and we were not able to remove these effects from the data. Furthermore, firms with less than five million dollars in assets are not representative firms in the petroleum refining industry.

[43] For a more detailed discussion of the petroleum industry within the framework developed here, see: Lester M. Salamon and John J. Siegfried, "The Relationship Between Economic Structure and Political Power: The Energy Industry," in *Competition in the Energy Industry*, ed. Thomas D. Duchesneau (Cambridge, Mass.: Ballinger, 1975).

[44] Information provided by Mr. William Haga, representative of major oil company interests in the State of Tennessee. Since 1971 some major oil companies have taken a position of not opposing gasoline taxes which are used for nonhighway purposes if they are dedicated instead to mass transit purposes. Not all companies currently take this position. Since our empirical test is for 1967, however, this change in policy will not affect our hypothesis. Furthermore, we are seeking to discover evidence of general relationships between economic structure and political decision making, and consequently the relevance of our analysis is not diminished by a change in the policy of some oil companies.

[45] On the permeability of state legislatures to special interest pleading, see: Terry Sanford, *Storm over the States* (New York: McGraw-Hill Book Company, 1967).

To test these relationships, we developed measures of the overall size of the petroleum industry in each state and the degree of dominance of large firms within this industry in each state. The first of these measures was computed as the ratio of the value added by the petroleum industry in each state to the personal income of the state. The second was computed as the ratio of the number of firms greater than a fixed employment size to the total number of firms in the industry in each state.[46] In both cases, we computed our indices separately for the crude oil production and petroleum refining segments of the petroleum industry. This was done because there is reason to believe refiners are relatively more interested in excise taxes than crude oil producers, both because a relatively greater proportion of refinery products go into motor fuel production and because a much larger proportion of refinery products are consumed at home, in the states where the refineries are located.

To assess the validity of the first hypothesis, i.e., that states with large petroleum industries are more likely to dedicate their motor fuel taxes to highway construction because of the political clout of this industry in state politics, we grouped the states in terms of two factors: whether they contain large or small petroleum industries, and whether or not they dedicate their motor fuel excise taxes to highway construction. The results, reported in Table 4, generally support our hypothesis. Almost four-fifths of the states where petroleum refining accounts for a substantial share of state personal income dedicate their motor fuel taxes to highway construction, compared to only one-half of the states where the overall size of the petroleum refining industry relative to the overall state economy is small. A similar relationship holds with respect to the crude production industry, but there the disparity is much narrower (60 versus 54 per cent), which is what our hypothesis suggested.

To test the impact of firm size and relative industry size on the *level* of state motor fuel excise tax rates, a somewhat more complex analysis was necessary. Like other state revenues, gasoline taxes too may respond more to the availability of income or the general effective demand for public services in a state than to oil industry lobbying. In fact, several political scientists have demonstrated a substantial relationship between state income levels and state governmental expenditures.[47] Since there was reason to expect that the *relative* size of the petroleum industry in a state would vary with the size of the state's economy, it was necessary to introduce controls to verify that our economic structure variables are not really proxies for these overall income variables. In particular, we utilized two control variables: first, the ratio of total state governmental revenues to total state personal income (which measures the state's preference for public vs. private consumption and thus the effective demand for revenues); and second, the state's per capita income (which measures the supply of wealth available). Multiple regression techniques were then applied to determine the impact of these economic structure and control

---

[46] The index was computed for crude petroleum using 100 employees as the cutoff size and for petroleum refining using 250 employees as the cutoff size. These sizes were chosen on the basis of data availability and to provide sufficient variation in the explanatory variables constructed from them. 1967 *Census of Manufactures* data were used.

[47] See footnote 7 above.

Table 4. Distribution of States Cross-Classified by Motor Fuel Excise Tax Dedication and Size of Industry in State

	Dedicate Motor Fuel Taxes to Highway Construction		Do Not Dedicate Motor Fuel Taxes to Highway Construction	
Size of Petroleum Industry	Number of States	Per Cent of Row	Number of States	Per Cent of Row
Petroleum refining				
Large[a]	7	(78%)	2	(22%)
Small[b]	21	(51%)	20	(49%)
Crude oil production				
Large[a]	9	(60%)	6	(40%)
Small[b]	19	(54%)	16	(46%)

[a]Greater than one per cent of state personal income.

[b]Less than or equal to one per cent of state personal income.

variables on the level of state excise tax rates for all states, and for dedication and nondedication states separately.

The results of these tests, recorded in Table 5, substantially confirm our hypotheses about the relationship between industry economic structure and the successful exercise of political influence. For all fifty states, the structure of the petroleum refining industry is significantly related to the level of motor fuel excise tax rates in precisely the direction hypothesized: tax rates are lower where the petroleum refining industry is large and where it is dominated by large firms. By contrast, no significant relationships were found between tax rates and crude oil industry structure, which is consistent with our argument that crude oil producers are less interested in motor fuel tax rates than are refiners.[48] By the same token, the controls for state tax effort and per capita income showed no significant relationship to motor fuel tax rates, suggesting that the economic *structure* of

Table 5. Relationship Between State Motor Fuel Excise Tax Rates and Petroleum Industry Economic Structure

		Refining Industry		Crude Oil Industry		Controls		R^2 (Proportion of Variance Explained)
Regression Model for:	Constant	Overall Size[a]	Large Firm Dominance[b]	Overall Size[a]	Large Firm Dominance[c]	State Tax Effort[d]	State Per Capita Income	
All 50 States	8.537	–0.423*	–1.295*	+0.046	+1.936	–0.065	–0.010	.291
		(–2.78)	(–2.54)	(2.02)	(0.41)	(–1.54)	(–0.03)	
22 Nondedication States	8.290	–0.414	–1.963*	+0.070	2.816	–0.093	0.251	.454
		(–0.88)	(–2.77)	(1.15)	(0.08)	(–1.75)	(0.65)	
28 Dedication States	7.543	–0.370	–.518	0.024	3.024*	0.044	–0.183	.283
		(–0.43)	(–0.93)	(0.17)	(2.31)	(0.31)	(–0.73)	

(Figures shown are partial regression coefficients [B] with *t*-ratios in parentheses.)

*Statistically significant at the .05 level.

[a]Value added by refining or crude industry in state as a percentage of state personal income.

[b]Number of refineries with greater than 250 employees as a percentage of total number of refineries in state.

[c]Number of crude oil and natural gas establishments with greater than 100 employees as a percentage of total number of crude and natural gas establishments in state.

[d]Total state government revenues as a percentage of state personal income.

---

[48] Several alternative explanations for this lack of relationship might also apply. In the first place, the result might reflect relatively less success at influencing policy on motor fuel tax rates because of lower concentration and the associated higher organizational costs of lobbying even though crude producers have a similar desire for lower rates. Furthermore, the data used to measure the share of activity of this sector in each state also included natural gas production. Although natural gas production and crude production are highly correlated across states, the additional variation produced by the inclusion of natural gas production (which does not go toward motor fuels) may be the source of the absence of an effect of this variable on the gasoline excise tax rate.

a state may be more important in explaining significant policy differences than aggregate levels of state economic activity. Altogether, our model accounted for close to 30 per cent of the variation in state motor fuel tax rates among the fifty states, most of it as a result of the impact of the refining industry structure variables.

A comparison of the results for the twenty-two nondedication states to those for the twenty-eight dedication states lends further support to these conclusions. As expected, our economic structure/political influence model is far more successful in explaining tax rate variations in the nondedication states, where the industry lobbied against tax rate increases, than in the dedication states, where it did not. Particularly noteworthy is the operation of our refining industry firm size variable, which is significantly negatively related to the tax rate in the twenty-two nondedication states but not in the dedication states. Evidently, where the large refiners flex their political muscles (the twenty-two nondedication states), they have considerable success. Here is important additional support, therefore, for the firm size/political power hypothesis outlined at the outset of this article.

This finding is even more noteworthy in view of the absence of any statistically significant relationship between motor fuel tax rates and the state per-capita income variable that other researchers have identified as an important determinant of state policy. But these researchers did not control for the economic structure variables we have argued are most relevant politically. Interestingly, the per-capita income variable is not even consistently related to the tax rate in the expected direction. The same is true, moreover, of the state tax effort variable, which, far from having a significant impact on motor fuel excise tax rates, is also not even related to the tax rate in the expected direction in two out of the three tests.

In summary, it appears that states with substantial oil refining industries are more likely to dedicate motor fuel tax revenues to highway construction. Similarly, in those states where no such dedication occurs and where the petroleum industry has consequently made political efforts to keep tax rates low, industry success has been a function of the size of the firms in the industry in the state: the larger the firms, the greater the success in keeping motor fuel tax rates low. The results of this test of the relationship between economic structure and political influence in the area of state tax policy thus lend support to the results of our earlier test in the area of federal income tax policy, especially with regard to the political influence flowing from large firm size.

Summary and Conclusions

Despite its tentative and partial character, the evidence presented in this study lends empirical credence to the notion that the structure of the American corporate economy has important implications for the operations of the American political system at both state and federal levels. Especially striking are the positive relationships discovered between firm size and industry success in avoiding both federal corporate income taxes and state excise taxes. To the extent that political power is reflected in such actual policy outcomes, we can therefore say that larger firm size does indeed seem to yield greater political power. Also interesting are the findings suggesting that larger industries (as opposed to larger firms) are less successful politically, which supports the "free rider" hypothesis advanced by Mancur Olson, since larger industries in general have more firms than smaller industries. In addition, we found evidence that industries which are most visible and most fearful of government intervention because of their attractive (i.e., concentrated) market structure or profitability are more inclined to avoid (or are less successul at mounting) political influence efforts aimed at reducing their tax burdens.

Beyond these immediate substantive conclusions, however, the work summarized here also has broader methodological and theoretical implications because of what it suggests about the feasibility of a comprehensive empirical approach to analyzing the relationship between economic structure and political power, a relationship that is vital to the future of American democracy. For too long now, political scientists have contented themselves with imperfect case study approaches to this question, ignoring the insights available in the economics literature that make it possible to identify measurable aspects of economic structure and thus to undertake more systematic testing of hypotheses.[49] While we are fully aware of the special strengths of the case study approach and the limitations of our own, we believe that the relationship between economic and political power will not be fully understood until the findings of particular case studies can be compared with the results of more comprehensive and systematic empirical research of the sort reported here. Given the importance that an understanding of this relationship holds for the structure of the American political system, and the implications it has for the shaping of public policies, it is our hope that the analysis offered here will help open the way for the progress on the systematic, empirical side of this research strategy that is so clearly needed.

---

[49] Two recent exceptions to this point, focusing on the trade policy area are: C. P. MacPherson, "Tariff Structures and Political Exchange," Ph.D. Dissertation, Department of Economics, University of Chicago (December, 1972); and J. J. Pincus, "Pressure Groups and the Pattern of Tariffs," *Journal of Political Economy*, 83 (August, 1975), 757–778.

Appendix Table 1.
1963 Tax Avoidance Rates for Industries with Rates More than One Standard Deviation from the All-Industry Mean
(Mean = 12.6, Standard Deviation = 6.82)

IRS "Minor Industry" Code	Industry Description	Tax Rate Avoidance (Percentage)
1498	Miscellaneous Nonmetallic Minerals	35.8
1100	Coal Mining	33.8
2912	Petroleum Refining with Extraction	32.2
3330	Nonferrous Primary Metals	30.5
3662	Electronic Components	28.2
1310	Crude Petroleum Natural Gas and Liquids	27.9
1410	Stone and Gravel	27.6
2870	Agricultural Chemicals	27.2
2911	Petroleum Refining without Extraction	27.0
2091	Vegetable and Animal Oils	25.5
3098	Miscellaneous Plastic Products	23.7
2410	Logging, Lumber, and Basic Wood Products	21.0
1020	Copper, Lead, Zinc, Gold, Silver Ores	20.9
2010	Meat Products	20.2
3198	Leather Tanning and Finishing	19.5
2070	Confectionary Products	5.8
3530	Construction, Mining, Material Handling Machinery	5.6
3611	Electrical Transmission and Distribution Equipment	5.6
3260	Pottery	5.4
3698	Electrical Machinery, N.E.C.	5.4
2212	Broad Woven Fabric Mills, Man-Made Fibers	5.0
3560	General Industry Machinery	4.9
2640	Converted Paper and Paperboard Products	4.6
2100	Tobacco Manufactures	4.1
2611	Pulp Mills	4.1
2711	Newspapers	3.9

Appendix Table 2.
Mean and Standard Deviation of Independent Variables and Correlation with Tax Avoidance Rate

Independent Variables	Mean	Standard Deviation	Simple Correlation with TAR
Firm Size (F)	$166 million	$368 million	.060
Industry Size (I)	223,500 employees	225,900 employees	–.050
Market Concentration (C)	39.62%	14.34%	–.176
Profit Rate (R)	11.15%	5.46%	–.189
Geographical Disperson (D)	.1401	.0742	.007

COMMUNICATIONS

Standing to Sue Updated

To the Editor:

I think it worthwhile to bring my article "Standing to Sue: Interest Group Conflict in the Federal Courts" (*APSR*, September, 1976) up to date, especially in light of two recent cases decided by the Supreme Court, *Warth v. Seldin* and *Simon* v. *Eastern Kentucky Welfare Rights Organization.*[1]

In so far as the essay pertained to conflict in the judicial forum between private groups, and in particular to cases in which groups seek to forestall actions by corporate business, the argument remains intact. Courts have continued to enlarge on the substantive standing test in force from *Data Processing* to enable environmentalists, consumers, and others to press their suits against a wide range of projects. Since *Warth,* Courts have recognized some new varieties of injuries-in-fact (e.g., "the risk that serious environmental impacts will be overlooked"); and they have further extended requisite zones of interest (e.g., socioeconomic impacts such as increased problems of fire and police protection and higher tax rates are protected by the National Environmental Policy Act). Organizations continue their post-*Sierra Club* representations: remarks one judge, after members of an unincorporated group without by-laws are deposed by defendants and shown to disagree about the purposes of the suit: "I am troubled by the lack of unity and substance of this organization. However, some members of Concerned Citizens live, work, and spend leisure time in Umtillo County. They meet the *Warth* standing test."[2]

In *Warth* v. *Seldin,* a divided Court denied an array of organizational plaintiffs standing to challenge local zoning ordinances that the Court acknowledges had the effect of excluding low-income and minority residents. The opinion is complex, shifting from one set of criteria and facts to eliminate one plaintiff, to another set to eliminate the next; it concerns Article III standing rather than standing under the Administrative Procedures Act; and zoning is a peculiarly local matter. Nevertheless, the opinion does indicate an insistence on an injury which (1) is demonstrably an effect of the alleged infraction of law, and (2) may be afforded relief by the Court's action. The economics of the housing market, and not merely the zoning policy, is said to be responsible for plaintiffs' problems.

These requirements for injury-in-fact are reiterated in *Simon* v. *Eastern Kentucky Welfare Rights Organization,* where the Court dismissed as "speculative at best" the claim that indigents denied care by nonprofit hospitals have been harmed by an IRS ruling removing tax incentives to provide such care, and therefore have no standing under the *Administrative Procedures Act.* The Court in its opinion explicitly upholds S.C.R.A.P., claiming the plaintiffs there would have met the tests of causation and efficacy-of-relief. Resisting the temptation to agree with Justice Brennan's dissent in *Warth,* that the decisions of these cases can be explained only by a hostility to the claim on the merits, I might venture a distinction which squares with these decisions and leaves private defendants still vulnerable to challenges through the previously liberalized law of standing. For in these recent cases the Court rejects the argument of some social might-have-been in the absence of official behavior: minority residents would still have been excluded without the zoning; hospitals might not have given indigents care without the tax ruling. But anticorporation groups usually seek to stop or modify some specific, concrete project. Thus, the lower courts, based on *Warth,* denied standing for minority and other groups who claimed to be injured by H.U.D.'s granting funds for sewage and recreational improvements without due regard to the recipient town's exclusionary zoning ordinances; and, based on *Simon,* found standing for an organization of farm workers and farmers seeking to force corporate landlords to sell off certain excess lands, in accordance with reclamation laws.[3] There is no question, however, that the majority on the Court is groping toward a more restrictive role for the judiciary in social conflict, and until their approach moves beyond the invocations of prudence in *Warth* and is anchored in clearer doctrine, any such prognosis must be, as Justice Powell might say, speculative at best.

---

[1] 422 US 490 (1975); 426 US 26 (1976).

[2] City of Davis *v.* Coleman, 5 ERL 20635 (1975); McDowell *v.* Schlesinger, 6 ERL 20225 (1976); Port of Astoria *v.* Hodel, 5 ERL 20658 (1975). These cases are picked from literally scores of recent examples.

[3] NAACP *v.* FPC, 425 US 662 (1976).

One final point. Federal judges generally will, without raising questions of standing, allow parties who have participated in agency proceedings to challenge administrative rulings in Court. For example, the Supreme Court recently reviewed a ruling by the FPC that the Commission was not required by its legislative mandate ("in the public interest") to bring about equal employment in utility companies, contrary to the petition of the NAACP.[4] While upholding the ruling, a "loss" for the NAACP, the Court went on to say that the FPC must make certain that consumers are not required to absorb any costs of strikes, boycotts, loss of contracts, or other effects of compliance or noncompliance with the antidiscrimination statutes – thereby putting a new subject matter on the regulatory agenda and paving the way for future challenges to utility companies. No issue of standing was raised. In the last couple of years the agencies themselves, in part because of prodding by the judiciary, have become relatively relaxed about what parties and organizations they will hear; indeed some are already in the business of financing witnesses to come before them. It is possible that even though the law of standing is tightened, a variety of groups, some with broad-gauge social claims like the NAACP, will reach the courtroom by the administrative route. Moreover, since this Court seems to want to restrain itself from overturning judgments made in the other branches, it would appear also bound to defer to the agencies' determination of who is a proper party at interest. While in such circumstances judges' discretion will in this limited sense have been trimmed, all of the dilemmas of interest group litigation discussed in "Standing to Sue" persist.

KAREN ORREN

University of California, Los Angeles

---

[4] Evans *v.* Lynn 537 F.2d 571 (1975); National Land For People, Inc. *v.* Bureau of Reclamation, 417 F. Supp. 449 (1976).

John Locke: Problems and Perspectives

TO THE EDITOR:

In his fine review of *John Locke: Problems and Perspectives* (*APSR*, September 1975), Robert H. Horwitz says the following of Hans Aarsleff's contribution to this collection of Locke studies: "But what is the status of Eternal Salvation [which is required for divine justice to be effectual] in Locke's teaching? Aarsleff's inspiring interpretation depends decisively on whether Locke has demonstrated the immortality of the soul. Unfortunately, according to Locke, its demonstration is beyond the capacity of human reason." (The classic argument in this vein is to be found in Leo Strauss's *Natural Right and History*, pp. 203 ff.: "[Locke] says, on the one hand, that, in order to be a law, the law of nature must not only have been given by God, but it must in addition have as its sanctions divine 'rewards and punishments,' of infinite weight and duration, in another life. On the other hand, however, he says that reason cannot demonstrate that there is another life. Only through revelation do we know of the sanctions for the law of nature or of 'the only true touchstone of moral rectitude.' ") Horwitz, agreeing with if not necessarily following Strauss, goes on to suggest that "Aarsleff appears to help Locke overcome the critical question of the immortality of the soul through nothing less than an act of faith: 'Absolute proof of the nonexistence of a future state is not to be had, consequently the knowledge of the mere probability of a future state with rewards and punishments is that which must direct our search for happiness.' "

Now Aarsleff has not "helped" Locke in any way, for it is Locke *himself* who argues (in the *Essay Concerning Human Understanding*) that "the bare possibility, which nobody can make any doubt of," of another life governed by divine rewards and punishments, makes it a good bargain to conform one's voluntary actions to the divine law: "If the good man be in the right, he is eternally happy; if he mistakes, he is not miserable, he feels nothing." Whatever one may think of this argument – it has the look of debased Pascalianism – it is Locke's *own,* and not one provided by Aarsleff with a view to "helping" Locke to overcome the incoherence of his theory through an "act of faith." (Ironically enough, one of the clearest things in Aarsleff's interpretation of Locke is his careful distinction between "demonstration" and mere "probability"; since, however, this cannot be treated at length here, I will evade that responsibility by pointing out that I have already treated Aarsleff and Strauss on Lockean "demonstration" and "probability" in my article, "On Finding an Equilibrium between Consent and Natural Law in Locke's Political Philosophy" [*Political Studies* (Vol. 23, No. 4, 432–452), Oxford (December, 1974)] – an article which at least indicates the passages in Locke, Aarsleff and Strauss which should be *consulted*, whatever one might think about the proper interpretation of those passages.)

PATRICK RILEY

University of Wisconsin-Madison

TO THE EDITOR:

A brief response is called for – even at this late date – lest I appear ungrateful for Professor Riley's thoughtful critique of my review and for his generosity in drawing to our attention his interesting Locke article in *Political Studies*. Through his suggestion that I have wrongly accused Aarsleff of trying to "help" Locke, Riley has at least continued to focus our attention on such basic and difficult issues as Locke's position on natural law and the relationship, if any, in his thought between natural law and divine law. Riley stresses in his criticism that "it is Locke *himself*" who provides Aarsleff with the basic elements of his interpretation.

Now, Riley and I have no argument regarding the Lockean sources of Aarsleff's interpretation. Indeed, I went to considerable pains in my review to emphasize that "Locke's writings are replete with quotations from scripture and his professions of Christian piety can hardly fail to capture the attention of even the most superficial readers" (p. 1016). Unfortunately, as Riley observes repeatedly in his article, Locke qualifies and even at times flatly contradicts many of these quotations in other parts of his work. This leaves us with the difficult task of discovering his ultimate position on these questions and whether the elements in his position are consistent. Riley himself makes clear in his discussion of the secondary literature how few are the interpreters of Locke who have demonstrated the tenacity and other qualities of mind and heart required to see these tasks through to completion. It is easier by far to "help" Locke achieve a seeming consistency by accepting as conclusive certain limited elements of his thought while overlooking, failing to take seriously, or even repressing those aspects that don't fit. That, I had implicitly suggested in my review, is the crux of Aarsleff's problem.

Although Riley struggles manfully and openly to avoid this all too common problem, I daresay that he, too, succumbs to the temptation of trying to "help" Locke. This is suggested by the very title of his article, as well as by its substance, intent as he is "On Finding an Equilibrium Between Consent and Natural Law in Locke's Political Philosophy." It seems to me that it is Riley, not Locke, who provides the "equilibrium." To his credit, Riley identifies elements of Locke's thought that don't fit the "equilibrium" hypothesis. Nevertheless, he concludes rather condescendingly that Locke's "theory of natural (or rather divine) law . . . taken as a whole, is intelligible, even if one might not want to derive the validity of such law from divine will, and even if one might wish that he had not (occasionally) argued as if such a law could be derived from reason alone" (*Political Studies*, p. 452). "But at least," concludes Riley, "the defects of Locke's system can be remedied with his own concepts, and in a Lockean spirit." Whether it is Locke's work or Riley's analysis that stands in need of improvement, the interested reader must judge for himself.

ROBERT H. HORWITZ

Kenyon College

More on Response Instability

TO THE EDITOR:

The recent exchange between Professor Achen and his critics in the *American Political Science Review* [December, 1976] centered on the problem of whether to attribute the random variability of attitude responses reported by Converse in American panel data to respondents who were vague about their attitudes or to vague questions. I suggest a third model: both respondents and questions were clear but on different wavelengths so that respondents attempting to map clear but complex or ambivalent attitudes onto a clear but simple Likert scale were forced into an unstable translation process that appears to be highly random.

For example, suppose I believe in affirmative federal action to end racial desegregation. But suppose I also oppose coercive government action, especially where it produces strong vocal opposition and civil unrest. An SRC researcher hands me a questionnaire, a seven-point Likert scale asking about my attitude toward court-ordered school busing with options ranging from "agree strongly" to "disagree strongly." I submit that I might reasonably translate my clear attitudes onto this scale by selecting almost any of the responses. I might signal my agreement in principle and say "agree strongly." Or, expressing my ambivalence, I might signal my deep discomfort in practice by checking any option through "disagree moderately."

Or take foreign aid. I believe in helping needy people. But suppose I also dislike waste, corruption, using my dollars to line pockets of foreign elites, and making people dependent on handouts. Translating this attitude onto a simple seven-point scale about my attitude toward foreign aid might again be done by agreeing strongly or by checking any of the mixed, intermediate positions through "disagree moderately." Over time my true attitude may remain the same, but my recorded attitude could be highly variable as I faced anew the problem of translating a fairly complex set of relevant attitudes into one point on a scale.

In general a "translation difficulty" model of response instability agrees with Professor Achen in the conclusion that better questions probably would reveal American adults to have more stable orientations than Converse concluded. However, I am skeptical that *vagueness* is the relevant issue; both the attitudes in the examples above and Converse's questions seem quite clear to me. Measured attitude stability could be predicted to rise if researchers either use more complex attitude items that align themselves comfortably with naturally occurring ambivalences or perceived complexities among the general public or if researchers use questions that decompose derived attitudes toward policy questions into their constituent parts.

LLOYD S. ETHEREDGE

Massachusetts Institute of Technology

Alliance Behavior in Balance-of-Power Systems

TO THE EDITOR:

Patrick J. McGowan and Robert M. Rood in their article, "Alliance Behavior in Balance of Power Systems: Applying a Poisson Model to Nineteenth-Century Europe" (*APSR*, 69, 3, pp. 859–870) have inferred three specific hypotheses from Morton Kaplan's "theory" of the balance-of-power system, based on the assumption that alliance behavior in such a system is a stochastic process, specifically a Poisson process. The hypotheses are that in a balance-of-power international system: (1) the occurrence of alliances will be stochastically distributed; specifically, the number of alliances formed per unit of time is a Poisson random variable; (2) the time intervals between alliances are randomly distributed; specifically, the distribution of interalliance intervals is a negative exponential random variable; and (3) a decline in the systematic rate of alliance formation precedes system changing events, such as general war. Testing these hypotheses against data on the frequency of formal and informal alliances between 1815 and 1914, they conclude that Kaplan's theory "has greater credibility than heretofore" (p. 870).

There are, in the first place, certain problems regarding the applicability of the Poisson distribution to the data on alliance formations over time. The Poisson distribution is applicable where a large number of objects (events) is distributed over a large area (time span). The data used by McGowan and Rood, however, consist of 55 cases of alliance formation (formal and informal) over 20 five-year periods. The small number of events makes the apparent fit with the Poisson distribution questionable.

Moreover, the Poisson process requires that each event be independent of all others, i.e., for any two events A and B, $p(B|A) = p(B)$. Now, while it is commonly asserted of the European classical balance-of-power system that alliances were made irrespective of the ideological characteristics of alliance partners, that a particular alignment did not "predispose the same nations to align themselves with each other at the next opportunity" (p. 861), this is not quite the same thing as the independence of events as required in statistical theory. For a change in the composition of one coalition *following* a realignment in the opposing coalition is not really independent of the latter event. Think of a balance-of-power system consisting of six nations $a_1, \ldots, a_6$. Let the opposing coalitions S_1 and S_2 at time t_0 be

$$S_1 = (a_1, a_2),$$
$$S_2 = (a_3, a_4),$$

with a_5 and a_6 neutral actors. Now, suppose at t_1 coalition S_1 expands to (a_1, a_2, a_5). Then, if at t_2, coalition S_2 is reorganized to consist of (a_3, a_4, a_6), we cannot maintain that what happens at t_2 is independent of what happened at t_1. Not only that, even the time interval between the two events is dependent upon the fact that one is a *response* to the other. Instances of such readjustments of alliance structures, one in response to another, abound in the history of the classical balance-of-power system. The tenability of the assumption becomes particularly problematic when one is trying to locate clusters among as small a number as 55 cases of alliance formation over 20 time intervals in order to fit the data to a Poisson distribution.

This is revealed in the results of the test of the third hypothesis, which is the one of major substantive political interest. This hypothesis predicts that "system-changing events" such as a general war will be preceded by a decline in the rate of alliance formation. The prediction is borne out for the five-year period 1910–1914 preceding World War I in which no alliances were formed. However, McGowan and Rood's data reveal another period in which no alliances occur – the period 1820–1824 – following which no "system-changing events" happen: this the authors dismiss rather casually, and from a theoretical standpoint quite unsatisfactorily, as "the quiet period after the burst of diplomatic activity that concluded the Napoleonic Wars" (p. 869). Further extending the scope of the third hypothesis (although they concede that they do not want "to push it too far"), they also suggest "a serendipitous finding of some theoretical interest," viz., that in four out of five instances of change from one diplomatic period to another as described by Rosecrance [*Action and Reaction in World Politics* (Boston: Little, Brown, 1963), pp. 232–266], there was a decline in the rate of alliance formation (pp. 869–870). However, there are three other instances of a similar decline in the alliance formation rate (the periods 1830–1834, 1850–1854 and 1855–1859) in which no such change "in the structure of the system" occurs. The relationship between "system-changing events" and a declining rate of alliance formation, therefore, is not, it would appear, as firmly established as McGowan and Rood maintain.

Besides these problems of a statistical nature relating to the empirical testing of the hypotheses, there are certain more fundamental problems involving the theoretical status of the hypotheses themselves. Since McGowan and Rood's hypotheses are derived from Kaplan's "essential rules" for the balance-of-power system [*System and Process in International Politics* (New York: John Wiley, 1957], it would not be out of place here to discuss these briefly. Of the six rules, Rules (1) and (2) are the rationality assumptions for the behavior of individual actors. Rule (4) is the method of operation of the system, the means by which actors maintain the essential features of the system within a dynamic process. Rules (3), (5) and (6) describe the state of equilibrium in the system: they prohibit a change in the number of actors in the system, and that is what the equilibrium is supposed to preserve. Now, Kaplan combines within this set of rules for the model the two analytical levels of description and explanation, for he assumes in Rules (3), (5) and (6) the existence of an equilibrium which ought to be deducible from the behavior of actors as specified in Rules (1), (2) and (4). This point has been made by Arthur Lee Burns [*Of Powers and their Politics* (Prentice-Hall, 1968), pp. 113–116]. In other words, instead of analyzing the dynamics of the balance-of-power system in terms of the behavioral assumptions about the actors (Rules (1) and (2)) and the adjustment mechanism of the system (Rule 4) and proving the existence of a stable equilibrium in the system, Kaplan merely states that the actors will behave so as to maintain the equilibrium (Rules (3), (5) and (6)). Thus, we do not have a model in which stability exists as a *necessary logical consequence* of the primary assumptions regarding the behavior of the actors and the dynamic adjustment process of the system; we have stability simply because we *assume* that actors act so as to maintain the equilibrium.

The result is that there is no way of determining whether a deduction made from this model is a necessary (essential) property of a balance of power system. Take Hypothesis 3 deduced by McGowan and Rood. This purports to relate the transformation of the system to a change in its activity rate. Since we do not know, however, the essential conditions which guarantee stability in the system, we have no way of knowing whether this is either a necessary or a sufficient condition for the transformation of a balance-of-power system. Kaplan himself, for instance, lists an elaborate, though by no means exhaustive, catalogue of the conditions which might cause a balance-of-power system to be transformed into some other arrangement. These conditions are as diverse as the existence of actors who refuse to play according to the rules, capability changes inducing positive feedback, incompatibility among the essential rules of the system, existence of actors who prefer supranational organizations, failures of informational outputs, lack of flexibility of the adjustment mechanism (Kaplan, p. 27). True, all of these factors may contribute to the instability of a balance-of-power system, and one could probably list several others as well. The point, however, is that this enumeration of the various possible factors which might transform the system remains entirely at the descriptive-empirical level; such an enumeration does not give us an explanatory theory for the system or its transformation. (It is interesting to note that McGowan and Rood start off by referring to Kaplan's contribution as "theory" within quotes, but later on quietly drop the qualifying marks.) That is to say, even if we confirm the hypothesis that there was a freezing of alliance structures before the outbreak of World War I, we would still not necessarily have a tested theoretical answer to the question "Why did a general European war break out in 1914?" (McGowan and Rood, p. 889). The prerequisite for a satisfactory answer to this question is a theory of the classical balance-of-power system specifying the necessary conditions for stability of the system in terms of its structure, the behavior of the actors, the adjustment mechanism and the resources of the system, all in the *historical* context of the existence and collapse of the classical balance-of-power system.

PARTHA CHATTERJEE

Centre for Studies in Social Sciences,
Calcutta

TO THE EDITOR:

Because "the classical literature on the balance of power consists of vague, often ambiguous, descriptions of the way the system is supposed to operate,"[1] contemporary studies of balance of power processes are open to at least two types of criticism: (1) the propositions investigated are drawn from a theoretically inadequate model of the balance of power, and (2) the techniques used to evaluate the empirical fit of the propositions are inappropriate to the task at hand. This is the essence of Dr. Chatterjee's communication regarding our article. More specifically, we are charged with making three errors of a statistical nature,

(1) our application of the Poisson is incorrect because of our small *N;*

(2) our use of the Poisson is incorrect because we confuse statistical independence with causal independence; and

(3) we have misread the results of the test of our third hypothesis, leading to an ambiguous conclusion.

Finally, a "more fundamental" theoretical problem of our research, Dr. Chatterjee argues, is that

(4) our hypotheses, even if they are consistent with the data, do not tell us anything of theoretical interest because Kaplan's model fails to deduce the necessary conditions for a stable balance-of-power system.

In response to the first criticism, Dr. Chatterjee clearly misreads our article, and he apparently misunderstands the Poisson model. Our test of hypothesis 1 is *not* based on an *N* of 20 five-year periods as he says, but rather on an *N* of 101 years. This should be clear to any reader from the discussion on p. 867 and from Table 1 on p. 868. Dr. Chatterjee's confusion on this point probably stems from the fact that in testing hypothesis 3 we reaggregated our data on the basis of five-year intervals and *retested* for the presence of the Poisson. Moreover, this demonstration that the goodness of fit of the Poisson is not affected by how time is aggregated strengthens our findings regarding our first hypothesis.

Furthermore, the applicability of the Poisson model is not limited by the size of *N* as Dr. Chatterjee states. In any application in which the intervals are of equal length, say days or years, the sole parameter governing the generation of the Poisson distribution is *m*, the mean. Our estimated *m* produced by dividing 55 alliances by 101 years is .54, very similar to the *m* estimated by Richardson in his path-breaking studies of deadly quarrels when he divided 59 wars by 110 years.[2] Moreover, the Poisson is an exact distribution, that is, for any given value of the mean there is a distinct Poisson distribution. And since the mean has the well-known statistical property of being an unbiased estimate of *m* regardless of sample size, the size of *N* does not affect the applicability of the Poisson. Its effect is on good-of-fit tests such as Chi-square, rather than on the Poisson itself. With our *N* of 101 this is not a problem.

In his second point, Dr. Chatterjee suggests that we have misapplied the Poisson because of his contention that balance-of-power alliances are time-dependent processes. However, his criticism is based on assertion rather than on the presentation of empirical evidence that this is the case. By devising a hypothetical case in which two alliances lack independence by definition, he in effect sets up a straw man which he can then attack. Of course, our demonstration that the pattern of alliance formation over time fits the Poisson *does not prove* that alliance formations are, in fact, independent. But, our findings do render the assumption of independence *more plausible.*[3]

---

[1] Partha Chatterjee, "The Classical Balance of Power Theory," *Journal of Peace Research*, 9 (1972), 51. 51.

[2] Lewis F. Richardson, *Statistics of Deadly Quarrels* (Pittsburgh: Boxwood Press, 1960), p. 129.

[3] On this point see Dina A. Zinnes, *Contemporary Research in International Relations* (New York: Free Press, 1976), pp. 252–253.

Our findings upon testing Hypothesis 3 have been misrepresented. The period from 1820 to 1824 was quiescent only in that no alliances were formed. It was a period identified by Denton (discussed by us on p. 869) as one in which war intensity was high. While we would agree that we have not provided rigorous statistical evidence to support the "serendipitous" findings in our article, Dr. Chatterjee chooses to ignore the citations in footnote 83 on page 870. In other research reported in a paper cited in that footnote, we provide statistical evidence rendering our serendipitous findings more plausible and we cite two studies by Singer and Small which reinforce our findings.

Dr. Chatterjee's fourth point is obviously more a criticism of Kaplan than of our article. We think that he has misunderstood Arthur Lee Burns, whose work he cites. Burns suggests that Kaplan's rules, especially Rules 1, 2, and 4 "... retain their heuristic capacity to suggest explanations of the process which, together, they help to describe."[4] Moreover, Burns suggests that the appropriate way to evaluate Kaplan is to develop and test hypotheses about the general effects of these rules which in turn would yield a set of quantitative indicators of the process (e.g., the alliance formation rate).[5] This is exactly what we did in our study.

Dr. Chatterjee ends his communication with an eloquent plea for an axiomatic theory of the classical balance-of-power system that will permit deductions concerning the necessary conditions for system stability and that at the same time achieves historical specificity. How can we disagree with yet another call for deductive theory in international relations? Several comments, however, are in order.

An axiomatic theory "in the *historical* context of the existence and collapse of the classical balance of power system" may be a contradiction in terms, and certainly very difficult to achieve. Where are social scientists with an empirical orientation to find such a theory? Professor Chatterjee's own attempt to provide the elements of such a theoretical model is strong on logical rigor but weak in historical context.[6] Kaplan's "theory," which in our article we put within quotes specifically because of its logical weaknesses, still represents the best known attempt to formalize the balance of power and, we think, it possesses considerable heuristic power as suggested by Burns.

In our view international relations research is based on deduction *and* induction. Our empirical generalization that in the case of nineteenth-century Europe, decreases in the alliance formation rate were associated with war, including World War I, should be of interest to proponents of axiomatic theory construction since they cannot include all possible system stability conditions in their theories and achieve at the same time their valued goals of parsimony and elegance. Axiomatic theory building must start somewhere, and what we know about the operation of balance-of-power systems as shown by previous historical and statistical research certainly must be accepted as a fundamental point of departure. Likewise, when the deductive theory of balance of power called for by Dr. Chatterjee is produced by him or another theorist, we shall welcome its appearance and rush to test its predictions. That we did not produce such an axiomatic theory in our article is certainly true, but not, we think, relevant to what we did do.

Thus, while we thank Dr. Chatterjee for his interest in our work we cannot accept his criticisms for the reasons we have given.

PATRICK J. MCGOWAN

University of Southern California

ROBERT M. ROOD

University of South Carolina

Primates and Political Authority

TO THE EDITOR:

I turned to Professor Willhoite's "Primates and Political Authority" (*APSR*, 70 [December, 1976], 1110–1126) with some excitement, expecting to find in it a new dimension of the theme of "the primacy of politics." The essay does indeed pull together some fascinating information about the social lives of primates. Upon reflection, however, it is not clear that the essay has any implications whatever in regard to political authority. The central thesis of the essay is that there are "... good reasons to believe that man's biological nature incorporates strong propensities to establish and sustain dominance-deference hierarchies within his social groupings; that is, the stratification of political authority, power and influence may be *by nature* intrinsic to human social existence" (p. 1110). This is followed in turn by the suggestion that liberalism and radicalism may be in need of critical reconsideration and perhaps drastic revision.

---

[4] Arthur Lee Burns, *Of Powers and their Politics* (Englewood Cliffs: Prentice-Hall, 1968), p. 115.

[5] *Ibid.*

[6] Chatterjee, "The Classical Balance of Power Theory," *op. cit.* This historical context may be present in his recent book, *Arms, Alliances and Stability* (Delhi: Macmillan, 1975), but we have not yet been able to secure a copy.

Willhoite's thesis is vulnerable to two serious criticisms. In the first place, it is a mistake to equate political authority with power, stratification, dominance-deference relationships, and so on. Power and stratification *may* truly appear in all human groupings. That is an empirical question. But *nothing* in man's nature can entail political authority or reduce it to the level of a natural or "intrinsic" phenomenon. For political authority, unlike power and other such concepts, is a relationship that includes reciprocal *moral* commitments, as well as power. The search for the conditions in which mere dominance may be made legitimate – which is to say, transformed into authority – is, as Rousseau suggested at the beginning of the *Social Contract,* the major task of political theory.

Furthermore, it must be insisted that the question of the meaning of "authority" is not to be dismissed as "merely semantical." The English language, though often vague, is not an amorphous pulp.

In spite of the attempts of some political scientists to operationalize "authority" by equating it with power or dominance, it continues to *mean* legitimate power, or legitimate right of decision or command (according to the context) both to laymen and to political scientists. A pair of hypothetical examples will suffice to illustrate this: (1) Were the Attorney General to advise the president that "you do not have the authority to dismiss the Chairman of the Federal Reserve Board on the sole basis of policy differences," no one would interpret this to mean that the president lacked the physical *means* to effect the dismissal. On the other hand, the phrase, "you don't have the *power* to dismiss him" is ambiguous. It could imply either the lack of the legal right or the lack of the practical capacity to dismiss. (2) Were the director of the F.B.I. to inform the president that "it is not within our power to prevent all terrorist acts at domestic air terminals," no one would suppose that he would be disclaiming the legal right to prevent such acts. But if he did want to claim a lack of jurisdiction, then he would have to say, if he wished to be understood, that "it is not within our *authority*. . . ." Authority, clearly, is not at all the same thing as power, stratification, and dominance, and nothing but confusion can result from ignoring or glossing over the differences.

The second difficulty appears to be a misuse of the claim that the supposed "propensity" of humans ("to establish dominance-deference hierarchies," etc.) is *natural.* What can such a claim mean? From the behavioral perspective, *everything* that man does is natural. "It is the fundamental premise of evolutionary biology," Professor Willhoite tells us, "that man's social behavior is totally a part of the 'natural world.' " Well, since used car salesmen, for example, have lately shown a propensity to wear white shoes and belts, this must, then, be natural. As must the propensity of steelworkers to vote Democratic. Is Willhoite's "propensity" of the same order as these? If it is, then he has told us nothing new – nothing that we need to observe primates in order to discover. Men do indeed establish and sustain structures of dominance and deference, and it must be natural. But others, even if only a small minority, aspire to live on a footing of equality and reject even egalitarian forms of authority. Is that therefore natural also?

Clearly, Willhoite wants to claim that man's propensity toward dominance-deference relationships is natural in some *stronger* sense. But how can one thing be more natural than another? This is possible, logically, only if a nonnatural category is admitted. The obvious candidate is the artificial, that is, the man-made, as opposed to the natural. But Willhoite, as we have seen above, rejects this dualism. This would seem to leave the propensity of men to establish dominance-deference relationships on the same basis as the propensity to abolish them. It will be instructive to attempt to save this argument by introducing the category of the artificial anyway: man, we shall say, has a natural propensity to establish dominance-deference relationships. But the propensity doesn't dictate the concrete details of the relationships. Given man's symbolic capacity, he has an indefinite number of ways in which to fulfill this propensity. Here, human artifice steps in. That there will be dominance and stratification is naturally determined. But their exact form is variable and subject to historical influences. Some human arrangements will fulfill the propensity more completely than others and thus, presumably, be more stable. Some will fall so far short that they will be destroyed immediately. Will this modification of the argument do the trick? It will, as long as we continue to find men, without exception, living in dominance-deference relationships. But what if we were to find even one case of men living in full equality, and with nothing more than transitory and *ad hoc* relationships of dominance-deference? Then we would have to say that there is no propensity that precludes such arrangements – since they actually exist.

The upshot of this is that the validity of Willhoite's thesis depends upon evidence about what *humans* do – not upon evidence about what primates do. And that is what we knew (or should have known) in the first place.

PHILLIP C. CHAPMAN

University of Arizona

TO THE EDITOR:

Professor Chapman's first criticism is that I illegitimately equated political *authority* with power, stratification, and dominance-deference relationships. I'm sorry if I conveyed that impression to any reader; I certainly did not mean to use "authority" in any eccentric way but simply as power or hierarchical relationships considered to be legitimate or rightful. Through my discussion of "attention structure" and "charisma," I was suggesting that hierarchy-forming is phylogenetically rooted in the human species and long preceded the emergence of hominids' capacity to distinguish symbolically between legitimate and illegitimate power. I would further suggest that we often tend to accord authority, in the strict sense, to those who are in fact our hierarchical superiors. It is also possible, of course, to believe that one is obliged to defer only to those to whom one has initially conceded authority. That I have a strong normative preference for this latter ordering of priorities should be evident from the concluding paragraphs of my article. My principal concern, however, was to outline a phylogenetic perspective on human hierarchies, the behavioral substructure of all authority relationships.

Professor Chapman's second critical point made me painfully aware that I had used the terms "nature" and "natural" equivocally and had thus unintentionally contributed to misleading readers about my meaning. In my central thesis statement (p. 1110), I used "by nature" in the traditional political philosophic sense. But on p. 1124, I used "natural" in Robert Bigelow's evolutionary-biological sense (as typified in the quote by him on pp. 1125–1126). I wish now that I had said "phylogenetically" instead of "by nature" in my thesis statement, because, as the remainder of the article makes clear, that is precisely what I meant. From an evolutionary-biological perspective I would continue to reject a dichotomy between "natural" and "artificial" in human behavior. Rather, the significant question is the degree to which specific types of behavior are constrained or channeled phylogenetically. From this standpoint, then, used car salesmen's apparel choices are perfectly "natural" but very likely free of any significant degree of genetic control. I continue to believe that there are persuasive reasons for giving serious consideration to my basic hypothesis: the propensity to form hierarchies is not only, in the descriptive sense, "natural" to humans, but has also been strongly selected for in the phylogenesis of our primate species.

FRED H. WILLHOITE, JR.

Coe College

Politicians in Uniform

TO THE EDITOR:

The recent article by Robert Jackman ("Politicians in Uniform: Military Governments and Social Change in the Third World," *APSR*, 70 [December, 1976] 1078–1097) attempts to resolve the question of the relationship between military governance and social change. The core of Jackman's argument is a reconstruction of Nordlinger's "Soldiers in Mufti," (*APSR*, 64 [December, 1970] 1131–1148). Jackman identifies important limitations in Nordlinger's tests of various hypotheses concerning the effects of military rule and attempts to improve Nordlinger's work by using covariance analysis. We think, however, that Nordlinger made a number of errors which Jackman repeats, albeit in a slightly different form. More importantly, we believe that Jackman's article illustrates a major pitfall in the empirical literature on policy outputs, i.e., the danger of analyzing political economy problems without giving careful thought to their nonpolitical dimensions.

Jackman correctly identifies major errors in Nordlinger's article, including the misspecification inherent in zero-order correlations and the indeterminateness of the direction of causality which results when the dependent variable (social change) occurs before the independent variable (military involvement)! Jackman is right to correct these errors, but they do not exhaust the difficulties in Nordlinger's piece.

One of the primary problems of Nordlinger's analysis is the high probability of measurement error. Nordlinger measured the effects of military involvement on seven different indicators (most coming from Adelman and Morris's *Society, Politics and Economic Development*), including such items as "leadership commitment to economic development," "rate of improvement of human resources," "change in the effectiveness of the tax system," "change in the rate of gross investment," and "rate of growth of GNP per capita." "Control" variables included the size of the middle class and the degree of "modernization of its outlook." Whether such a concept as "leadership commitment to economic development" makes any sense we're not sure, but in a situation where lending agencies like the International Monetary Fund and the World Bank require a plan as a precondition for aid, it is hopeless to think that the existence of a plan indicates the extent of such a commitment. Moreover, even were we to accept to validity of this measure, it still has problems. As defined it does not appear to represent any kind of scale, and even if it did, it would have but three levels, thereby engendering problems of limited variation.

Similar problems arise with Nordlinger's second index of economic development. Can one talk about the modern outlook of social classes? Adelman and Morris's category of the most "modernized" middle class included such countries as Argentina, Brazil, Chile, Greece, Rhodesia, South Africa, Taiwan and Uruguay. Perhaps the middle class is just fickle; perhaps the concept makes little sense in the first place. Moreover, even a cursory reading of Nordlinger's index reveals that it measures much the same thing as the dependent variables of several of the equations. This may account for the more frequently significant t-coefficients and the higher R^2's in Table 1 and 2 than elsewhere in the article. By adopting Nordlinger's choice of variables, even while changing his specification, Jackman is thus vulnerable to major errors.

Let us now consider Jackman's own model. He specifies the problem to be explained as one of "social change" and utilizes four indicators of that concept: change in per capita energy use (a proxy for economic growth), change in the ratio of school enrollment to population, change in the number of doctors per capita, and change in the number of radios per capita. These four indicators are largely unrelated (see Table 5). As separate concepts, this would be fine, but since they are supposed to be measures of a single concept, their near-orthogonality raises major questions. How are we to know that the indicators are measuring the same thing? Perhaps there is no concept here at all. We agree with Professor Jackman that these are interesting variables in themselves, but we disagree with his apparent belief that he has got around the problem of multidimensionality by applying the same explanatory model to each variable. That this approach is unsuccessful is most evident in the results: R^2's that average .196 with one control variable and .211 with the other. In the presence of evidence of misspecification of this magnitude, nothing can be concluded about the slopes of the included variables. *In other words, the effect of the military's involvement in politics cannot be determined in these models.*

While Jackman may have corrected some of Nordlinger's problems, he thus did not correct all of them. Problems of measurement, contamination, and specification clearly remain. These problems are solvable in principle, but it is our conviction that Nordlinger's problems lay deeper than Jackman recognized and that this is his ultimate undoing. More importantly, it is at this point that the fundamental problems that bedevil this particular article can be seen to underlie much of the broader literature on policy outputs. Nordlinger's dependent variables include some that are directly controllable by the government (like the tax system), some that are partially controllable by the government (like the rate of gross investment), and some which the government influences much less, *even if it adopts "appropriate" policies.* The presence of these different kinds of dependent variables has important implications for this kind of work.

One consequence of recognizing these distinctions is that we should shy away from any analysis that uses the same set of variables and the same mathematical form to account for variations in different kinds of dependent variables. Not only is the government differently related to each kind of indicator, but some indicators may logically precede others and may, in fact, be parameters in a model determining others. Gross investment rates, for example, may depend on the tax system, while per capita GNP growth depends, at least in part, on the gross investment rate. Minimally one might conclude that identical models of these phenomena are inappropriate.

Suppose we want to examine the effect of government policy on agricultural productivity. We might consider such factors as the incentives to invest in new agricultural technologies, the domestic terms of trade between agriculture and industry, the international terms of trade and the relative price advantage thereby given to domestic agricultural producers, the structure of land law, and the efficiency of the transportation network. All these clearly have a considerable bearing on the rate of growth of agricultural productivity, and they might well be included in any regression model.

A very different model would be appropriate to explain change in school enrollment ratios. Such a model might include lagged measures of public expenditures on education, the previous level of enrollments (including the colonial heritage), population density and the number of languages spoken (which affect costs). Worth noting, however, is that governments seeking to speed social change might choose not to spend money to accelerate enrollments, either because they already have a school-leavers problem, or because it might be cheaper to educate students abroad, or because the private return to education is sufficiently high relative to the social return that subsidies are economically unproductive.

Modeling these kinds of outcomes seems relatively easy compared to modelling changes in per capita product, partly because the underlying structure appears to be more obvious, and partly because some of these indicators are themselves logically precedent to other kinds of change. (There is, of course, the complication that changes in school enrollment ratios, for example, may depend on economic growth, but in such a model economic resources can be taken as a parameter.)

Perhaps this argument sounds so involved that crunching ahead is the only alternative to giving up the whole affair. We think not, and it seems to us that our earlier distinction indeed offers a simplifying key to the problem. First, one should separate results of (1) direct government activities like changing the tax structure, allocating public funds, or repressing and torturing; from (2) partially controllable results like increasing school enrollments and raising gross investment; and from (3) ultimate outcomes like changing GNP, reducing the Gini coefficient, or increasing the population's caloric intake. Immediately we see that in order to affect ultimate outcomes, directly controllable activities must vary. So we can start with these direct and more easily modelable policies: public spending, taxation, repression. If they do not vary according to the degree of military involvement, the problem is resolved right there. If they do, then we must move to the next (less controllable) activity, always modeling with a plausible specification.

Such an approach raises the question of whether we can expect government policy to have much of an effect on ultimate economic outcomes. Put another way, is it credible that policy instruments represent real control variables in determining the behavior of the dependent variables? A negative answer to this question may, in fact, be plausible. The 1973 rise in the U.S. agricultural prices had more to do with Russia's decision to enter the world market than it did with U.S. agricultural policy, whatever our feelings about Butz's response to this fact. And much of the third world's development in the early 1950s resulted not from the domestic policies of their governments but rather from the rapid expansion of demand for primary products resulting from the Korean War. The importance of these arguments can be seen in Jackman's null finding. Methodological caveats aside, is this result to be taken as showing that military governments in fact do not differ from civilian regimes in their policy outcomes? Or does it simply mean that government policy in general does not have much influence over the phenomena of concern?

In sum, we wish to emphasize that the technical features of a complex analysis are significant and pose difficult problems, but the deeper problem is the danger of modeling complex political-economic relationships without giving primary attention to their distinctive structures.

BARRY AMES

Washington University, St. Louis
Visiting Scholar, Stanford University

ROBERT H. BATES

California Institute of Technology
Visiting Scholar, Stanford University

TO THE EDITOR:

Ames and Bates voice a series of muddled complaints with my paper. None of these complaints is relevant to my analysis, most are unoriginal, and some are quite silly.

The major objection appears to be that I did not give careful thought to the "nonpolitical dimensions" of "political economy problems." (I assume that this is intended to be *my* "principal undoing" rather than Nordlinger's.) This complaint is not without its logical and semantic difficulties: if *non*-political issues are the key to a problem, in what sense can that problem meaningfully be labeled one of *political* economy? More puzzling, however, is the very restrictive definition of "political" implied in their assertion (nowhere do Ames and Bates provide an explicit definition). Toward the end of their remarks, political is taken to refer to "controllable activities." Thus, "direct government activities" like allocating public funds are "political"; "partially controllable results [sic]" such as increasing school enrollment ratios are (presumably) semipolitical; while "ultimate outcomes" like altering the size distribution of income are "nonpolitical." This implied definition is too restrictive to be useful, and fits poorly with the thrust of most political economy. For example, it is naive to assert that the size distribution of income is nonpolitical, but Ames and Bates cannot be prevented from making that assertion for this reason alone.

Ames and Bates have had some difficulty representing what my analysis was all about.

Their remarks serve only to cloud the issues that I addressed. I concluded that government *structure* (in the form of the "simple civilian-military government distinction" [p. 1097]) has no effects on a variety of aspects of social change (when I wrote the paper in 1974, the dominant view[1] was that such effects were quite strong). My conclusion does not imply that government *policies* have no effect on these dependent variables, although it does imply that if such policy effects exist, then variations in government policies themselves are not dependent on the simple civilian-military government distinction. A large body of literature does indicate that governments can effectively pursue policies designed to stimulate the economy, to increase school enrollment ratios, and so on. My results simply suggest that the probability that a government will successfully pursue such policies is not dependent on whether or not it is a military junta.

Ames and Bates should familiarize themselves with the elementary analytic distinction between political structure and government policy. Unfortunately, they seem not to realize that this distinction makes their remarks irrelevant to the issues addressed in my paper. I did argue that "military governments . . . do not differ from civilian regimes in their policy outcomes." Whether or not "government policy in general does not have much influence over the phenomenon of concern" (the question that exercises Ames and Bates) is a wholly separate issue. Because answers to the second question do not depend on answers to the first, their remarks are quite beside the point, however much Ames and Bates may wish to speculate on this topic.

A second area of confusion in their remarks centers on the distinction between model building and hypothesis testing. My paper was concerned with *testing a series of hypotheses that link military regimes to social change.* In contrast, Ames and Bates would like to specify *causal models that explain change* (see, for example, their paragraph 9), which leads them to take my low R^2's as "evidence of misspecification." This misrepresents the purpose of my paper. Low R^2's might have provided *some* (but hardly the most interesting) evidence of poor specification had I attempted to specify a general model of social change. However, given (a) that this was *not* my goal, and (b) that I was testing the validity of a series of well-known arguments, the low R^2's are more usefully viewed as evidence *inconsistent* with those hypotheses and consistent with my original conclusion. Actually, other evidence (for example, the small *t*-ratios and the mixed pattern in the signs of the parameter estimates) are more important in this context. Rather than belabor the point, I invite Ames and Bates to consider more carefully the ambiguities inherent in using R^2 as a criterion for model specification (the less obvious of these ambiguities are discussed in most econometrics texts).

Ames's and Bates's confusion carries over into their claim that my model does not allow me to determine "the effects of the military's involvement in politics." This is nonsense. If my model is misspecified owing to an omitted-variables problem, and if this results in biased estimates (it need not), the typical result would be an *over*estimate of the size of the military-regime parameters.[2] I fail to see how this could lead me to revise my original conclusions. If my model is misspecified in the sense that I do not consider intervening variables (say, government policies), and if military regimes do affect social change indirectly, my estimates would still show military-government effects. This point is so elementary that I will not pursue it here.[3] Ames and Bates do not offer an alternative specification that they think better addresses the issues with which I was concerned. That they do not is, in my view, indicative of their confusion and uncertainty over what they are trying to say.

The remarks about the multidimensionality of "social change" are gratuitous. I argued (p. 1092) that social change is not a unidimensional concept, and further, that there are no good substantive reasons for treating it as such. In fact, there *are* strong grounds for treating it as multidimensional, as I noted. Ames and Bates apparently agree (they offer no arguments to the contrary). However, they seem to think that military regimes might have different effects on each of my four dependent variables (if they do not, their objection is not in the least bit relevant). Had they considered the issue seriously, they would have noticed that my analysis was *designed* to allow for this possibility. Had they stopped a little longer to digest my results, they might have also found that there is no evidence to support the argument that the effects of the civilian-military regime distinction on social change vary by *type* of social change.

---

[1] In fact, Huntington's argument and Nordlinger's results seem to be widely accepted. See, for example, S. E. Finer's analysis in the new edition of his well-known *The Man on Horseback: The Role of the Military in Politics* (Baltimore, Md.: Penguin Books, 1976), pp. 260–267.

[2] On this general subject, see for example Potluri Rao and Roger L. Miller, *Applied Econometrics* (Belmont, Calif.: Wadsworth, 1971), pp. 60–67.

[3] For a recent discussion, see Duane F. Alwin and Robert M. Hauser, "The Decomposition of Effects in Path Analysis," *American Sociological Review,* 40 (February, 1975), 37–47.

Finally, Ames and Bates assert that the "core" of my analysis is based on Nordlinger's data, and that I am "thus vulnerable to major errors." This is a strange criticism, because nowhere do I recall defending the validity of those data. I invite Ames and Bates to reread my paper, where I noted instead that Nordlinger's data "have serious limitations, which means that they cannot be used to generate an overwhelming challenge to Huntington's original hypothesis" (p. 1090). My reason for reanalyzing Nordlinger's data before proceeding to my own (which I consider the core of the analysis) was to show that differences between my conclusions and his are not an "artificial" function of data differences. Ames's and Bates's remarks on this subject are quite unintelligent.[4]

Where does all of this leave my analysis? Ames and Bates have failed to raise any issues that would lead me to modify any of my original conclusions (pp. 1096–1097). Instead, they have employed a naive definition of the term "political," and then muddled through a set of "issues" that (a) have no bearing on my paper, and (b) have dubious implications for anything. Had Ames and Bates spent a little more time formulating their argument, we might have been spared their platitudinous closing prescription. Even better, had they remembered that the efficacy of any prescription depends on the validity of the initial diagnosis, we might have been spared their remarks altogether.

ROBERT W. JACKMAN

Michigan State University

The Minimum Winning Coalition

TO THE EDITOR:

In his paper "Hollow Victory: The Minimum Winning Coalition" (*APSR*, 70 [December, 1976], 1202–1214), Mr. Russell Hardin offers an excellent lemma which significantly improves the proof of the size principle.

He then goes on, however, to spoil his argument by trying to show that the proof is insufficient by reason of what turns out to be an erroneous counterexample. This counterexample is supposed to show that on some occasions people prefer larger than minimal winning coalitions, whereas what it actually shows is that they prefer these larger coalitions only if they can change the rules of the game.

This counterexample derives from a five-person game (Game 1) which he says (p. 1203) has the characteristic function:

"The value of each 3-player coalition is 60.

"The value of each 4-player coalition is 50.

"And the value of the 5-player grand coalition, of course, is zero."

Mr. Hardin's use of the word "each" catches the spirit of Von Neumann and Morgenstern's remark just before their definition of the characteristic function that the features of the definition hold "for all subsets of S of I" where I is the total set of players.[1] Since Mr. Hardin's game is said to be zero-sum and symmetric, we can infer that the rest of the characteristic function of his game is:

The value of the empty coalition is zero.

The value of each one-person coalition is −50.

The value of each two-person coalition is −60.

Quite properly Mr. Hardin defines the characteristic function as "the total payoff which the coalition can guarantee its members no matter what the players outside the coalition do" (p. 1203). Of course, since in zero-sum games not all sets of players can achieve their values, what this means is that, for *every* set of a given size, that set can guarantee at least the value of a coalition of that size, if the set forms a coalition.

When Mr. Hardin comes to offer his supposed counterexample, however, he violates this definition and his given characteristic function. The payoff schedule in his counterexample is:

A	B	C	D	E
26	26	26	−28	−50,

and is obtained by (ABC) bribing D or vice versa. Quite obviously the subset composed of D and E gets less than what it could guarantee itself against all the other players and furthermore could unilaterally enforce. By the characteristic function, D and E as a two-player set *can guarantee itself no less than* –60; *yet in the* counterexample it gets –78. Clearly what Mr. Hardin has done is change his game in midargument, allowing D and E to content themselves with what they could easily avoid. Let us call his new game Game 2, then the characteristic function of the two games can be depicted as in Figure 1. As can be seen from the figure, three-person sets would much prefer Game 2 to Game 1. Of course a counterexample that relies on changing the characteristic function of the game under consideration does not prove much of anything. The fact that the members of the set of D and E in the supposed counterexample are receiving less than they could guarantee and enforce for themselves in a coalition should alert us that something funny is going on. Specifically, it should alert us to the fact that the four-person coalition and payoff is simply not realizable among rational players who have plenty of time to bargain. If this payoff schedule were proposed (as it well might be) or even temporarily adopted (as also might happen), then E could offer a counterproposal in which D and E as a coalition both improve their positions individually, for example

---

[4] For example, they claim that a variable that has only "three levels" *ipso facto* suffers from "problems of limited variation." The absurdity of this remark is clear when we think of binary variables (e.g., the black-white distinction in the U.S. and the Catholic-Protestant distinction in Northern Ireland): presumably Ames and Bates do not think that such variables can have only trivial (i.e., substantively meaningless) variation. Surely they know that to address the question of limited variation, one compares the observed dispersion of a variable with its theoretically possible range.

[1] John Von Neumann and Oskar Morgenstern, *The Theory of Games and Economic Behavior* (Princeton, N.J.: Princeton University Press, 1953, 3rd edition), p. 241.

$$\begin{array}{cc} \underbrace{\text{A} \qquad \text{B} \qquad \text{C}}_{60} & \begin{array}{cc} \text{D} & \text{E} \\ -20 & -40 \end{array} \end{array}$$

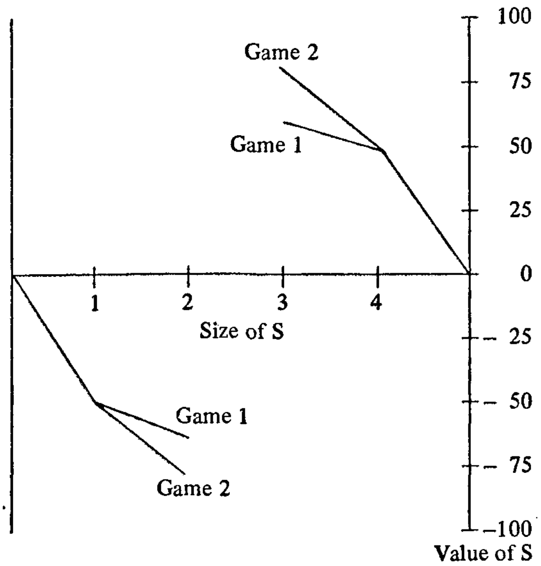


Figure 1.
Characteristic Function

I do not suggest that such a payoff schedule is itself stable. I do point out, however, that

(1) it is better for both D and E than the distribution proposed by Mr. Hardin, and

(2) D and E by themselves can *enforce* it. That is, regardless of what A, B, and C do, D and E can refuse to pay out more than –60 in Game 1. Since they have a positive incentive to so refuse, they have the motive as well as the ability to prohibit this supposed counterexample.

Furthermore, in general for this game, if any coalition of four persons contains a subset of three persons who together get more than 60, there is some division of –60 among the remaining two members which is individually better for both of them and which the two person coalition can *enforce* by reason of the rules of the game in the characteristic function. If they do enforce it – and among rational players enforcement of the two-person coalition is to be expected – then they can also force the replacement of a four-person coalition with a three-person one. Consequently, any four-person coalition containing a three-person subset that receives more than 60 is unstable, unless, of course, the game is changed, which is what I suppose Mr. Hardin means to do by allowing it to stop at his proposed payoff distribution.

If there is a four-person coalition in which some three-person subset receives exactly 60, then, while the four-person coalition can be formed, there is no need for it, and it is difficult to see what role the fourth person plays in it. Surely the three-person set has no motive to expand to a four-person coalition just to contain the unnecessary player. In this case the four-person coalition is trivial.

If there is a four-person coalition in which no three-person subset receives as much as 60, then some such subset can, with an advantage for each of its three persons, reduce the coalition to a three-person one and enforce it to obtain 60. Consequently, with both the motive and the power they can be expected to do so. Of course, such a three-person coalition may itself be unstable. Thus, when

A	B	C	D	E
25	25	10	–30	–30 (ABC), (DE)

is proposed or exists, players C, D and E can upset it with the payoff schedule

A	B	C	D	E
−30	−30	20	20	20 (CDE), (AB)

and enforce it. Just what three-person coalition will form and be enforced cannot be predicted, although *some* three-person coalition can be enforced if the members together receive 60.

To summarize the results for *four*-person coalitions, in Game 1 it is the case that either

(1) they are pointless, or

(2) there is within them some subset of three persons all of whom individually prefer an enforceable three-person coalition, or

(3) there is some immediately available set of two players (one inside and one outside the four-person coalition), both of whom prefer some enforceable payoff arrangement in a two-person coalition to permitting the four-person coalition to come into existence or to continue to exist.

Consequently, we may be sure that so long as Game 1 is being played, there will be no four-person coalitions because no four-person coalition can enforce itself against the sets of players that necessarily have some preferable payoff in two- or three-person coalitions. That is, a four-person coalition can be forced, either from inside or outside, to reduce to three persons.

But we do not know which three-person coalition will exist, for in zero-sum games there are no undominated payoff schedules. Hence, for every division among three persons some superior alternative of distribution and coalition can be found. Is there any way out of the cycle? Intuition tells us that, since three-person coalitions valued at least 60 can enforce themselves, some one of them will in fact do so. But which one? Solution theory attempts to offer an answer. To sketch one such simple theory for this game – and to show that it absolutely excludes four-person coalitions – I introduce the notion of the bargaining set.[2] In this theory there are payoff configurations in which each actually formed winning coalition receives at least its value and no individual player does worse than the value of a coalition of a single member. The problem is to determine whether or not a payoff configuration, x, is stable over n players:

$$x = (x_1, x_2, \ldots, x_n; B_1, B_2, \ldots, B_h),$$

where $B_1, \ldots, B_h$ are existing coalitions and $x_1, \ldots, x_n$ are the individual payoffs. The set of all stable payoff configurations is called the bargaining set. In theory, all actually occurring outcomes will be chosen from this set.

To determine stability, consider some winning coalition B_j in x with disjoint subsets K and L. We say K has an objection or threat against L if K can propose a configuration

$$y = (y_1, y_2, \ldots, y_n; C_1, C_2, \ldots, C_r),$$

in which L is not a member of K's new coalition and in which

$y_i > x_i$ for all i in K

$y_i \geqslant x_i$ for all the players, i, whom K has brought into the winning coalition to replace L.

We say that L has a counterobjection or counterthreat against K if L can propose a configuration

$$z = (z_1, z_2, \ldots, z_n; D_1, D_2, \ldots, D_k),$$

where L has formed a winning coalition with some of K's partners in y (but not including K itself) such that

$z_i \geqslant x_i$ for all i in L and for all i who are L's new partners,

$z_i \geqslant y_i$ for all i who are in L or who are in L's new coalition and who were partners of K in y.

We say *that x is stable if for every objection* y, there is a counterobjection z. The fundamental notion is that, if K and L threaten to split up, they will do so if one can get an advantage that cannot be countered; but if every threat can be countered, there is no point to splitting up, and hence x is in that limited sense stable.

In Mr. Hardin's game, the four-person coalition with the configuration

$$x = (26, 26, 26, -28, -50; (ABCD), (E))$$

is clearly unstable. Let K be person D and let him propose against L (here (AB)), the objection

$$y = (-30, -30, 28, 16, 16; (AB), (CDE)),$$

which is a valid objection because

$y_D > x_D \; (16 > -28)$

$y_C \geqslant x_C \; (28 > 26)$

$y_E \geqslant x_E \; (16 > -50).$

Can L (here (AB)) offer a valid counter objection? To do so A and B must each get as much as they got in x, that is, 26 each. Then they may ally with either C or E or both. If with C, then C must get at least $z_C = 28$ and the total for (ABC) is 80; but D and E do not have to pay out more than -60 so (ABC) is an impossible dream. If (AB) ally with E, then E must get at least $z_E = 16$, and the total for (ABE) is 68, which is also an impossible dream, again because C and D do not have to pay out more than -60. If (AB) ally with both C and E, they (C and E) must get respectively 28 and 16 so that the total for (ABCE) is 96 and D need not pay out more than -50. Hence L (here (AB)) has no possible counterobjection, and therefore K's (here D) objection can stand uncountered and x must be regarded as unstable.

---

[2] R. J. Aumann and Michael Maschler, "The Bargaining Set for Cooperative Games" in *Advances in Game Theory* (No. 52 in Annals of Mathematics), ed. M. Dresher, L. S. Shapley, and A. W. Tucker (Princeton University Press, 1964), pp. 443–476. I am indebted to Richard Smith for pointing out the relevance of the bargaining set.

In general in Game 1, all four-person coalitions involve unstable payoff configurations either because one member is getting a negative payoff or because no set of three players is achieving its full potential. But there are some stable payoff configurations with three-person winning coalitions, namely all the permutations of (20, 20, 20, -30, -30). Consider

$$x = (20, 20, 20, -30, -30; (ABC), (DE)).$$

Let K be C and L be (AB). Then, if C threatens

$$y = (-30, -30, 20 + \epsilon, 20 - \epsilon, 20; (AB), (CDE)),$$

which is valid because $y_C > x_C$ and $y_D \geqslant x_D$ and $y_E \geqslant x_E$, L (here (AB)) has a counterobjection:

$$z = (20, 20, -30, 20, -30; (ABD), (CE)),$$

which is also valid because $z_A \geqslant x_A$, $z_B \geqslant x_B$, $z_D \geqslant x_D$ and $z_A \geqslant y_A$, $z_B \geqslant y_B$, $z_D \geqslant y_D$. By appropriate rearrangements of $+\epsilon$ and $-\epsilon$, it is possible to show that *all* objections of A, B, and C against each other with alternative three-person coalitions can be responded to with similar counterobjections. Hence x is stable and in the bargaining set.

I have thus shown, albeit informally, that Game 1 admits of no stable four-person coalitions because they can be broken up from either inside or outside by players who have both the motive and power to do so. But Game 1 does admit some three-person coalitions that have a kind of limited stability even though there is no absolute guarantee against cycling among possible coalitions and payoff schedules. We can conclude, therefore, that while all possible coalitional arrangements in this (and all other) zero-sum games can be dominated and hence need not last, still there is some possible stability in three-person coalitions, and, for certain, there is no stability at all in four-person coalitions, which is precisely what the size principle asserts.

In a footnote on page 1206 Mr. Hardin remarks that his example is based on a "more general demonstration" in Norman Frohlich "The Instability of Minimal Winning Coalitions," *APSR*, 69 (September, 1975), 943–946. This essay contains exactly the same error as Mr. Hardin's example, except where Mr. Frohlich applies it to characteristic functions with positive slope.[3]

In Mr. Frohlich's argument, for a game with $n \geqslant 5$ players, there is a winning set S, with s members, where $s \leqslant n-2$, and a complement, $-$S, which is divided into sets R and Q. The essential feature of the situation is that the members of R can "bribe" their way into (S∪R) by paying S an amount a, where a means less of a loss for the members of R than their current loss in R. Now before the bribe the set S was paid v(S), and the set (R∪Q) was paid $-$v(S), since the game is zero sum. After the bribe (S∪R) is paid (v(S)+a), while the sum of the payments to the subset composed of R and Q is ($-$v(S)$-a$). Now, by itself (R∪Q) can guarantee that it receive at least $-$v(S). But after the bribe its members actually receive ($-$v(S)$-a$), which is less than they could jointly guarantee themselves. Hence R and Q have both the motive and the power to form a coalition that reduces S back to s members in exactly the way I have pointed out in discussing Mr. Hardin's example. The only way in which the bargaining among players could possibly stop at the larger coalition (S∪R) rather than the smaller coalition, S, at least for zero-sum games with negative slope, is that there be some heretofore unmentioned rule that allows the larger coalition to change the game into one in which v(S) is greater and $-$v(S) smaller than originally stipulated. In the absence of such a rule, by the generalization of the arguments I offered about Mr. Hardin's game and the bargaining set, it can be shown that the set of outcomes that actually come into existence consists entirely of configurations with minimal winning coalitions. Indeed, it has been pointed out to me that the size principle is a straightforward (and trivial) deduction from the notion of the bargaining set.

WILLIAM H. RIKER

University of Rochester

---

[3] In William H. Riker and Peter C. Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffe, N.J.: Prentice-Hall, 1973), p. 185, a similar argument is used about characteristic functions with positive slope, but it is described on p. 186 as "organizational" as distinct from mathematical.

TO THE EDITOR:

Professor Riker's letter falls into two parts: a criticism of my "Hollow Victory," and a new set of assumptions on which to base an alternative proof of the size principle. The first part, the criticism of "Hollow Victory," is based on the assertion that "what Mr. Hardin has done is change his game in midargument." This assertion and the accompanying Figure 1 are wrong. The payoff schedule (26, 26, 26, −28, −50) is *demonstrably an imputation for my Game 1*, because it is derivable by inspection from the characteristic function which Riker spells out (Luce and Raiffa, *Games and Decisions* (New York: Wiley, 1957), p. 193, give the defining conditions on imputations, which are merely the permissible outcomes in a game). The four-person coalition ABCD receives 50 as it should. It requires none of the disingenuousness of changing the rules of Game 1 for the players to achieve these payoffs, and Figure 1 is without game-theoretic meaning. Therefore the first half of Riker's letter as well as his concluding remarks on Norman Frohlich's paper are wrong.

If, on the contrary, Frohlich's and my papers were genuinely wrong, then Occam's razor (what can be done with fewer assumptions is done in vain with more) should have excised the second part of Riker's letter, because it is based on a restricted version of the Aumann-Maschler bargaining set (see the reference in Riker's fn. 2). The latter is built by piling assumption on assumption until, in a limited form which does not entirely please Aumann and Maschler, the restriction to minimum winning coalitions is virtually assumed. Bargaining set theory is too involved for adequate treatment in this space, but I will briefly summarize several aspects of it which are relevant to the size principle.*

A simple version of bargaining set theory defines "individually rational payoff configurations" to be stable (or in the bargaining set). Such configurations are those in which all extant coalitions achieve their value and in which every permissible objection can be met by a valid counterobjection. Riker writes that the outcome (26, 26, 26, −28, −50; ABCD, E) enforced by a four-person winning coalition is not in the bargaining set of Game 1. He is right. In the simple bargaining set theory, however, this is of no consequence for the size principle, because the outcome (12½, 12½, 12½, 12½, −50; ABCD, E), all its permutations, and the outcome (0, 0, 0, 0, 0; ABCDE) are in the bargaining set. Hence, the bargaining set places no restrictions on the size of winning coalitions.

Such outcomes as (0, 0, 0, 0, 0; ABCDE) are annoying in a solution theory. To prevent them, Aumann and Maschler (p. 445) propose the additional assumption that a coalition will not form if some of its members can obtain more by themselves forming a permissible coalition." Noting that this is "a strong assumption," they call it "coalition rationality." If we now add this assumption to the definition of a revised bargaining set, outcomes associated with four- and five-person coalitions are no longer in the bargaining set for Game 1. If a coalition has incentive to expand under Riker's earlier assumption set (in Riker and Ordeshook, *Introduction to Positive Political Theory*, p. 177), it cannot now do so if the resultant coalition would include a subset with incentive to evict one or more members. *Definitio ex machina*, larger than minimum winning coalitions will not be permitted. In this sense, the size principle is, as Riker here writes, a trivial deduction from the revised Aumann-Maschler bargaining set, because the principle is proved by a question-begging assumption.

The objection and counterobjection argument from bargaining set theory renders the word "bargaining" peculiarly inapt – the actual nature of the theory is more nearly antibargaining. The objection-counterobjection definition of rationality prohibits all bargaining in which one player gives up something in order to bribe another into a mutually beneficial coalition. A player is permitted to bargain not in order to protect most of what he might receive but only to increase it (in an objection) or to maintain it exactly (in a counterobjection). This implies a peculiar preference ordering. If Aumann (or Maschler) were wealthy and disdainful of the larger society, he naturally would prefer keeping his wealth untaxed; but faced with a hard choice of losing it entirely or merely paying a small tax on it, he evidently would prefer losing it entirely (the more "stable" outcome). We have become inured to the fact that assorted game theorists can devise situations in which counterintuitive outcomes do not seem perverse, but we may still hesitate to put great store in behavioral theories based on counterintuitive (even counterfactual) behavior assumptions.

Unfortunately, this is not the only, nor even perhaps the most important, prohibition on bargaining in the Aumann-Maschler theory. Bargains initiated by losers are also prohibited, because objections can be posed only from within extant coalitions. Why must a loser shut his political mouth until someone among the winners approaches him? Perhaps because otherwise the bargaining set would often be as empty as the core. For example, in Aumann and Maschler's (p. 447) game one of whose two "stable" outcomes is given in the payoff configuration (75, 25, 0; AB, C), suppose the loser were permitted to object with (0, 26, 24; A, BC). Player A could not counterobject, and no payoff configuration in the game would be in the bargaining set. Is it rational of player C to be reticent as required by Aumann and Maschler when he might bargain his way to a better life while the worst he risks by speaking up is to remain in his dismal state?

---

*Much of my understanding of bargaining set theory I owe to discussions, under the duress of the printer's deadline, with Joe Oppenheimer.

Kenneth A. Shepsle ("On the Size of Winning Coalitions," *APSR*, 68 [June, 1974], 505–518) used bargaining set theory to argue for the size principle in five-person symmetric games. Unlike Aumann and Maschler, Shepsle permitted losers to bargain. Rather than prove the principle, he provided a four-person payoff configuration with an objection to which he said there was no counterobjection. The *bete noire* of the size principle, Robert L. Butterworth, in a comment (*ibid.*, pp. 519–521), provided a counterobjection. With a display of courage that a contributor to a mathematics journal would not dare, Shepsle nevertheless let his paper go to print. Perhaps the only constructive result in that twenty-page use of the *Review* was Shepsle's final footnote in his acknowledgement of error, or rejoinder (*ibid.*, p. 524n), noting that "the size principle is an immediate consequence" of imposing coalition rationality. In his main paper, he cited a paper by Robert Wilson (with which I am not familiar), saying Wilson assumes coalition rationality "by restricting consideration" to minimum winning coalitions (*ibid.*, p. 513n). For properly defined games, assuming coalition rationality and permitting only minimum winning coalitions to form are equivalent. Aumann and Maschler (pp. 445–446) express unease with the assumption of coalition rationality because it is subject to the usual objections to the core as a solution theory. Among these is that the core is essentially counterfactual in that players of zero-sum games commonly achieve outcomes, whereas if the assumptions underlying the core are the relevant standard of rationality, such games are not resolvable.

The strongest objections to bargaining set theory, especially in its restricted Aumann-Maschler variant, are that it rules out of order much behavior which seems more compelling than the theory. I will not elaborate the type of such behavior any further here other than to note that not lowest on the list is the behavior of players in Game 1 in forming a larger than minimum winning coalition. Suppose A, B, and C contract to coalesce and to cooperate in trying to increase their payoffs above those in (20, 20, 20, –30, –30; ABC, DE), which belongs to the revised bargaining set. ABC may now offer separately to both D and E entry into a larger coalition with a payoff of –28. D and E now face a partial Prisoner's Dilemma. If they cooperate in coalition with each other, they can achieve a joint payoff of –60, although at least one of them will then receive less than –28. If one of them defects, they receive a joint payoff of –78, as in the outcome (26, 26, 26, –28, –50). Of course, it is impossible for both to defect, but that is all the more reason for each to hasten to defect first. Much as in usual Prisoner's Dilemma, at least one of D and E will be better off to defect no matter what the other plans to do. Indeed, because the Prisoner's Dilemma is only partial in this respect, it is of little advantage to D and E that they might communicate to each other their offers from ABC.

All of this behavior is defined out of the realm of rationality in the revised Aumann-Maschler solution theory. If one nevertheless likes the fundamental emphasis of bargaining set theory, he must either assume the size principle or accept such outcomes as (0, 0, 0, 0, 0; ABCDE). I and no doubt many others, including Ransom Gillet, would not find it uncongenial to assume the size principle in many contexts, although that leaves us very far from proving it in the elegant fashion Riker originally attempted from an unobjectionable set of assumptions. Indeed, it leaves us in a position not unlike conventional speakers of the mother tongue who invoke "rationality" in explaining politics. One might say it was rational of the United States to bomb Hiroshima. The only difficulty with the claim is that another might say it was irrational. There being no commonly accepted set of assumptions from which to arbitrate the claims, the debate cannot be resolved. Similarly, if Tip O'Neill says Ransom Gillet was silly, that the tax reform or tariff coalition ought to be as large as possible rather than as small as possible, then political scientists have no acceptable positive theory with which to instruct O'Neill of his folly. Indeed, he might be right.

RUSSELL HARDIN

University of Maryland

Freedom and Distributive Injustice

TO THE EDITOR:

Douglas Rae's review of *Anarchy, State and Utopia* by Robert Nozick (*APSR*, 70 [December, 1976], 1289–1291) is an example of the highest level of the reviewer's art. He takes a position he disagrees with seriously, and attempts to criticize where he finds it weak and to learn where he believes that it makes a contribution.

Rae regards Nozick's "Wilt Chamberlain" critique of systems of distributive justice which are based on their results rather than upon a method embodying freedom as "deadly serious," because such end-state principles contradict the "freedom to dispose of one's just share." Rae attempts to transcend this dilemma, although he is too intelligent to think this solves it, by contrasting the Chamberlain freedom example with "the very different cases of Satchel Paige and Jim Thorpe. It is not that they earned too much but too little, and they earned so little because of raw and simple prejudice worked out in the marketplace" (p. 1291). So Rae finds Nozick's minimal state "unresponsive to our trouble," "innocent," and "too optimistic to suit our case" (1290–1291).

Yet is Nozick's minimal state unresponsive to the Satchel Paige and Jim Thorpe cases? For Thorpe, the criticism that he earned too little cannot stand. In the market, he was paid one of the highest salaries of his day both in professional baseball and football. When he signed with the New York Giants, he was paid "unusual money for a rookie."[1] Rae must be thinking of the AAU decision taking Thorpe's Olympic medals away. But the marketplace had a reaction to this. His biographer says that the AAU decision was made and sustained "despite the scorn and criticism of public and press."[2] The marketplace then paid Thorpe handsome salaries as compensation during his best years and later elected him the first president of the American Professional Football Association.

Now what was the non-Nozickian government doing? Well, it had an end-state program not only for Thorpe but for all Indians. It had earlier redistributed the land of Thorpe's tribe to more worthy beneficiaries first in Kansas and then again in Iowa.[3] It, likewise, had a population distribution policy which reduced the tribe from 4500 members in Kansas to 387 who reached Oklahoma. The state so believed in redistributing property that it repeated the process for a third time in the "land rush" of 1891. The government was so concerned with the welfare of its clients that it even closed the Carlisle school (it was begun with partial financing from the government) the school which had given Thorpe his opportunity to escape from end-state justice for a while. It seems that the school was not meeting current educational standards and that the discipline procedures shocked congressional sensibilities. So to protect the students, they were sent back to reservation end-state justice.

Rae is on stronger grounds with the case of Satchel Paige, for there was marketplace prejudice which cost him income. But it is instructive to look at the situation more closely. Baseball originated in 1839, and by 1867 there were 100 professional clubs in the United States. At that time, blacks played for several teams. On December 12, 1867, the National Association of Baseball Players Nominating Committee voted to exclude clubs which hired Negroes.

It is significant that the ban was instituted by a union. Peterson says that, although the succeeding players' association did not formally adopt this rule when it was organized in 1871, it kept the rule as a "gentlemen's agreement."[4] The owners in most of the early leagues, however, did not accept the ban for the obvious reason that several had valuable players already on their clubs. Moreover, they willingly traded for blacks when this was to their advantage. Thus, the Northwestern League, called one of the four strongest at the time, had Negroes; so did the International League, then a major league, the American Association (later league), and the Western and Middle States leagues.

So, at this time, baseball offered a classical example of union restrictions on a labor market. There even is evidence that money, rather than raw prejudice, was the issue. Thus, the great black star for the International League in the 1880s, Frank Grant, was resented by some of the white players because he asked for more money. Yet, one also notes the usual lack of success of these techniques when they are not backed by law. They fail because the market incentives to "cheat" on the other owners to gain profit are too great to make the restrictions work. Even when the International League formally adopted the color line in 1886, not only were the five black players in the league not ejected but the Syracuse club signed two more at the end of that season. The same year the Ohio State League rescinded its previous color ban.

---

[1] Jack Newcombe, *The Best of the Athletic Boys: The White Man's Impact on Jim Thorpe* (Garden City, N.Y.: Doubleday, 1975).

[2] *Ibid.*, p. 209.

[3] *Ibid.*, pp. 15, 21.

[4] Robert Peterson, *Only the Ball Was White* (Englewood Cliffs, N.J.: Prentice-Hall, 1970), p. 17.

Peterson, who is not a political scientist, finds that, mysteriously, the era of black players in white leagues ends in 1895.[5] Of course nothing in social life is this simple, but we political scientists know that the government gave us *Plessy v. Ferguson* the following year. The informal exclusion was now backed by the power of law, and between 1898 and 1945 it was so effective that blacks played only in Negro leagues.

It is interesting that John McGraw tried to "sneak" a black on the Giant team as an Indian in 1901[6] and that two dark-skinned Cubans played between 1911 and 1921. I find it critical, though, that major-league baseball integration preceeded *Brown v. Board of Education* by almost a decade. The facts are revealing. In 1943 Branch Rickey told the major stockholder in the Dodgers that he needed to rebuild the team with young talent and that this might mean signing Negroes. The major owner – a bank of all things – told Rickey to go ahead if it meant getting a pennant-winning and, therefore, successful team. An economic boycott of Yankee Stadium in 1945 also helped prepare the way for Jackie Robinson's breaking the color ban in 1946.

Paige was not hired until Bill Veeck signed him in 1948. Since Paige played his first game in 1924, he was underpaid for perhaps 24 years. But was it the market that was to blame or the state acting through the Supreme Court? Or rather, was the market not being bold to defy the separate-but-equal doctrine of the state? Of course, *Plessy* did not actually mandate segregation as an end-state but several state and local laws did, especially in St. Louis, Baltimore, and Washington and for spring training, and in the minor leagues. The market eventually permitted the hiring of Paige (although not his qualified early teammates) and even his election to the Hall of Fame. When the state finally did act, all it did for Paige was restore the status-quo of pre-1896 integrated ballfields which he had already experienced some six years earlier.

Is the minimal state too optimistic in its unresponsiveness to problems of distributive justice as apparently believed by Rae? Or is it naive to believe that the state can do much more than give equality before the law as Nozick thinks? As Rae says, the question "is no newer than the old story of freedom versus equality" (p. 1291). Yet, it seems to me, if one looks at the Chamberlain, Thorpe, and Paige examples, these chapters favor freedom.

DONALD J. DEVINE

University of Maryland, College Park

The Strained Alliance

TO THE EDITOR:

Professor Byung Chul Koh has written a specious comment on my recent *The Strained Alliance: Peking, P'yongyang, Moscow and the Politics of the Korean Civil War* (*APSR*, this issue). He sets up a straw man and, apparently to his own satisfaction – surprise – has knocked it down. This has been accomplished by ignoring much of the volume's thrust and the data which support it. Instead, he has devoted his limited space to constructing porous and stilted criticisms.

The review's cynical tone is constant; e.g., "... sets out to demolish ... hypotheses are so precariously grounded ... no one, including the author, denies ... apparent lack of integrity ... admits at the outset it is 'not a definitive work.' " Each atmospheric phrase is designed to implant doubt upon the book's major points, which the review largely neglects.

The volume itself, in contrast, is more restrained. For example, the full context of the above last phrase which "admits at the outset it is 'not a definitive work,' " reads:

> Because of this lack of facts regarding each of the four involved countries, this is not a definitive work. Rather, it attempts to present data and raise hypotheses not usually considered. This is done with the belief that it is as worthwhile to ask questions and to suggest new viewpoints as to repeat what appears to have been the unsatisfactory answers previously advanced about the war. Therefore, not much space is devoted either to arguments which are now generally accepted, and to which we agree, or to earlier hypotheses which seem inadequate explanations to important questions (p. xvii).
>
> . . . In this exploratory book I have attempted to walk a middle line between widely known and less familiar data, hoping that others will be prompted to research further some of the hypotheses pursued here. At points in the narrative where the facts of a situation are well-known and widely accepted, the story is touched upon only to provide context (*Ibid*).

Although not the principal purpose of a review, this one, in its eagerness to nit-pick, did not accept my invitation to offer, however briefly, reasonable substantive additions.

The only factual criticism which Professor Koh does deal with concerns the term "civil war." He is not persuasive. Of course I do not

---

[5] *Ibid.*, p. 49.

[6] *Ibid.*, pp. 54–56.

'deny' that there was "massive external intervention in the war." Neither does Seoul, P'yongyang, Moscow, Peking, or Washington. However, it is a nonsequitur to assert that, because there were outside participants, the war, therefore, was not in any important measure a civil one. If outside forces are involved, are internal ingredients of the origins of a war to be ignored? This is the implication of the reviewer's logic. It should be noted that civil wars (either potential or current) do, in fact, often attract outside participants. My book discusses the war as primarily, but *explicitly not solely,* originating from internal causes, which were externally encouraged. It was (and is) a reality that Koreans in both halves of the peninsula considered themselves to belong to the same, artificially divided, nation. Does Professor Koh 'deny' the fact that the civil aspect of this war was an important dimension? The review states: "Setting aside its origins for the moment. . . ." But it never returns to this vital problem. Does Professor Koh dismiss the idea that looking at the war from its partially internal nature provides a valuable perspective?

Related to this is the reviewer's denial that "the Korean language makes no rigorous distinction between civil and other types of wars." Aside from his approving reference to the Korean term for "fratricidal rebellion" (remembering that in 1950 both Seoul and P'yongyang claimed to represent the legitimate Government of the nation, this would be a reasonable synonym for civil war), is the fact that English-Korean dictionaries have entries for "civil war." More importantly, while spending time wondering about the translation of this term, Professor Koh might have taken advantage of the citation which carefully traced its utilization by each state.

A major interest of the book was which state was saying what to whom and when. The volume quotes a Soviet domestic broadcast of January 1951, contrasts it with a Korean message sent to the Soviet Union in August 1952, and then speculates "may well have been meant as an indirect rebuke to the Soviet Union." (As in each of the review's crippled quotes, the cautioning phrase – "may well have been meant" – was dropped.) I was questioning the *timing* and *frequency* of the term's use by *Moscow.* The "preface" explains why such attention had been paid to the transcripts of radio broadcasts:

> From close textual readings of the transcripts from the 1950–53 period, it is frequently possible to determine shifts in attitudes between the three communist states. This is particularly true when a radio broadcast is being beamed to one specific country in that state's language.

Was this the first time that the Soviet Union had translated this Korean phrase as "civil war"? Would this not suggest Moscow's intensifying interest in separating itself from the war?

The review's use of my phrase "As a 'harder' indication that June 25 was not the prearranged date. . ." as a prelude to its editorial ". . . simply strains one's imagination. . ." is dramatic, but of small significance. The word " 'harder' " was punctuated purposefully with quote marks *in the book itself* (p. 118) in order to suggest to the reader that this specific document should be dealt with cautiously. On the other hand, this 'red herring' allows the reviewer to avoid the fact that this single quote is but one from a lengthy list of indicators which, I stated, *may* suggest an original invasion date of early August 1950. Moreover, it should be noted that the validity of this document did not "strain the imagination" of the scholars who are cited, Profs. Rodger Swearingen and Paul Langer.

In a similar fashion, while Prof. Koh is accurate in suggesting that weapon supplies often continue after an invasion, the *pattern* of supply and the overall context of timing and flows indeed *do* contribute insights into intentions and strategies.

Among the review's overconfident assertions is this statement: "That the relations among the three communist states on the eve of, during, and after the Korean War were consistent with this general pattern [of strains] therefore does not come as a surprise." It may, however, have come as some "surprise" to readers of several earlier books which touched on these states' relations of this period (including Prof. Koh's 1969 work) which had not examined tensions within the alliance during the war. The Soviet Union, moreover, had been viewed as the sole factor causing the North's invasion. My book readily acknowledged the Soviet's crucial military assistance, but supplements this fact with the equally important factor of North (and South) Korean politics.

The reviewer mixes oranges and apples when he asks why doesn't Peking "set the record straight with respect to its perception of Moscow's role in the U.N. in 1950?" (It also should be mentioned that the review does not comment on my argument that the Soviet Union consciously 'froze' China out of the United Nations in 1949–50.) After all, he notes, "In November, 1975, Peking even *distorted* Soviet Ambassador Malik's U.N. speech. . ." The apparent answer to the review's question is that

Joseph Stalin and his policies generally are praised, not criticized, by today's China. He likewise misleads by failing to note the depth of the Chinese sources traced in the footnotes.

Prof. Koh's gratuitous comment on the respect for "father" in Korea is irrelevant; firstly because the text *explicitly* was referring to a Moscow radio broadcast. Secondly, and of more import, North Korea had become increasingly disenchanted by the unsatisfactory support which the Soviet Union was supplying, and was relying more on China. Analogies to "indirect rebukes" are not uncommon (e.g., Saigon's similar-style messages to the U.S. during its last decade of war).

Unfortunately, the review ignores other data and hypotheses, partly drawn from then–classified U.S.-Army Intelligence sources, which might interest readers. Three examples: (1) In spite of the frequency of the claim, there had been *no* actual Taiwan Straits 'interdiction' in the summer of 1950 of a strength sufficient to prevent a successful Chinese invasion of Taiwan; (2) the Soviets pulled most of their personnel out of Korea after the U.S. strafed a Soviet air field inside of Siberia in October 1950. I then discuss the effect this had on China and North Korea; (3) the book details the inadequate and insufficient Soviet aid given to China and North Korea throughout the war, and discusses how this reality affected the alliance.

Perhaps most important, it is necessary to respond to the reviewer's statement about my "apparent lack of integrity." The allegation is serious, and would be applicable *if* I were taking credit for original points. In fact, however, I attribute unique points to their author's scholarship. Moreover, the review fails to tell the reader that my book is replete with citations to unpublished Ph.D. theses and conference papers. As carefully noted in the book, my purpose was both to present commonly agreed-upon information and to reinterpret some of this information, as well as to offer "new" data.

It is paradoxical that the review faults the book even as it demonstrates how carefully the source material was handled, e.g., the Korean term rendered as "civil war," *via Pravda, via* an English translation. I am not alone in building upon background material (as distinct from data and hypotheses which are presented as original) which is widely shared. My "preface," only a part of which is quoted above, made this sharing of widely accepted material explicit. Further, if an interested reader wished to look at our two respective volumes and the phrases which are briefly parallel, they will note that Prof. Koh has selected only the congruent wordings, with ellipses filling in the nonsimilar thoughts. Preceding or following sentences and paragraphs often diverge from the quotes presented by the reviewer; moreover, these phrases are well known to students in this subfield. This same exercise of seeking agreed-upon information can be followed with other works dealing with widely accepted material.

In this context, Prof. Koh's remarks on three specific sources in the book are all drawn from 28 pages of text. These in turn – again the reader of the review would be unaware of this – are supported by 76 footnotes which frequently drew on original, and sometimes contradictory, sources. Another comment, which unsubtly implies possible plagiarism, refers to a chapter of 33 pages, with 34 notes (most of which have multiple entries). This includes three unpublished theses. The footnote which caused Prof. Koh to wonder has both the Korean and English publication data, and is followed by a relevant Ph.D. thesis by Prof. Benjamin Min, with his page number given. It should also be noted that in the remainder of the book the Korean volume *(Fifteen-Year History of North Korea)* is referred to only in its English-translation citation. Interestingly, after this chapter, only English language translation, with its page numbers, are given. I am sorry if Prof. Koh felt slighted, but there was no appropriation of original ideas.

Perhaps it is necessary to point out that in the workings of memory, one retains not only background, but the language in which it has been encased. The review would have been more useful if it had weighed my intentions and caution seriously, examined my use of translation from Chinese to English, and told the reader if there were data (or hypotheses) in the book which he considered informative, interesting, or helpful in taking a fresh look at several of the book's various themes.

ROBERT R. SIMMONS

University of Guelph, Ontario

TO THE EDITOR:

A basic and explicitly articulated premise of my review was that the scholarly value of the type of Kremlinological exercise which Professor Simmons performs in his book hinges on the plausibility of inferences in terms of the rules of elementary logic. If the inferences are not plausible, such an exercise remains at the level, not of serious academic discourse, but of trivial armchair speculation. It was and remains my considered judgment that his work is seriously encumbered by logical fallacies, of which a few examples were given in the review. What is more, its credibility is severely undercut by what I view as questionable ethical standards of the author.

It is noteworthy that the author's lengthy letter does not contain any explicit denial of my implied charge of plagiarism. Instead, he refers to my failure "to tell the reader that my book is replete with citations to unpublished Ph.D. theses and conference papers" and to my alleged selection of "only the congruent wordings, with ellipses filling in the nonsimilar thoughts." He further argues that "these phrases are well known to students in this subfield" and points out that "Koh's remarks on three specific sources in the book are all drawn from 28 pages of text [which] in turn are supported by 76 footnotes which frequently drew on original, and sometimes contradictory, sources." Finally, the author finds it "necessary to point out that in the workings of memory, one retains not only background, but the language in which it has been encased."

It is precisely because of the deceptively impressive scholarly paraphernalia in which the work is packaged that I felt compelled to point out the evidence of questionable scholarly practice. The latter calls into question the integrity and credibility of the author, which in turn considerably dilutes the worth of scholarly trappings. As for his allegation that I have "selected only the congruent wordings, with ellipses filling in the nonsimilar thoughts," it should be pointed out that "ellipses" are used only in the third example, where they occur *at the end* of the quote from Simmons. In the quote from my own 1969 work, the omitted phrases are as follows: "after operating under the banner of the Korean Independence League for a while"; "(Sinmindang)"; "as demonstrated by its growing membership"; and, finally, "(NKWP)." Not only do these phrases not contain any "nonsimilar thoughts," they also illustrate how Simmons makes use of other people's works without citation – i.e., by leaving out a few phrases here and there and by making slight changes.

If "these phrases are well known to students in this subfield," is Simmons implying that it was I who have plagiarized them in the first place? Contrary to Simmons's contention, the three quotations are drawn, not from "twenty-eight pages of text," but from *four* pages (pp. 23–26). The author's plea that "the workings of memory" may have been responsible for the inclusion of "the language" in question is understandable but, in my opinion, unconvincing. For I found other examples of a similar practice (p. 22 of Simmons and p. 2 of Koh; p. 26 of Simmons and pp. 9–10 of Koh).

What makes this episode rather perplexing is that the author is obviously a gifted writer; unlike many foreign students from non-English speaking countries, he had no compelling necessity to borrow, stealthily or otherwise, the words of other people – particularly those of a foreign-born scholar writing in an alien language. Moreover, as I pointed out in my review and the author concurred in his letter, none of the passages in question involved any novel ideas.

The author writes in his letter that I took issue with "one from a lengthy list of indicators which, I stated, *may* suggest an original invasion date of early August 1950." I discussed two of his "indicators" in my review; let me take up some of the others here. One is the paucity of Russian advisers attached to the Korean People's Army at the time of the invasion. However, whether the presence of "between 3 and 8" Russian advisers "per division" bespeaks inadequate preparation or the consummation of the Soviet plan is a matter of judgment. Another "indicator" is the reported absence of the chief of North Korean military intelligence from North Korea at the time of the invasion. If his alleged presence in Moscow at the time is confirmed, that would, in my view, buttress rather than undercut Moscow's preeminent role in the affair.

Still another "indicator" is that, "of 100,000 North Korean troops in the ranks of the Chinese People's Liberation Army (PLA) in 1949, [o]nly 12,000 . . . had returned to North Korea before June 25" (p. 118). This assertion must be examined against the backdrop that the PLA-affiliated Korean troops owed their primary loyalty not to Kim Il-song but to the Yenan faction led by Kim Tu-bong and that Kim Il-song's power position was by no means unassailable in June, 1950.

Simmons also cited Khrushchev's statement in his memoirs that "Kim [Il-song] was the initiator" of the Korean War. The authenticity of at least parts of the Khrushchev memoir, however, remains to be corroborated. The author also cites a State Department study which states that "[t]op secret work plans of the Standing Committee of the Labor Party headquarters dated January–June 1950 make absolutely no reference to the forthcoming invasion," that "a number of fairly highly placed North Korean officers that were interviewed, including the Chiefs of Staff of two divisions, stated that they had only the barest presentiment of the coming of hostilities, and that they were given no concrete indication of their onset until approximately one week before the invasion took place" (pp. 120–121). This would suggest, in my view, the extreme care with which the secret invasion plans were guarded, for if the vital advantage of a surprise attack was to be safeguarded, it was absolutely imperative that the number of people privy to the plans be kept to the barest minimum until the latest feasible moment. It is also significant that Professor Simmons conveniently overlooks the major thrust of the same State Department study – that "the U.S.S.R. was able to keep close check over the movement of the North Korean army by allocating gasoline to the army on a monthly basis" and that, given the nature and extent of Soviet control in North Korea in 1950, it is highly improbable that the decision of the North Korean regime to attack South Korea could have been taken without "Soviet approval if not inspiration." (See Department of State, *North Korea: A Case Study in the Techniques of Takeover* [Washington, D.C.: Government Printing Office, 1961], pp. 113–120.)

Another important "indicator" cited by Simmons is the rivalry between Kim Il-sǒng and Pak Hon-yong in the period preceding the June 1950 invasion. While the existence of such rivalry is well documented, it still remains to be demonstrated that it "led to the war" (p. 110). That the two North Korean leaders may have tried to outmaneuver each other in an effort to bolster their respective nationalist credentials remains a conjecture, as the author makes clear. Taken together, the preceding "indicators" do not constitute a credible basis for the author's hypothesis, no matter how imaginative the latter may be.

Professor Simmons argues that my review is "specious," "porous," "stilted," "cynical," and based on a "straw man," while his book "is more restrained." The author challenges the views of such established scholars as Allen S. Whiting (p. 48, p. 75, and pp. 149–152), George Liska (pp. 249–250), Robert Osgood (p. 251), and Raymond Aron (pp. 251–252). Not that there is anything wrong with such an approach; in fact, it is to be applauded. Whether it represents "restraint" on the author's part is an entirely different matter, however.

The author betrays his style of reasoning when he argues in his letter that the use of material which is two steps removed from the original "demonstrates how carefully the source material was handled." He avoids the central issue involved – i.e., the hazard of making inferences from something which has gone through two translations in such disparate languages as Russian and English. More important, the author is amply aware of the problems caused by errors in translation, for his own critique of Whiting rests largely on this very point. (See, especially, pp. 150–151.)

Needless to say, my review represents but one man's appraisal of the author's study which is the product of "two years [of research] in Hong Kong, Taiwan, the People's Republic of China, North and South Korea, Japan, and Washington, D.C." (back flap of the book). Regretfully, however, I am constrained to report that, whatever merits it may possess are eclipsed by the various flaws I have discussed above and in my review.

B. C. Koh

University of Illinois at Chicago Circle

EDITORIAL COMMENT

In Conclusion. It is said that somebody once asked Lord Reith near the end of his life what the best form of government was. "Tyranny," replied the old man, "tempered by assassination."

We do things more gently than that around the *APSR*. The managing editor may get away with an occasional whim, and that is about as close to tyranny as a scholar comes – or ought to come. Assassination, likewise, is fortunately stronger medicine than we provide for superannuated managing editors. They are, rather, periodically just let out to pasture.

And what a pasture it is! Insofar as we have been tired out by our editorial labors, there is the refreshing prospect of first-hand reengagement with some of the fundamental problems of our age and place, which, after all, is what writing and thinking everyday political science is all about for most of us. Insofar as editorial activity has been a stimulus and a goad to thought, we can contemplate a rededication to our own work, to see if any of the vagrant notions we have entertained from time to time will sustain a concentrated dose of attention.

To our successors we leave the faint rumble of complaints about the *Review* that one perennially hears around the profession. Sheer relief at a changing of the guard will certainly give them the honeymoon they need – and deserve – as they discover for themselves what they want to do and how they want to do it.

Errata. In "Industrial Conflict in Advanced Industrial Societies" by Douglas A. Hibbs, Jr. (December, 1976) in footnote 19 on page 1045, Equation (a) and the sentence that follows should read

$$\text{(a) } SV_t = \beta_0 + \beta_1 \{\Delta R_{t-1} - ((1-\lambda) \sum_{i=0}^{\infty} ((\lambda+\delta)B)^i \Delta R_{t-2})] + \beta_2 U_t + \beta_3 \Delta C_t + \epsilon_t .$$

$\sum_{i=0}^{\infty} ((\lambda\ \delta)B)^i$ may be viewed as a convergent geometric series with solution $1/1 - (\lambda+\delta)B$. . . .

And on page 1051, in the sentence following Equation (20) ΔW_{t-1} should read $\sum_i \Delta W_{t-i}$, and ΔP_{t-1} should read $\sum_i \Delta P_{t-i}$.

An error by our printer supplied each June *APSR* with two copies of page 639 and omitted page 638, which contained the bulk of a review of Plato's *Republic,* translated by G. M. A. Grube, by Thomas L. Pangle. The entire review can be found on pages 1336–1337 of this issue.

Articles Accepted for Future Publication

Joel D. Aberbach, University of Michigan, "Power Consciousness: A Comparative Analysis"

Paul R. Brass, University of Washington, "Party Systems and Government Stability in the Indian States"

Paul Burstein, Yale University, "Social Cleavages and Party Choice in Israel: 'A Log-linear Analysis' "

Melissa A. Butler, The Johns Hopkins University, "Early Liberal Roots of Feminism: John Locke and the Attack on Patriarchy"

Arthur DiQuattro, University of Washington, "Alienation and Justice in the Market"

Lloyd S. Etheredge, Massachusetts Institute of Technology, "Personality Effects on American Foreign Policy, 1898–1968: A Test of Interpersonal Generalization Theory"

John A. Ferejohn and Roger G. Noll, California Institute of Technology, "Uncertainty and the Formal Theory of Political Campaigns"

Morris P. Fiorina and Charles R. Plott, California Institute of Technology, "Committee-Decisions Under Majority Rule: An Experimental Study"

Mark N. Franklin, University of Strathclyde, and Anthony Mughan, University College (Cardiff, Wales), "The Decline of Class Voting in Britain: Problems of Analysis and Interpretation"

Norman Frohlich, Joe A. Oppenheimer, Jeffrey Smith, and Oran R. Young, University of Texas at Austin, "A Test of Downsian Voter Rationality: 1964 Presidential Voting"

Norman Furniss, Indiana University, "The Political Significance of the Property Rights School"

Dante Germino, University of Virginia, "Eric Voegelin's Framework for Political Evaluation in His Recently Published Work"

James L. Gibson, University of Wisconsin at Milwaukee, "Judges' Role Orientations, Attitudes, and Decisions: An Interactive Model"

Robert T. Golembiewski, University of Georgia, "A Critique of 'Democratic Administration' and Its Supporting Ideation"

George D. Greenberg, Jeffrey A. Miller, Lawrence B. Mohr, and Bruce C. Vladeck, University of Michigan, "Developing Public Policy Theory: Perspectives from Empirical Research"

John G. Gunnell, State University of New York at Albany, "The Myth of the Tradition"

Douglas A. Hibbs, Jr., Massachusetts Institute of Technology, "Political Parties and Macroeconomic Policy"

Barbara Kellerman, Tufts University, "Mentoring in Political Life: The Case of Willy Brandt"

Samuel Kernell, University of Minnesota, "Explaining Presidential Popularity"

James H. Kuklinski, Wichita State University, "Representativeness and Elections: A Policy Analysis"

Arthur J. Lerman, Yeshiva University, "National Elite and Local Politician in Taiwan"

Richard D. McKelvey, Peter C. Ordeshook and Mark D. Winer, Carnegie-Mellon University, "The Competitive Solution for N-Person Games Without Transferable Utility, With an Application to Committee Games"

Manus I. Midlarsky, University of Colorado, "Analyzing Diffusion and Contagion Effects: The Urban Disorders of the 1960s"

David W. Moore and B. Thomas Trout, University of New Hampshire, "Military Advancement: The Visibility Theory of Promotion"

Edward N. Muller and Thomas O. Jukam, State University of New York at Stony Brook, "On the Meaning of Political Support"

John M. Orbell, University of Oregon, and L. A. Wilson II, University of Nevada, "Institutional Solutions to the N-Prisoners' Dilemma"

A. Kenneth Organski, University of Michigan, and Jacek Kugler, Boston University, "The Costs of Major Wars: The Phoenix Factor"

Charles W. Ostrom, Jr., Michigan State University, "A Reactive Linkage Model of the U.S. Defense Expenditure Policymaking Process"

John G. Peters and Susan Welch, University of Nebraska-Lincoln, "Political Corruption in America: A Search for Definitions and a Theory, or, If Political Corruption Is in the Mainstream of American Politics Why Is It Not in the Mainstream of American Politics Research?"

David E. Price, Duke University, "Policy-making in Congressional Committees: The Impact of 'Environmental' Factors"

Steven J. Rosen, Institute of Advanced Studies (Camberra, Australia), "A Stable System of Mutual Deterrence in the Arab-Israel Conflict"

Howard Rosenthal, Carnegie-Mellon University; and Subatra K. Sen, University of Rochester, "Spatial Voting Models for the French Fifth Republic"

Arlene W. Saxonhouse, University of Michigan, "Comedy in Callipolis: Animal Imagery in the Republic"

Lars Schoultz, Miami University, "The Socio-Economic Determinants of Popular-Authoritarian Electoral Behavior: The Case of Peronism"

Donald D. Searing, University of North Carolina, "Measuring Politicians' Values: Administration and Assessment of a Ranking Technique in the British House of Commons"

Goldie Shabad, University of Chicago, and Sidney Verba, Harvard University, "Workers' Councils and Political Stratification: The Yugoslav Experience"

Donald T. Studlar, Centre College of Kentucky, "Policy Voting in Britain: The Colored Immigration Issue in the 1964, 1966, and 1970 General Elections"

Robert Weissberg, University of Illinois at Champaign, "Collective vs. Dyadic Representation in Congress"

Robert A. Dahl and the Study of Contemporary Democracy: A Review Essay*

GEORGE VON DER MUHLL
University of California, Santa Cruz

More than ten years ago Robert Dahl set forth his approach to the systematic study of politics in a slender volume entitled *Modern Political Analysis.* No political scientist of the period could claim that title for his treatise with more justice. A decade later the claim remains unshaken. The corpus of Dahl's works has not merely hastened the transformation of American political science from a subdivision of historical studies into a vigorously autonomous discipline; it continues to epitomize that transformation. His name, now as for nearly twenty years, inevitably appears on any list of the most distinguished contributors to the field. To review his writings is to inform oneself of the standards of excellence most generally accepted within the profession.

So much is by now no longer in controversy. And yet Dahl's pre-eminence among political scientists cannot readily be explained to those who have not read his work. At no point in his career has he revolutionized our way of looking at politics through a radical reconceptualization of the categories we employ to discuss its elements. Nowhere has he drawn the organizing premises of his multitudinous essays into a single sweeping metatheory. His writings, though often elegantly parsimonious, have generally lacked the architectonic qualities of rational behavior theory at its best. Save in *Who Governs?,* he has not sought to attain the anthropological richness, the literary imaginativeness, and the sophisticated realism of the outstanding monographs in the field. Dahl's name is associated with no one major methodological innovation, nor can he be said to have established any one finding or disproved any one hypothesis as conclusively as several less celebrated contributors to this *Review.* His incisive critiques and fine-honed distinctions have left their imprint throughout political science, yet the number of top-quality studies of contemporary governments that have borrowed wholesale from his constructive schema are surprisingly few. He is remarkable neither for the force of his moral concern nor for his capacity to stimulate fresh appraisals of familiar philosophical positions. While Dahl is in his own way as "rare" at definitions as Thomas Hobbes, and certainly far more ingenious in his use of diagrams and data, one seldom quotes him with the aesthetic satisfaction the *Leviathan's* leading passages inspire.

---

*This review will focus on the following major articles and books:

"The Science of Public Administration: Three Problems," *Public Administration Review,* 7 (Winter, 1947), pp. 1–11.
Congress and Foreign Policy (New York: Harcourt Brace and Co., 1950. Pp. 305.)
Domestic Control of Atomic Energy. With Ralph S. Brown, Jr. (New York: Social Science Research Council, 1951. Pp. 110.)
Politics, Economics, and Welfare. With Charles E. Lindblom. (New York: Harper and Row, 1953. Pp. 557).
A Preface to Democratic Theory (Chicago: University of Chicago Press, 1956. Pp. 154).
"The Concept of Power," *Behavioral Science,* 2 (July, 1957), pp. 201–215.
"A Critique of the Ruling Elite Model," *American Political Science Review,* 52 (June, 1958), pp. 463–469.
Who Governs? (New Haven: Yale University Press, 1961. Pp. 355.)
"Letters to the Editor: Thomas J. Anton's 'Power, Pluralism, and Local Politics'," *Administrative Science Quarterly,* 8 (Sept. 1963), pp. 250–268.
Modern Political Analysis (Englewood Cliffs, N.J.: Prentice Hall Inc., 1st ed., 1963, 2nd ed., 1970. Pp. 118 (1st ed.).)
Pluralist Democracy in the United States: Conflict and Consent (Chicago: Rand McNally and Co., 1st ed., 1967. Pp. 471. 2nd ed., 1972, entitled *Democracy in the United States: Promise and Performance.)*
Political Oppositions in Western Democracies. Editor and Contributor. (New Haven: Yale University Press, 1966. Pp. 458.)
"The City in the Future of Democracy," *American Political Science Review,* 61 (Dec., 1967), pp. 953–970.
After the Revolution? (New Haven: Yale University Press, 1970. Pp. 171.)
Polyarchy: Participation and Opposition (New Haven: Yale University Press, 1971. Pp. 257.)
Regimes and Oppositions. Editor. (New Haven: Yale University Press, 1973. Pp. 411.)
Size and Democracy. With Edward R. Tufte. (Stanford: Stanford University Press, 1973. Pp. 148.)

The above is by no means a complete bibliography of Dahl's published writings. In the text I have referred to, and footnoted, several further essays. On various grounds – mostly because, in my judgment, the principal ideas have been more forcefully stated in one of the above works – I have not included these other essays in what I take to be "the essential Dahl."

Why then is Dahl so esteemed by all but those who wish to belabor him as the prime symbol of Establishment complacency toward the evils of American pluralist democracy? The answer is in part, of course, that while along any one dimension one can cite more outstanding practitioners of the art, the list is seldom a long one. If some political scientists are more adept at system-building, if others draw more freely and more richly on a wider range of comparative data, if some have developed and applied the techniques of behavioral analysis to greater advantage while others have seen more deeply into the inner meaning of these findings, there are few who have combined these various capacities at so high a level. But Dahl's standing rests on much more than his readers' ability to calculate excellence along multidimensional margins. His special strengths are those that have always served social scientists well – logical acumen, skill in analytic classification, sound judgment, sheer knowledgeability, a strongly developed sense for a line of argument, and a quick eye for the significance of others' work in relation to his own. In these respects, he has few rivals. Dahl's writings all have the stamp of purposive authority on them; there are few grey patches, few that are logically slipshod, tendentious, manifestly untenable or trite, and none at all that exhibit mere technical virtuosity for its own sake. They are classical – if the term can be applied at so short a time horizon – in the sense that Aristotle's writings are classical: one turns to them repeatedly to remind oneself what a judicious social scientist, habitually inclined to assimilate and categorize all data in its most fruitful form, regards as the most important questions to ask of his world.

Continuity Amid Diversity

As one surveys the totality of Dahl's published work to date, one is struck not only by its volume, density, and fecundity, but by its sheer variety. Public administration, secrecy in government, foreign policy, judicial decision making, the formal logic of political argument, economics and politics, community government, pluralism, the concept and study of political power, comparative oppositional politics, governmental size and civic participation – on all these topics (a small fraction of the ones he has taken up) Dahl has left his distinctive mark, and the subject has never been the same again. Exceptional effort is required to identify central concerns of the discipline to which he has not addressed himself with more than ordinary penetration.

Yet behind this superficially disparate range of interests one can discern a single unifying thread of inquiry. For more than a quarter of a century, Dahl has sought to analyze modern democratic government – its operation, the conditions that encourage or threaten it, the logical status of the propositions in terms of which it is discussed and evaluated. This inquiry has led him in many directions. In his early years, it pointed toward assessing the competence required of citizens in a democracy in an era of nuclear war and unprecedented expansion in the scope of governmental responsibilities. Later, it drew him toward conceptual clarification of the relationships purportedly prevailing between governors and governed; still later, toward establishing the social matrix within which democratic government occurs. Always, however, his questions have been organized by a continuing effort to illuminate our understanding of the distinctive political institutions – all relatively recent social inventions, as Dahl has repeatedly reminded us – that have enabled the members of large-scale societies to enforce some significant degree of correspondence between their self-defined preferences and the decisions of those who govern them.

But if Dahl's focus on modern, large-scale democratic government has remained unwavering, his treatment of the subject exhibits marked discontinuities of content, scope, and level of abstraction over the years. Periodization of a scholar's work is inescapably an artifice. It ignores ongoing research and thought, underplays incongruent subordinate themes, magnifies differences and minimizes continuities in central tendencies, and it too often confuses publication dates with states of mind. These well-known pitfalls acknowledged, Dahl's publications do lend themselves to being instructively grouped into four periods.[1] Thus from the end of the Second World War until the beginning of the Eisenhower Era, he mainly addressed himself to selected operational and moral aspects of American governmental institutions and policies. Thereafter, until perhaps the close of the decade, his work became notably more universalized, more formal, more explicitly concerned with the logic of his own and others' arguments. A third period, inaugurated with his close empirical study of politics in New Haven and ending amid the growing disorder of American public life at home and abroad, marked a return to the study of contemporary American politics – this time, however, with the declared object of locating and assessing the capacity of various groups to control the terms of governmental policy decisions. The fourth (and so far, final) period has been characterized by a dramatic expansion of purview in both space (from the United States to the globe) and time (from Ancient Athens to serious consideration of utopian futures) as Dahl has sought to enhance our understanding of the implications of governmental size for civic participation and to explicate the relationship of various social and political factors to a peculiarly significant form of such participation – the participation of loyal oppositional groups in the work of democratic governments.

---

[1] The following periodization is based strictly on Dahl's published work, and not at all on more direct evidence as to the evolution of his scholarly interests. Hence publication dates, rather than the period of active research and writing, are treated here as controlling.

Continuities and incongruities show that this scheme is not thoroughly accurate. Thus, *Politics, Economics, and Welfare* (1953) is as much a summation of Dahl's previous work on American public policy as it is a bold effort to resolve all policy-making processes into four basic techniques of control. *Modern Political Analysis* (1963) appeared after *Who Governs?* (1961), but looks back to Dahl's earlier preoccupation with the general dimensions of political power. *Pluralist Democracy in the United States* was published one year later than *Political Oppositions in Western Democracies.* A record of organic scholarship is less amenable than Gaul to valid segmentation. Nevertheless, these subdivisions should provide a useful perspective on the evolution of Dahl's interests.

Democracy and Civic Competence

Dahl first attracted general scholarly notice with a subject to which he was never again to return. "The Science of Public Administration: Three Problems" (*SPA*, 1947) is very much a piece of its times. It addresses itself to the case several "theorists" of public administration had made out for their "science" over the previous two decades. Using awesomely complex diagrams, systematically numbered propositions, and sometimes even algebraic notation intimating quantifiability in principle, the leading writers in this field had presented their ideas in universalistic terms suggesting fusion of pure deductive science with practical prescription.[2] Although many were less interested in public administration than in other kinds of formal organization, their proclaimed achievements necessarily compelled the attention of all political scientists who aspired to introduce rigorous systematization into their own discipline.

Dahl's object was to show that these imposing claims rested on a fragile foundation. Writers in public administration could not expect to construct a pure science of administration, he contended, because "[S]cience as such is not concerned with the discovery or elucidation of normative values. . . . [S]cience cannot construct a bridge across the great gap from 'is' to 'ought'," whereas the appraisal of administrative design and performance inescapably entails some choice as to the ends an administrative arrangement is to serve.[3] Terms like "efficiency" and "responsibility" either are empty of content or contain unacknowledged assumptions regarding the ends to be served or ignored.[4] This point could only be overlooked by the proponents of "value-free principles" of administration because of their commitment to an empirically inadequate model of organizational man in which he appears as a "rational" creature who seeks only to maximize the goals of his organization.

Once it is recognized that participants in administration may seek a multiplicity of goals both inside and outside the formal organization of which they are members, the notion of an abstractly universal science of administration, wholly independent of the character of the organization or of the cultural and political setting in which its members operate, necessarily vanishes into thin air. Administrative science must therefore become comparative. It must rest on field research in different cultural and social settings, and it must recognize the diversity of human motivations. It must take into account the special constraints on human behavior in publicly accountable administrative organizations, and it must investigate the implications for administration of the different terms on which the political life of the various nations has been organized.[5]

---

[2] The *locus classicus* of these now gleefully belabored pretensions was, of course, the *Papers in the Science of Administration,* ed. L. Gulick and L. Urwick (New York Institute of Public Administration, 1937).

[3] *SPA*, pp. 1; 2–4.

[4] Cf. the similar – and, as a matter of historical record, the more memorable – formulation of this argument that same year in Herbert Simon, *Administrative Behavior* (New York: Macmillan Co., 1947), chap. 2.

These arguments mark Dahl's debut in the arena of scholarly controversy. It was an impressive debut. One notes already in this article the qualities for which he was later to be celebrated – among these, a sure sense for the truly fundamental issues in the field, and an ability to characterize other people's positions (and his own as well) concisely, carefully, and with great lucidity. Likewise evident is a keenly developed critical orientation that presses controversies up to a plane of logical generality at which the implications of the resolution become compelling for all members of the discipline.

It must also be said that in this article Dahl succumbed to one of the rare recorded fallacies in his long career. For on inspecting his conception of a "science" of administration, one realizes that he uncritically accepted his opponents' conception of administrative "science" as instrumental prescription, adding only that the values served should be made explicit. In thus confounding positive cause-and-effect propositions with "If . . . then" technological prescriptions, Dahl simply obscured the possibility that descriptive statements of the former order about administration can be formulated and tested. It may well be, as Gunnar Myrdal has argued for the last forty years, that most bodies of purportedly scientific doctrine in the social sciences reveal under critical inspection many opportunistically obscured value premises, and that the best safeguard against this tendency is to address oneself to explicitly formulated ideals.[6] But Dahl has argued otherwise on many occasions; and certainly it cannot be said that in this essay he self-consciously denied the possibility of a purely descriptive social science of administration. Indeed, it is all too clear he did not consider that alternative at all.

Whatever the epistemological niceties of this seemingly unresolvable debate, Dahl clearly adhered to his own prescription in his first full-length book.[7] *Congress and Foreign Policy* (*CFP;* 1950) is an unabashedly value-oriented treatise. Given, Dahl writes, that the policy-making processes of a democracy should facilitate civic agreement while resulting in "rational" decisions and the maintenance of "responsible" leadership, how well does Congress perform at present in relation to these ends in the foreign policy arena, and what are the prospects for improvement? Such questions obviously derive from a tradition of normatively self-conscious scholarship that was on the verge of being displaced by the "behavioral revolution." Even in their more general form these questions have seldom received a better answer.

Some purely verbal ambiguity attended two of Dahl's three key concepts at the outset. If "rational" policies are those "best designed to achieve the purposes agreed upon . . . that are, in other words, best calculated to satisfy, in the real world of international politics, the desires and aspirations underlying the agreement," then "rationality" would seem to have less to do with the state of mind of the decision makers – the clear focus of Dahl's subsequent chapters – than with the proven effectiveness of their choices. Policies might well prove retrospectively "rational" in this sense through a happy conjunction of outcomes brought about through a president's reliance on private astrological forecasts and outcomes he was publicly pledged to seek. How rationality can aid in establishing a proper trade-off among the policy objectives of different constituencies is also unclear. The term "responsible leadership" invites many alternative constructions, of which Dahl adopted one – "that it selects policies based upon the preferences of the citizen" – without further argument.[8] And no one has shown more clearly than Dahl himself in his subsequent work how many logical problems a strict constructionist would confront in seeking to make such a selection.

---

[5] *SPA*, pp. 5–11.

[6] See, e.g., his *Objectivity in Social Research* (New York: Pantheon Books, 1969) and the reference to his other works cited therein.

[7] In the intervening three years, two of Dahl's least influential articles appeared in the professional journals of the period. "Worker Control of Industry," *American Political Science Review,* 41 (October, 1947), 875–900, is essentially a summary of the debate within the postwar British Labour Party concerning the institutional implications of its commitment to increase worker participation in the management of industry. "Marxism and Free Parties," *Journal of Politics,* 10 (Nov., 1948), 787–813, is a somewhat more ambitious enterprise. Nowhere else has Dahl assessed in print at such length the adequacy of Marxism as a theory of politics. The relative obscurity of the article is presumably attributable to Dahl's more extensive development in his later work of its central ideas on majority rule and the role of parties in political conflict.

[8] For Dahl's formal definitions of his three value concepts, see *CFP,* pp. 6–7. In effect, he sided with Herman Finer in the latter's celebrated debate with Carl Friedrich on the concept of "responsibility." See C. Friedrich, "Public Policy and the Nature of Administrative Responsibility," *Public Policy, 1940* (Cambridge, Mass.: Harvard University Press, 1940), pp. 3–24; and the rejoinder by Herman Finer, "Administrative Responsibility in Democratic Government," 1 *Public Administration Review,* (Summer, 1941), 335–350.

Once under way, however, Dahl removes all doubt as to his meaning. The stability – indeed, the survival – of democracy in America is threatened, he argues, by a rapidly widening disjunction. On the one hand, the level of informed competence regarding foreign affairs so far attained by the American citizens at large and all but a handful of their representatives in Congress remains alarmingly modest. On the other, small groups of decision makers (largely centered in the executive branch) have accumulated a quite overwhelming expertise based on their unity, specialized training, intensely focused interest, and access to pertinent and sometimes secret information. This distinctive competence has enabled these policy elites to make plausible their claim that they alone are equipped to assess and respond to the crises arising from America's newly assumed global responsibilities in an era of potential nuclear war.

From this perspective, Dahl's three value criteria become intimately related, with "rationality" the strategic factor. Unless civic and congressional competence in foreign affairs can be drastically upgraded, Dahl contends, congressional efforts (and those of the electorate) to hold foreign policy experts accountable for their decisions can only be ineffectual or disastrously misguided. Yet Congressional insistence on detailed accountability, however unfortunate its effects, must be expected as long as a gap in shared understanding of foreign policy problems impedes the development of mutual agreement on goals and a consequent willingness on the part of Congress to entrust the executive branch with the discretion necessary for their implementation. In the absence of such agreement, congressional abandonment of this claim would signal the ebbing vitality of the American democratic system.

Unhappily, Dahl finds good reason to argue that neither the general circumstances of the electorate in a mass democracy nor the specific policy processes of the American form of government hold out much promise of overcoming the estrangement of the president and his foreign policy experts from the other politically accountable decision makers of the system. The round of life of the citizen does not encourage the accumulation of reliable, low-cost, pertinent information on foreign affairs; neither do the qualities required for success in American electoral politics. The constitutionally prescribed separation of powers builds in a fundamental tension between the legislative and executive branches of the national government that the weak party system rarely bridges. Dahl's prognosis is gloomy indeed: since little will result from efforts to raise the citizens' level of informed participation in foreign affairs, the prospect is one of increasing drift toward unfettered presidential discretion and the resultant growth of a "plebiscitary dictatorship" in the foreign policy arena.[9]

Even this sketchy outline of an intricate, richly elaborated thesis should be sufficient to show the larger dimensions of Dahl's treatise. Although his immediate subject is the role of Congress in the foreign policy process, he uses his analysis to bring into focus the more general question of a democracy's capacity to cope with challenges demanding swift, united responses based on specialized knowledge in an atmosphere of secrecy and potential emergency. Moreover, the discussion is as rewarding in its parts as in the whole. Its pages abound with analytic observations clearly foreshadowing major arguments in his later works. Thus, for example, he already points out here that "majorities" in a pluralistic society are largely made up of minorities; and that in democratic societies the range of discretion tends to decrease as the range of knowledge increases.[10] Many of his theses retain a disturbing contemporary relevance. Readers in the 'seventies will find no reason to dismiss his warnings against permitting presidents to seek triumphs of secret diplomacy unencumbered by a sense of responsibility for enlightening Congress and securing its comprehending assent to the ends pursued. With memories of Vietnam still fresh, we can perceive a painfully deepened force to his plea that, in the long run, democratic theory does not offer sanction "for a majority of fifty-one per cent to ask a minority of forty-nine per cent to offer up their lives to support a foreign policy with which they, the members of the minority, fundamentally disagree."[11] And those who have become accustomed to think of Dahl as an uncritical apologist for American pluralism might be startled to encounter the following passage:

> [C]ompetent choice on questions [of foreign policy] requires a preliminary process of discussion and inquiry, not only to clarify reality, but to discover what one's preferences are in that context of reality and what, if anything, may be done to realize those preferences in the real world . . . [But] Modern urban life is not conducive to the kind of intermediate organizations implied by, say, the neighborhood discussion group. Individual hostility, anonymity, and

---

[9] *CFP*, pp. 63–65, 263–264.

[10] *Ibid.*, pp. 55, 142.

[11] *Ibid.*, pp. 180–181, 223.

> moral isolation; the physical layout of the city; the bothersome aspects of transportation; the lack of facilities for group life; the intensified satisfactions of passive participation, exploited by highly organized and influential commercial and advertising groups; the centralization of control over techniques of influencing attitudes and the consequent standardization and banality of opinion – all militate against the spontaneous growth of local discussion and inquiry groups.[12]

Indeed, perhaps the most enduringly interesting theme within *Congress and Foreign Policy* is Dahl's concern at this point with improving the quality of democratic participation. Again and again, in tones reminiscent of John Stuart Mill and John Dewey, he deplores the extent to which the outcomes of political activity in America represent manipulative bargains among self-interested elites rather than the fruit of an informed sense of collective purpose. The process by which agreement is reached appears to him quite as important as its substance. "[I]t is one thing to arrive at policy by a process that searches out those common purposes on which various and often conflicting minorities may nevertheless agree," he wrote, but "... quite another to make policy by patchwork-compromising that concedes everything to the special claims of an articulate minority, and nothing to the preferences of an unmobilized and inarticulate majority."[13] It is this perspective, as much as anything, that leaves one feeling that Dahl's first book is still one of his most satisfying.

The implications of secrecy and technical expertise for competent popular participation and effective control of bureaucratic activity by democratically elected bodies were obviously very much on Dahl's mind at this time. For the following year, acting on a commission from the Social Science Research Council, he co-authored with Ralph S. Brown, Jr. a pamphlet on possible subjects for research on the *Domestic Control of Atomic Energy* (1951). Once again one encounters the themes made familiar in his previous work – the difficulties in persuading an apathetic public to take an intelligently informed interest in collectively consequential matters of little immediate benefit or loss to any one citizen; the problems of lay supervision of experts; the deadening effect of security regulations on public debate, and the consequent possibility of ignorant or irrational public reaction to technically rational policy decisions. These are themes, as Dahl had shown, that can be linked to the most fundamental and searching questions about the prospects for democracy. They do not receive that treatment here. Partly the problem lies in the format. The pamphlet is, in effect, a 110-page exhortation to other political scientists to undertake research the authors find interesting but do not propose to undertake themselves. The inconclusive hypotheses, the many questions without answers, do not make stimulating reading. But there is also an extreme unevenness of treatment, with many thin chapters and a concluding section on "general theory" that is little more than a recapitulation. It is all too plain that this time Dahl did not rise to the potential of his subject. In sharp contrast to his preceding work, *Domestic Control of Atomic Energy* does not reward the reader who returns to it.[14]

The Logic of Democracy

Shortly after this final foray in the field of substantive public policy, Dahl's published work underwent a dramatic change of character. Hitherto, despite the markedly analytic quality of his arguments, Dahl had kept his specific subjects in firm focus at a relatively short range. Now, however, he apparently felt ready to make explicit, and to organize into sustained theses, the general concepts and theoretical propositions implicit in these arguments. The result was an outpouring of essays and books that firmly established him among the very front rank of political scientists.[15]

Politics, Economics, and Welfare (*PEW*; 1953), the first of these publications, is in some respects the most creative book Dahl ever wrote.[16] In this book he and his economist collaborator Charles Lindblom addressed themselves uncompromisingly to the most general properties and problems of rational social action. These they found to be embedded in the techniques of calculation (including both the scheduling of goals and the processing of information pertinent to the selection of appropriate strategies) and in the social mechanisms relied on to control behavior in ways required to implement the resulting decisions.

---

[12] *Ibid.*, pp. 88, 90.

[13] *Ibid.*, p. 51.

[14] A somewhat similar problem beset an article by Dahl that appeared some eight years later in this *Review* on the possibilities for research on the interconnections between business firms and politics. The individual points are generally well made; but most had already been made better in actual research reports, and without exceptional effort one can think of other important questions that might well have been included. This particular genre of essay seems to be falling out of favor. On the evidence provided by even so able a social scientist as Dahl, its demise does not seem indisputably regrettable. See Dahl, "Business and Politics: An Appraisal of Political Science," *American Political Science Review*, 53 (March, 1959), 1–34.

[15] This change of character was seemingly quite evident to his readership. Among the ten recorded reviews of *Congress and Foreign Policy*, nine were published in the popular press. Henceforward, although Dahl's work has been largely lost to sight for the casual lay reader, it has regularly received widespread coverage not only in the political science journals but in the book review sections of the standard sociology and economics journals as well.

In one of the boldest and potentially most fruitful simplifications of the decade, the two authors contended that calculation and its accompanying controls formed the central core of most organized social interaction. Furthermore, they showed that activities directed toward carrying out these two operations could be resolved into one or more of four elemental control mechanisms – market pricing, bargaining, hierarchy, and the institutionalized form of competition among leaders for needed support from nonleaders that Dahl and Lindblom called "polyarchy." Having drawn attention to structural similarities among control processes conventionally discussed in nominalistic terms, they undertook repeated comparisons of the four control techniques in an imposing variety of specific policy contexts. Conversely, they demonstrated the sterility of many contemporary controversies that failed to disaggregate such hypostasized compounds as "bureaucracy" or "planning" into their component control techniques.[17]

Looking back through the political science – or, for that matter, the social sciences generally of this period – one can find few analytic schemata as comprehensive in scope, as systematically elaborated in detail, and as searchingly applied to societal processes. Yet many of Dahl's later works – works less adventurous in conception, less general in their application – were to attract far more sustained attention than this one. It may therefore be instructive to ask why *Politics, Economics, and Welfare* was seemingly more often footnoted than closely read within the discipline, and why it has now been steadily dropping out of the footnotes.

One reason may simply be that it was too original for its time. Reviewers obviously had difficulty in classifying the work and in relating it to other studies. Everyone could see that it stood at the interface of economics and political science. But in precisely what sense was it interdisciplinary? Many economists judged it harshly, correctly contending that it made no direct contribution whatever to the development of positive economic theory. Sociologists and political scientists were generally more respectful but were unable to see in it either a report on specific findings or a satisfactory explanation for such findings.[18] Like Talcott Parsons's and Neil Smelser's *Economy and Society* – with which, in my opinion, *Politics, Economics, and Welfare* would repay close comparison – it offered numerous stimulating metatheoretical perspectives without pointing directly to any particular line of research.[19]

It may well be that the authors themselves did not clearly understand what they had accomplished. There is some evidence to support this suggestion. Despite their extreme concern for establishing systematic linkages among their concepts, they never really show how their four central sociopolitical processes (or techniques – the designation remains unstable) might be arranged along an explicit set of dimensions. Perhaps this omission is attributable to the nonanalytic origins of their terms. The concepts of market pricing, hierarchy, polyarchy, and bargaining appear in fact to be drawn from descriptions of the dominant decisional practices within four substantive societal arenas – the conventionally conceived economic marketplace (i.e., the competitive or semicompetitive private sector of capitalist systems and the semi-autonomous productive sector of such decentralized socialist systems as the Yugoslavian); the internal organizational arena of monopolies and administrative agencies; the arena of competitive electoral politics in large-scale modern democracies; and the intragovernmental arena of lobbies, parliamentary committees, and executive elites (with, as an economic analogue, the relations prevailing among trade unions, oligopolistic producers, and monopsonies). But Dahl and Lindblom are quite insistent that their scheme is analytic, not merely descriptive. Quite correctly, they argue that combinations of these techniques are to be found to some extent in all of these arenas.

---

[16] *PEW* was coauthored with Charles E. Lindblom of the Yale Economics Department. According to the Preface: "Many works appear as 'collaborations,' but in few can the dialectical process – in the Greek, not in the Marxist, sense – have been more heavily relied on. As much as any work by two people can do, each chapter represents the single product of two minds" (p. xxiii). I have taken the authors at their word, and shall make no effort here to detect and disentangle their individual contributions. For present purposes, this decision entails ascribing to Dahl whatever ideas appear in the book. Those interested in assessing Lindblom's contributions might wish to consult his subsequent essays on bargaining and decisional strategy, and most particularly *The Intelligence of Democracy* (New York: Macmillan Co., 1965), although the danger of unwarranted inference is real.

[17] See, e.g., the instructive series of charts in *PEW*, pp. 10–17, on the scope of governmental economic controls.

[18] Interestingly enough, one of the most favorable reviewers was C. Wright Mills, who found in the constructive utopian potential of the book's discussion and appraisal of alternative techniques a welcome break from the fact-bound descriptiveness of *status-quo* political science. See his review in the *American Sociological Review*, 19 (August, 1954), 495–496.

[19] Cf. Talcott Parsons and Neil Smelser, *Economy and Society* (Glencoe, Ill.: The Free Press, 1956). Unfortunately, Parsons has never addressed himself as directly to Dahl and Lindblom's scheme as he has to the work of several other contemporary political scientists.

Alternatively, their scheme might be viewed as a characterization of the terms on which decisions are made in allocative systems having structural properties that range from those of competitive markets (the pricing system) through oligopoly (polyarchy) to monopoly (hierarchy). Arranged this way, the three techniques so cited would be graded according to the "vertical" control relationships prevailing between "producers" and "consumers"; but then bargaining could then only be fitted into the scheme as the "horizontal" control dimension of systems structurally similar to polyarchies.[20] Again, market pricing, bargaining, and hierarchical allocation of resources would seem to occur continuously, whereas the polyarchal component of government appears confined to the selection of leaders and perhaps to the scheduling of goals. With some ingenuity, no doubt, a reviewer could fit together these various processes into something like a systematic fourfold table. The significant fact remains that the authors did not do so themselves.[21]

Compounding these conceptual problems is one of exposition. *Politics, Economics, and Welfare* is a book that generates an enormous number of cells the authors feel compelled to fill. Sometimes the results are dazzling. The two short chapters on hierarchy bear favorable comparison with Max Weber's celebrated passages on bureaucracy. The comparison of collective with market choice deftly disposes of a great body of fallacious reasoning on the subject, while the subsection on aids to rational calculation truly transforms the reader's analytic perception of some thoroughly familiar political techniques.[22] On the other hand, the discussion of social economizing and the pricing system is simply stupefying for anyone who has been exposed to the elements of economic theory, while the obligatory condensed treatment of "freedom" leaves the conviction that the authors should have said either much more or much less.[23] The most determined reader's mind clots somewhere amid the many long, grey, embarrassingly banal passages on contemporary governmental activity, particularly in the unsatisfactory sections on bargaining. The limp postscript does disservice to the authors' achievement. *Politics, Economics, and Welfare,* one concludes, is a good two- to three-hundred pages too long. The pity of it all is that these superfluous pages so inextricably interlard some of the most brilliant paragraphs in the social sciences as to deter the scholar from reading any at all.

These blemishes were notably absent from Dahl's next book, *A Preface to Democratic Theory* (*PDT;* 1956), one of the leanest works in the history of American political science. Dahl has never written another book as rigorous, as condensed, as foundational, and as pellucid in style as this one. In barely 150 pages he moves assuredly through the central controversies that must arise in any serious, close discussion of democratic theory, crisply indicating not only how one might resolve these questions but even more how they should be formulated so as to admit of orderly and conclusive answers. The treatise attracted much attention at the time as one of the first in political science to make extensive use of the notational devices of symbolic logic. Yet Dahl does not gloss over conceptual ambiguities in the interest of formalization; on the contrary, he elucidates them. In explicating the choices one must make in order to convert loose verbal statements into the demandingly precise propositions of formal logic, he serves as a teacher of the highest order.

Understandably, but somewhat unfortunately, the attention of most critics quickly settled on the opening chapter of the book. Here Dahl set out to show that James Madison, often praised as a polemicist of matchless lucidity, in truth was censurably careless in both definition and construction of argument. On his own grounds, Dahl was indisputably successful. Lucidly and conclusively, he showed that Madison begged crucial questions in his use of "faction" and "majority tyranny"; and that when they are not begged, Madison's most celebrated arguments became substantially less determinate than admiring commentators had previously perceived. And this perception is not trivial. "Madisonian ideology," Dahl contended, "is likely to remain the most prevalent and deeply rooted of all the styles of thought that might properly be labeled 'American'."[24] Since Madison's persuasive definitions have been operationalized to sanctify patterns of authority and procedural rules at odds with many powerful alternative conceptions of democracy, his ambiguities have clearly invited opportunistic exploitation.

---

[20] Moreover, Dahl and Lindblom specifically assert that in their scheme the pricing system includes far more than purely competitive pricing practices. See *PEW*, pp. 193 ff.

[21] One consequence of not working out explicit dimensions in *PEW* for the four politico-economic techniques is that they are certainly not exhaustive and possibly not exclusive. To judge from the chart on p. 193, and the discussion elsewhere, "pricing" appears to contain elements of both bargaining and hierarchy. And among the various control techniques not considered in the scheme, the control induced by appeal to previously accepted standards – as, for example, in much judicial decision making – is a particularly conspicuous omission. As a general rule, but without systematic critical defense of the position, Dahl has been inclined to discount heavily the efficacy of this particular form of control.

[22] *Ibid.*, pp. 227–271; 413–438; 64–88.

[23] *Ibid.*, pp. 129–226; 28–38.

Many readers, nevertheless, have been left with the unsettling sensation that a formidable logical apparatus has been brought to bear against its target with a power disproportionate to the demands of the subject matter. Dahl's critique springs out of a philosophical tradition, and is directed at a contemporary audience, that in principle profess to take nothing for granted. But Madison's world was a very different one. His argument becomes much more defensible as practical ideology, if not in ultimate philosophical principle, if one allows that he was writing for an eighteenth-century post-Revolutionary War public to which such terms as "natural rights" and "tyranny" (the severe deprivation of "natural rights") had a reasonably operational shared meaning. Because one cannot specify the scale on which tyranny shades into imposition of unwelcome policies, the former concept does not thereby necessarily become meaningless.

In other ways, Dahl presses his critique too hard. He persuasively argues that any arrangement requiring the assent of more than simple majorities reciprocally invites tyranny by minorities; but he is (characteristically) rather too inclined to discuss the issue in terms of veto power, and too little disposed to give weight to the educative value of the dialogues that may arise when constitutional checks can be invoked to retard instant alteration of basic rules.[25] He is also not above treating as a serious definitional proposal Madison's rhetorical declamation to the effect that "the accumulation of all powers, legislative, executive, and judiciary, in the same hands . . . may justly be pronounced the very definition of tyranny."[26] Again, he converts Madison's belief that governmental systems must guard against the possibility of tyranny – an outcome that Madison (unlike Hamilton) seems to have regarded as of relatively low probability – into a flat hypothesis that any given individual or group of individuals *will* tyrannize over others if unrestrained by external checks.[27] So construed and scrutinized, Madison's thesis quickly becomes more of an intellectual shambles than a more sympathetic reading might suggest.

Perhaps because the rest of the book deals with no specific treatise, its argument seems less captious, more truly elucidative. One chapter offers, and criticizes, a maximizing model for populist democracy; the remaining three seek to state the necessary and sufficient institutions for maintaining an operative polyarchy.[28] These latter chapters are of particular interest because they contain, in essence, the propositions that Dahl was later to expand into a full-scale descriptive treatment of American pluralist democracy. Their object, overall, is to establish the overriding importance of elections (as against constitutional rules and the division of governmental authority) as the crucial institution for keeping governmental elites responsive to the preferences of the governed. He is equally bent, however, on demonstrating the logical invalidity of elections as indicators of majority preference, and on showing further that the principle of majority rule in elections promotes democratic stability only when the preferences of a society are distributed in one of a certain limited number of patterns. This thesis in turn leads to a close and careful discussion of the problem of measuring and comparing intensities of preference, and of accommodating the conclusions in a general theory of democratic rule.[29] Since, Dahl argues, there is ultimately no wholly satisfactory resolution of this problem, he resolutely avoids a utopian tenor in his brief final appraisal of the American polyarchal system: it has not guaranteed all citizens an equal voice in the government, but it has enabled most active and "legitimate" groups to make themselves effectively heard at some crucial stage in the decision process; it has not headed off slavery, civil war, two world wars, or numerous periods of economic instability, but it has encouraged agreement, moderation, and a reasonable degree of social peace in a powerful, diversified, and incredibly complex society.[30]

---

[24] *PDT*, pp. 30–31.

[25] *Ibid.*, pp. 27–29. On p. 50, in discussing the problem of intensity, Dahl does indirectly acknowledge this argument.

[26] *Ibid.*, pp. 6, 12. On p. 6, Dahl promptly reformulates the proposal through the insertion of the empirical premises that are clearly implied; nevertheless, on p. 12 he again seriously entertains the possibility that Madison was solving his argument by definition before once again dismissing the point. Madison's quotation is drawn from *The Federalist*, No. 47.

[27] *Ibid.*, pp. 6, 8.

[28] Strictly speaking, "polyarchy" is at most, as we have seen, only one processual component among many in the actual operation of a concrete governmental system, but Dahl here extends the term to mean any government characterized by the selection of its effective leaders through freely competitive elections. For his formal definition of "polyarchy" in this sense, see *PDT*, pp. 84–85.

By almost any standard, *A Preface to Democratic Theory* is a more conservative work than *Politics, Economics, and Welfare.* Its scope is less general; its classifications are less ambitiously simplified and systematized; its careful, precise argument is directed to more familiar themes. At the same time, it constituted a decisive step forward for Dahl in the direction he was actually to pursue over the next ten years. For the first time one sees clearly in this book how he intends to adapt the economists' models of man to political analysis. Given certain distributions of preferences and political resources within a population, he will ask how these patterns will affect probable policy outcomes, and to what extent these outcomes can or will be affected by such factors as information, interest, organization, access, and rules. By attributing rational self-interest to a high proportion of the key participants, he is able to *explain* rather than merely to *describe* outcomes; his correlations are used to test propositions rather than to generate them.

This approach produces costs as well as gains. Social structure often appears as little more than an aggregate of the traits of individual actors. Dahl shows hereafter less interest in how attitudes are acquired and modified through participation, more disposition to discuss politics as a process by which individuals seek to control others in order to realize a fixed set of preferences. Since Dahl has never discussed the general aspects of his metatheoretical approach, one cannot be sure how self-consciously he paid that price. One can only be sure that he paid it.[31]

The Concept of Power

Once Dahl had settled on a general paradigm of inquiry, it was a natural next step for him to attempt to clarify the ambiguities of its central term. His major work for the next several years may be read as a recurrent attempt to unveil the mysteries of power.[32] It is not too much to say that, in purely expository terms, no other political scientist succeeded more completely. No one has written with more perfect clarity on this most protean of political concepts, and no one has inventoried more concisely and carefully its varied dimensions. If even Dahl's most finished formulations leave one ultimately dissatisfied, the dissatisfaction arises mainly in reaction to his conviction that what cannot be stated clearly should not be stated at all.

So sophisticated and so tautly drawn have been Dahl's essays on power, and so extensive has been the critical literature they have engendered, that a review of this compass cannot do justice to even the most central points. At most, one can sketch in broadest outline Dahl's most distinctive contentions. These, in turn, may prove sufficient to make comprehensible a severely limited appraisal at a high level of generality.

---

[29] *Ibid.*, Chap. 4.

[30] *Ibid.*, pp. 145–151.

[31] One illustration of both the costs and gains of this approach may be found in Dahl's discussion of the U.S. Supreme Court as a protector of "minority rights." His first formulation of the issue appeared in *PDT*, pp. 105–112; later he expanded the argument with additional data in "Decision-Making in a Democracy: The Supreme Court as a National Policy-Maker," *Journal of Public Law,* 6 (June, 1958), 279–295. In these articles he showed that, contrary to the nonempirical supposition of most legal scholars, the Court has not in fact effectively protected minorities from "majority tyranny," but instead has followed the election returns – at least in most instances involving an intense and persistent congressional majority.

This finding certainly does call into question many fundamental propositions about the role of the Court within the American polity. At the same time, the leading inference is by no means conclusive. Dahl's approach tends to convert judicial decision making (as he himself quite openly acknowledges) too much into terms of "who gets what?", with minority appeals being treated as if they were merely for discriminate benefits for that limited group alone. Leaving purely legal considerations aside, one gets little indication in such an approach of the Court's role as a teacher, and of the importance not only of the result in the instant case but of the terms in which the result is framed and justified.

[32] His major conceptual essays on power from this period include "The Concept of Power" (*CP;* 1957); "A Critique of the Ruling Elite Model" (*CREM;* 1958); *Modern Political Analysis (MPA)* Chaps. 5–6; and the article on "Power" in the *International Encyclopedia of the Social Sciences,* Vol. 12 (New York: Macmillan Co., 1968), pp. 405–415. Although the *I.E.S.S.* article must be taken as Dahl's most recent (and therefore presumably most authoritative) statement of his views, it is by its nature rather more a survey of the literature than a detailed exposition of his own formulations. I have therefore drawn most heavily in the following discussion in the chapters in *MPA,* which, though less technical than the early *CP* article, are also in some respects much clearer. Dahl's "Critique" raises highly important but somewhat special issues that can best be discussed together with *Who Governs?,* his major treatise in empirical power analysis.

At the core of all Dahl's writings on power is the insistence that power is not an attribute but a relation. This contention was not at all original with him. What is distinctive is his uncompromising development of its implications. For Dahl, "power," "influence," and "control" appear as interchangeable terms: all have in common the attribution of causal efficacy to a specified actor or set of actors in relation to other specified sets of actors.[33] In particular, we may say that "A influences B to the extent that he gets B to do something that B would not otherwise do."[34] Influence (or "power"), then, is an *active* concept; it is not a *condition*, not a structure, but a demonstrably asymmetrical causal connection among actors, the activation of which alone suffices to explain the observed changes of behavior in which we are interested. Like Herbert Simon before him, Dahl therefore argues that when conditions preclude observation of acts of power (or attempts to exercise power) – whether because of complete consonance among the objectives of all relevant actors, or because of perfect anticipation and adjustment by the presumptively less powerful – we cannot distinguish true from falsely attributed power relationships.[35] By their acts alone shall we know the powerful.

Insistence on the active, relational nature of power led Dahl directly to several distinctions of crucial importance. In the first place, he argued (as Simon had done before him) that the exercise of "power" must not be confused with its possible bases – wealth, military might, information, or whatever. That the wealthy are powerful is an interesting, testable, empirical proposition that should be regarded as probable but falsifiable in principle, not tautologically true.[36] The amount of resources available to an actor may prove, at the least, a significant constraint on the power he is able to exercise, and it may serve as a useful index of his power potential. But no reliable conversion rate between nonpolitical resources and political power has yet been worked out (not all elections can be bought), and the effectiveness even of purely political resources remains a function of the actor's interest, skill, and morale (as the history of presidential campaigns has repeatedly illustrated). Hence an actor's actual power must be sharply distinguished from his estimated *potential* power at the moment, and both of these from estimates of his probable future power.[37] Many of Dahl's most prolonged controversies with his critics turned on the difference between his documentation of the power an actor had actually exercised and their estimates of the potential power such actors possessed. Without slighting the importance of the latter enterprise, I am driven to remark that it is not the former, and that – given the numerous possibilities for slippage Dahl has outlined – it is necessarily a far more speculative undertaking.

Dahl asserts, moreover, that if "power" is a relational concept, it cannot logically be treated as a homogeneous quantum. Any comparative assessment of the power of two actors requires, as a preliminary condition, careful specification of the dimensions along which one proposes to compare their ability to control other actors. Dahl suggests five such dimensions: the number of people controlled, the extent of behavioral modification induced, the resistance (subjective costs of compliance) overcome, the probability of compliance, and (perhaps most importantly) the scope of the acts with respect to which control can be exerted.[38] Whether this list exhausts the range of relevant dimensions of comparison is less consequential than Dahl's underlying point: that the statement "A is more powerful than B" is logically vacuous until one has gone on to say "with respect to X." How, Dahl pertinently inquires, can one validly compare a Senate majority leader's success in changing the votes of five southern senators on a major civil rights bill with a president's ability to elicit one hundred thousand letters of support for his latest initiative in nuclear arms control? Unless one can devise a single persuasive formula incorporating the number of people influenced into a comparison of resistance overcome regarding policy issues with very different historical and affective dimensions, one is left in the unsatisfactory position of assigning addivity to the dissimilar properties of apples and oranges. Too many attributions of power, Dahl argues, attempt that task.[39]

---

[33] In *CP*, p. 203, Dahl explicitly eschews the language of causality out of deference to Hume. But since the rest of his argument makes no sense without the attribution of asymmetry to relations among people, I shall reintroduce it.

[34] *MPA*, p. 40. Although in *CP* Dahl declined to distinguish between "power" and "influence" (p. 202), he found reason to do so by the time he wrote *MPA*. However, the distinction does not turn, as in ordinary language, on the question of intent: when A is causally responsible for altering B's behavior in ways unknown to – and immaterial to – A, or when B's causally evoked response is different from and/or contrary to A's intent, Dahl prefers to use the unilinear scalar language of "negative" influence (or – presumably – zero influence; Dahl does not really deal adequately with unintended causal relations or those to which the actors involved are indifferent) rather than qualitatively distinguishing such linkages from acts of "power" as acts of "influence."

[35] *CP*, pp. 203–204. Cf. the more elaborate discussion of these problems by Herbert Simon, "Notes on the Observation and Measurement of Power," *Journal of Politics* 15 (August, 1953), 500–516.

[36] *CP*, p. 203. Cf. Simon, "Notes," p. 501.

[37] *MPA*, pp. 47–49.

[38] *MPA*, pp. 41–47.

If we accept that power is best thought of as a general control relationship among actors, one that involves a specified actor's ability to change the behavior of other actors, then Dahl's analysis is as lucid a discussion of the phenomenon as we shall find in the literature. The most telling and suggestive criticisms have therefore been those that challenge the very terms of Dahl's formulation. Some critics, for example, have pointed to Dahl's failure to elaborate the implications of the interdependent nature of control relationships.[40] Power, they note, can be asymmetrically ascribed to one actor in relation to other actors only because we have made certain assumptions about the context in which the act occurs. To treat an initiative by A as a *sufficient* condition for a determinate reaction by B is to deflect attention (as John Stuart Mill pointed out more than a century ago) from the many other conditions that must be present before the particular act in question will have the stated result.

Dahl would doubtless concede the general point quickly enough. What he does not do is to focus systematically on these "other" conditions. These range from (at the most proximate level) the immediate determinants of B's reaction to A's efforts to change his (B's) course of action to (at a hierarchically more general level of control) the complex set of cooperative arrangements that provide the social context within which A's initiative eventuates in a modification of B's behavior.[41] Yet a concern with the emergent properties of these interdependent contexts of agreement and collaboration is precisely what distinguishes the subject matter of the political scientist from that of the student of interpersonal relations.[42] To compare President Johnson's and President Nixon's capacity to control an individual airman's decision to release bombs over Cambodia seems less fruitful and more misleadingly formulated than to inquire into the political structure that implicates a president and an enlisted man of whom he is totally unaware in a relationship that eventuates in such an outcome. Viewed in this light, Dahl's treatment of *political* power as merely a sectorally specialized case of a general relationship between Actors A and B becomes much more than an exercise in logical clarification. It represents a theoretical stance toward politics – and a dubiously simplified theoretical stance – as well.

The alternative theoretical consequences of explicitly considering the emergent properties of power in political systems are most completely illustrated in Talcott Parsons's essay on the subject.[43] Parsons's subtle and inordinately complex argument defies summary; here we can only consider his direct criticisms of Dahl. Parsons locates Dahl's treatment of power in a tradition reaching back to Hobbes insofar as it conceptualizes the phenomenon "as simply the generalized capacity to attain ends or goals in social relations, independently of the media employed or of the status of 'authorization' to make decisions or impose obligations."[44] Closely allied with this tradition, though not logically derivative from it, is the tendency to discuss power in zero-sum terms as an interpersonal relationship of dominance and submission. Parsons offers as an alternative an elaborate analytic paradigm in which power is conceptualized, on an analogy with money in the economic system, as a facilitative medium within a defined network of political relationships. This system-specific medium he defines as the "generalized capacity to secure the performance of binding obligations by units in a system of collective organization when the obligations are legitimized with reference to their bearing on collective goals and where in case of recalcitrance there is a presumption of enforcement by negative situational sanctions."[45] A particularly important consequence of this conception is that it permits Parsons to discuss intelligibly, as Dahl cannot do within the terms of his paradigm, the phenomenon of power "inflation" (e.g., the overcommitments of governmental capacity by 1966 in President Johnson's Great Society Program) and of power "deflation" (e.g., the paralyzing distrust induced by McCarthyism) at the macro-systemic level.[46] So defined, "power" unquestionably becomes a more elusive phenomenon than in Dahl's straightforward vector diagrams. In this instance, however, it may be argued that Dahl achieved lucidity at too high a price.

---

[39] To state the problem is not, of course, to preclude solutions. One might conceivably link degree of change induced to resistance overcome. Dahl's point is that one must state control relationships in ways that make clear the need for, and logical foundations of, such solutions.

[40] A notably lucid short statement of the following argument may be found in R. Harrison Wagner, "The Concept of Power and the Study of Politics," in *Political Power: A Reader in Theory and Research,* Roderick Bell, D. V. Edwards, and R. H. Wagner, eds. (New York: The Free Press, 1969), pp. 3–12.

[41] It is true that Dahl lists the "subjective psychological costs of compliance" as one of the five dimensions along which to compare influence (*MPA*, 43–44). But the few accompanying paragraphs in no way suggest the perspective outlined above.

[42] An "actor," of course, may be a corporation or a nation-state as well as a concrete person. What is at issue is not the nature of the concrete entity but the terms in which *relationships* are conceptualized.

[43] Talcott Parsons, "On the Concept of Political Power," *Proceedings of the American Philosophical Society*, 107 (June, 1963), 232–262. This essay has been reprinted in Talcott Parsons, *Sociological Theory and Modern Society* (New York: The Free Press, 1967), pp. 297–354, and in Bell, Edwards, and Wagner, pp. 251–284. I have used the page numbers of the latter edition.

[44] Parsons, p. 251.

Political Power and American Society

After several years of unremittingly conceptual writing, Dahl felt the need to test the utility of his leading theses in empirical research.[47] The outcome was the two books – *Who Governs?* (*WG;* 1961) and *Pluralist Democracy in the United States* (*PDUS;* 1967) – which (together with his slender introductory text, *Modern Political Analysis,* 1963) are probably the titles other political scientists most readily associate with his name.[48] *Who Governs?*, indeed, has generated so large a polemical crossfire that less knowledgeable readers might be forgiven for taking it to represent Dahl's primary – perhaps his exclusive – contribution to the discipline. Any review of the totality of his writings cannot fail to suggest in the most forceful terms how grave a misperception this belief entails.

From the moment of its publication, *Who Governs?* elicited stronger reactions than any of Dahl's other works. These reactions fell into two reasonably well-defined phases. At first, the reception was overwhelmingly favorable. It is easy to see why. Dahl's chosen approach to his subject committed him (and his research team) to reconstructing in great detail a set of critical decisions in several different issue areas. By adopting this approach, he was able to accumulate evidence bearing directly on the demonstrated ability of the participants in each of these areas to initiate proposals later adopted as actual policy or to successfully block proposals initiated by others. The solid factual base of the research was illuminated, moreover, by explanatory propositions and conceptual distinctions formulated in sophisticatedly universal terms with Dahl's customary linguistic precision. Ingeniously arranged tables and graphs supplemented the verbal arguments, and the study contained several other features not normally found in studies of local politics – among them, an historical chapter that illustrated a critical thesis rather than merely serving as inert "background"; disquisitions on such topics of general interest as citizenship without politics and the nature of *homo politicus;* and an ambitious concluding chapter on stability and change in the American democratic creed. Only later, as a second generation of critics began to contemplate the possible ideological implications of these conclusions and then the methods by which they were reached, did *Who Governs?* begin to attract censure as a primary exhibit in the apologetic literature on American pluralist democracy.[49]

That *Who Governs?* should have had so striking an impact on the discipline of political science is of some significance in itself. As the subtitle ("Democracy and Power in an American City") indicates, the book is a report on research conducted in the single city of New Haven, Connecticut. On its face, the specific findings of the study would therefore seem to have rather limited significance for those not deeply engrossed by the politics of New Haven. But since quite the opposite has proved true, there is perhaps some excuse for underlining the point. New Haven is not Floyd Hunter's Atlanta, nor is it the Lynds' Muncie, Indiana – let alone the United States of America. Criticism based on the supposition that it somehow stands for these other locales as well and that its empirical findings must be reconciled with conclusions drawn from studies of such places would seem misplaced, to say the least. The urgency of such efforts seems predicated on movement from the questionable empirical generalization that governments everywhere are only a front for overtly nonpolitical economic interests to the conclusion that any given study of government must be judged by the extent to which it unmasks such connections. Those not committed to that syllogism need experience no difficulty in entertaining the possibility that, whatever may be true of Community X, New Haven is a pluralist democracy led by multiple coalitions centering on the elected mayor (the finding reported by Dahl and his associates).[50]

---

[45] *Ibid.*, p. 257. In this definition, "power" becomes complexly intertwined with "authority" and "obligation." It is a curiously significant fact that one can find no sustained analysis of the latter two concepts in all Dahl's voluminous writings.

[46] *Ibid.*, pp. 277–278.

[47] Personal communication, Feb. 2, 1974.

[48] Since I have already discussed the most central chapters of *MPA* in connection with Dahl's views on power, I shall say nothing further about it in this section. The book is otherwise remarkable chiefly for the highly effective graphs, diagrams, and charts that Dahl uses to illustrate his limpid text; throughout, the book bears the stamp of a great teacher. A quite extensively revised second edition in 1970 adds a great mass of up-to-date cross-national data to the tables, but unhappily fragments and truncates a splendid chapter (Chap. 7) on the factors reducing the probability of a coercive resolution of conflicting aims.

[49] Because a review of this compass must of necessity focus on Dahl rather than on his critics, citations from the latter have had to be kept to a minimum. Several of the better-known essays of that genre have been gathered together in Charles A. McCoy and John Playford, eds., *Apolitical Politics: A Critique of Behavioralism* (New York: Thomas Y. Crowell Co., 1967).

Something like this syllogism must be invoked to account for the intensity of the criticisms to which Dahl's research strategy has been subjected. For, taken on its own terms, the approach is surely more defensible than most.[51] Instead of presupposing a unified power elite, it treats the matter as an open question to be settled by investigation; instead of employing a terminology that begs the question of whether inequalities are cumulative and stable over time in all sectors of society with regard to all issues, it disaggregates the concept of a power structure into elements that become joined empirically, if at all. Rather than collecting data on opinions as to who holds "power," it calls for direct investigation of the phenomenon through reconstruction of issues in which actors actually seek to effect their will against opposition. There is nothing in the logic of this procedure that precludes the possibility of uncovering a pyramidal pattern of power.[52] It is consequently as misleading to call this research procedure the "pluralist" approach as it is to suggest that those who uncover a pattern of dispersed inequalities approve, as citizens, of what they have found. To call a political scientist a "pluralist" is therefore to traffic in a triple ambiguity.

More serious questions are raised by the unfortunately labeled "nondecisions" critique.[53] For this critique is really an affirmation of the underlying logic of Dahl's research strategy, conjoined with a sharpened focus on the question of which issues are to be selected for study. On this point – clearly of paramount importance for a research strategy that proposes to draw general conclusions about the distribution of political power in a community from an examination of the terms on which specific issues are resolved – Dahl is uncharacteristically brief and unsystematic. In his 325-page report he devotes only one short paragraph to explaining and justifying his choice of the three issue areas that are to form the subject of the succeeding three chapters, which in turn form the base for the remainder of the book.[54] The criteria for the choice seem reasonable enough (though rather too *ad hoc* to serve as much of a guide to others wishing to undertake similar research elsewhere); and if one has considerable faith (as I do) in Dahl's grasp of the dynamics of New Haven politics, one may not be inclined to question the conclusions he draws from examining this particular set. Nevertheless, we are dealing at this point with matters of faith and authority, not with the usual demanding standards of the behavioral sciences. In particular, we are obliged to assume, on the basis of fragmentary evidence, that Dahl remained as alert as his critics to the possibility that issues of potentially crucial political importance were kept off the public (governmental) agenda altogether, or reached that agenda only after several crucial decisional premises had already been established. In as apparently open, pluralistic, and politically vigorous a community as New Haven, such a "mobilization of bias" does not in fact seem especially likely. But it is surely warranted to suggest, as David Easton has done, that the "gatekeeping" activities of the political system deserve as much attention as the management of conflict within the political arena, and that a comprehensive research strategy should systematically direct attention to both elements of the process.[55]

---

[50] Certain passages and chapter titles in *Who Governs?* contribute to the propensity of critics to read into its findings some general theses on American politics. For reasons not elaborated, Dahl wishes to argue that New Haven is in many respects "typical" of American cities (cf. the Preface and Appendix A). The second half of the book is subtitled "Pluralist Democracy: An Explanation," and several of the succeeding chapters are premised on the contention that New Haven is a pluralist democracy in miniature. The final chapter offers a brilliant general explanation of why American democratic values prevail in political life despite evidence of apathy or hostility toward these values by the general public. Leaving the problems of circularity aside (why are there payoffs to the political elite for affirming rather than attacking these values?), one must ask on what evidence this extrapolation from New Haven is based.

[51] For two exceptionally vigorous defenses, see Raymond Wolfinger, "Reputation and Reality in the Study of Community Power," *American Sociological Review*, 25 (Oct., 1960), 636–644, and Nelson Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963).

[52] Particular formulations may run this risk by giving undue weight to peripheral issues.

[53] The original – and in many ways still the best – statement of this thesis appeared in Peter Bachrach and Morton S. Baratz, "Two Faces of Power," *American Political Science Review*, 56 (Dec., 1962), 947–952.

[54] The crucial paragraph appears on p. 64. An even briefer resume is offered (p. 333) in Appendix B, in which he outlines the methodology of the study. On this point see Bachrach and Baratz, Part III. But one must assume that Dahl did not see the force of this criticism, for in a surprisingly ill-tempered and uncharitable response to a mildly critical article by Thomas Anton he rebutted 17 points in Anton's article but did not even refer to Anton's questions about how issues are defined. See Thomas J. Anton, "Power, Pluralism, and Local Politics," *Administrative Science Quarterly*, 7 (March, 1963), 425–457, at p. 454; and an exchange between Dahl and Anton in the same journal, 8 (Sept., 1963), 250–268.

Pluralist Democracy in the United States, in scope at least, would seem a more appropriate target for many of the strictures directed at *Who Governs?*[56] In fact, it has attracted much less attention. Possibly the lower ratio of new information about a better-known system, together with greater familiarity with Dahl's approach, accounts for the relative neglect the book has suffered. The textbook format has also something to do with the matter.

It is, nevertheless, no ordinary textbook. Dahl has one clear focus – the determinants of conflict and consent in American politics – around which the entire book is organized. Once again, one confronts Dahl's ability to abstract the essentials of a question and jettison the detritus. The first two pages immediately involve the reader in the implications of a national history marked both by the triumph of the Philadelphia constitutional convention and the tragic and massively bloody civil war three quarters of a century later. This dramatic juxtaposition leads directly to several sharply pointed questions about the dilemmas of modern large-scale democratic government. It is the object of the remainder of the book to answer these questions.

Certainly *Pluralist Democracy* is not an unmitigated apology for American government. Dahl treats as contingent what most textbooks take for granted: the stability and efficacy of American political institutions. He highlights the failures – and the accompanying violence – as directly as the achievements. It is the explanations themselves – and the paradigm for the explanations – that are somewhat too unexhilaratingly conventional. His categories and conclusions will be familiar to all readers of American political science over the past quarter century – the stress on diversity, on cross-cutting cleavages as a source of stability, on the evidence that American voters are startlingly apathetic, ill informed, and unstructured in their views by the standards of traditional democratic expectations. The book has more to say than most about strategies open to those who seek to achieve their goals within – or against – the system; it is in this sense more dynamic, less purely descriptive, than the traditions of "status quo" behavioralism would lead its critics to anticipate. But a 471-page treatise on American government in which, for example, the only indexed reference to the military is to the Military Reconstruction Act of 1867 is not likely to persuade the previously unpersuaded that Dahl has addressed himself squarely to the central issues in American political life.[57]

---

[55] Cf. David Easton, *A Systems Analysis of Political Life* (New York: John Wiley, 1965), Chap. 6. One serious objection to the "nondecision" thesis is that it simply raises hypothetical possibilities in a manner leading potentially to an infinite regress of irrefutable speculations. Clearly, no researcher can check out every possibility that might occur to a critic; and since, by definition, "nondecisional" activities are of low visibility, there would appear to be no control on this process. Indeed, it might be said that the issue between Dahl and his critics is how far to push back the defining boundary between the "political" and "nonpolitical" stages of a developing issue, since Dahl's method clearly does allow for the suppression of alternatives within the political arena before an authoritative decision is reached.

[56] In 1971 Dahl prepared a second edition under the title *Democracy in the United States: Promise and Performance.* The change in title was deliberate. On the one hand, Dahl sought to get away from the ambiguities – and the question-begging controversy – over "pluralism"; on the other, he wished to stress more explicitly the gap between the traditional ideals of democracy and the current record of the American government (which he calls, harking back to *PEW,* a "polyarchy" in order to take direct account of the unequal distribution of power within the system). The greater concern for evaluation carries over into the addition of several preliminary chapters on "Democracy as an Ideal and as an Achievement" and a final chapter assessing the performance of the American polyarchy; none contain ideas more novel than that great inequalities of wealth and income remain in the country and that racial discrimination violates American ideals.

[57] Another theme of some interest is Dahl's ambivalence toward reduced political participation. Like all attentive readers of empirical survey research reports on civic orientations, he is struck by the fact that most "extremists" are nonvoters and that a significant fraction of nonvoters hold "extreme" orientations. This perspective emerges most clearly in a controversy with Jack L. Walker over "The Elitist Theory of Democracy," *American Political Science*

Political Oppositions in Comparative Perspective

By the mid-1960s, Dahl's position within the American political science fraternity was well defined. On virtually any subject relating to American politics – from school board elections to foreign policy in an era of nuclear weapons, from the technical requisites for national economic planning to the empirical delineation of local power elites – he could be counted on to write with singular acumen, clarity, and cogency. Yet a paradox remained at the heart of his work. The most universalistic of political scientists in his abstract formulations, he had restricted the territorial domain of his substantive references to his native land – a land regarded by all foreign observers since its foundation as radically deviant in its politics from the patterns of the Old World.[58] As Aristotle had once taken the Hellenic *polis* to be the natural unit from which to derive generalizations about political man, so now Dahl appeared – in a decade of rapidly cumulating monographs on chaotic foreign systems – to be using the United States of America as his universe. The risks of misleading overgeneralization seemed similar.

The publication of *Political Oppositions in Western Democracies* (*P.O.W.D.;* 1966) accordingly marked much more than the appearance of yet another hundred pages by Dahl on the subject of contemporary democracy. For in three concluding essays to a collection of papers on the politics of opposition in ten North Atlantic countries, Dahl took on the challenge of relating the appearance, survival rate, and differing patterns of Anglo-European oppositional groups to the varied socioeconomic national contexts in which they have operated. It was an ambitious task. That his answers proved less conclusive here than his previous analyses of the American polyarchy spoke more for the inordinate complexity of the undertaking than for any flagging of his insight and application. Granted that in their brevity the three essays lacked the rich texture of history customarily found in essays on European politics, it could still be said that Dahl's comparative inquiries into the social foundations of democracy now brought him within the great tradition of Tocqueville, Bryce, and (more recently) Seymour Martin Lipset.

These were, at any rate, the figures with whom Dahl was compared, and not unfavorably, in an efflux of long, thoughtful review articles that for once were almost uniform in their praise. Critics differed largely over the question of parsimony. Harry Eckstein, for example, complained in an otherwise favorable review that Dahl had not been sufficiently audacious in reducing the number of his theoretical variables. Conversely, Sidney Verba, who applauded the "disciplined configurative approach" of the volume, remarked with oracular wryness that in Dahl's chapters, "the typology is too simple to capture the richness of real patterns of oppositions; on the other hand, it is complex enough to impede the development of any simple and parsimonious explanation of oppositions – which probably means that it is just about right."[59]

And indeed, both charges would appear to be merited. Dahl does not clearly establish the theoretical utility of listing "distinctiveness" as a major dimension of oppositional patterns alongside "concentration," "competitiveness," and "site of encounter" – the three variables of which it is said at the same time to be a function. Conversely, categorical explanatory variables like "the record of governmental response to grievances" are obviously so summary as to require immediate decomposition into typologies that must themselves be explained by a complex of singular historical statements such as commonly occur in conventional political histories of the region. There are also intricacies to the levels of explanation that, if systematically elaborated, would so complicate the model of explanation as to render it rather obviously unmanageable. One has only to consider the manifold interlocking relationships among cultural premises, the record of historical grievances, and the governmental and socioeconomic determinants of that record to see the magnitude of the problem. Dahl himself points to some of these difficulties in certain

---

Review, 60 (June, 1966), 285–305. It is a frustrating controversy: Walker appears to brush aside the evidence referred to above in his highly normative structures, while Dahl is more convincing in his general assertion that he favors an increase in some kinds of participation than in a long footnote (No. 13) designed to show that Walker's characterizations have no foundation.

[58] The one conspicuous exception to this generalization is the cross-nationally comparative structure of *Politics, Economics and Welfare.* But the multitudinous references to planning experiences in Great Britain, Sweden, the Netherlands, New Zealand, et al., seem chosen (save in the case of Great Britain) at a quite superficial level for their illustrative effect: the book neither stands not falls on their validity. One suspects that Lindblom's independent contributions as an economist were particularly important here.

[59] Eckstein, *American Political Science Review,* 61 (March, 1967), pp. 158–160; Verba, "Some Dilemmas in Comparative Research," *World Politics,* 20 (Oct. 1967), 111–127, at p. 118.

passages, and they are worth underscoring mainly because the substance of his analysis – cumulative and crosscutting cleavages, problem-solving vs. ideological orientations, the significance of late and grudging admission of the urban working class to the political arena – has been the staple for some years of monographs on European politics. It is for their simplification and arrangement of ideas, rather than for any novelty of content, that these chapters have won such favorable notice; and it is as such that they must be judged.

Though less discussed by his reviewers, Dahl's evaluative observations in this volume regarding the "democratic Leviathans" of the contemporary North Atlantic region may now strike many readers, at a remove of a decade, as being worthy of as much attention as his explanatory paradigms. For Dahl – hitherto best known as the sympathetic exponent of an Anglo-American, incrementalist, problem-solving political culture – was nevertheless moved to draw attention from a comparative perspective to the limitations of the two-party polyarchy as a model of rationally oppositional politics. A dichotomous organization of political conflict, he noted, might promote the favorable outcomes conventionally attributed to a disciplined two-party system – among others, the dampening of extremism, the generation of popular support for effective governmental action, and the preparation of alternative teams of leaders for the responsible exercise of power. But whether it does in fact do so depends heavily, he argued, upon the distribution of preferences within the electorate. Where these could be described through congruent bell-shaped curves for every crucial dimension of political choice, the outcomes would indeed resemble the controlled purposiveness of modern British politics. Bimodal or multimodal distribution of constitutional or policy preferences, on the other hand, created conditions in which electoral competition between two disciplined parties might well accelerate political polarization. In such countries, two very loosely structured parties or a multitude of splinter parties would therefore, while reflecting a dissensual society, also possibly serve as the necessary vehicles for civic governance.[60] Dahl predictably concludes that intense ideological conflict among purely representational parties generates irrationalities of its own. But the serious attention he offers these alternatives, and his detached appraisal of many venerated Anglo-American institutions, convey in their totality an openness to comparative experience not evident in his previous work.[61]

The concluding three chapters of the volume on *Political Oppositions* can now be seen to have prepared the way for what is undoubtedly Dahl's most ambitious publication of the past decade. *Polyarchy* (1971), despite its slender size, is breathtaking both in its scope and in the foundational character of its analysis. By any standard, it is a major contribution to the systematic study of democracy. Not since the days of Lord Bryce had any scholar dared to undertake a full-scale empirical exploration of the principal social, economic, cultural, and political conditions necessary for a democratic order, and in Bryce's time it was still possible to confine one's attention to Great Britain and its dominions, Western Europe, and a select group of nations in the Western Hemisphere.[62] Yet Dahl's short chapters deal so directly with the leading explanations for the emergence and survival of democratic politics that one quickly comes to take for granted an enterprise that, in the range of countries considered and in the explicitness of the propositions regarding them, has in fact no real parallels in the literature of comparative politics. Much as Dr. Watson reacted to Sherlock Holmes's deductions, one marvels after the fact that no contemporary political scientist had thought to take up so obvious a project before Dahl did so himself.

For *Polyarchy* is, in its broad structural outlines, a treatise of striking simplicity. In its first sentence it poses a problem of immediately compelling interest: "Given a regime in which the opponents of the government cannot openly and legally organize into political parties in order to oppose the government in free and fair elections, what conditions favor or impede a transformation into a regime in which they can?" The remainder of the book is strictly limited to answering that question. In so doing, Dahl feels compelled to note that he is not concerned with identifying the social foundations of an ideal construct termed "democracy" (in his terms, a political system in which the government is completely responsive to the preferences of its citizens, all being political equals). His purpose is rather to explore the environments of a global set of currently operative, large-scale national governments distinguished by (1) their guaranteeing of eight necessary conditions for open political competition and choice and (2) their extending of these guarantees to a very large and institutionally defined proportion of their total adult population.[63] It is these inclusive, internally competitive political systems, which Dahl calls "polyarchies," that form the dependent variable of the propositions he wishes to test.[64]

---

[60] *POWD*, pp. 375–376, 392, 395. The general theoretical argument was, of course, already advanced in its essentials in his *PDT*, Chap. 4, and in Anthony Downs, *An Economic Theory of Democracy* (New York, Harper & Row, 1957), pp. 117 ff.

[61] Even more striking is Dahl's anticipation, at the high-water mark of President Lyndon Johnson's Great Society, of disenchantment among "young people" and intellectuals with the "remote and bureaucratized" state of the "democratic Leviathan" (*POWD*, pp. 399–400). In these final pages of his Epilogue to the volume, Dahl faintly foreshadows the concerns that were soon to lead to his systematic exploration of the ultra-democratic critique of polyarchy in *After the Revolution?*

[62] A few recent studies, on which Dahl himself draws heavily, offer statistical correlates of various categories of political regimes, including democracy, but do not purport to relate these findings with any degree of complexity to the distinctive institutions and processes of the democratic order.

In performing his tests, Dahl takes into account the evidence provided by a carefully constructed list of twenty-nine polyarchies and six "near-polyarchies" throughout the world that seemed to be meeting his criteria during the late 'sixties. It goes almost without saying that his detailed knowledge of many countries on the list falls short of what more specialized scholars in the politics of each might wish. The objection, while obvious, is also somewhat beside the point. The gains as well as the costs of performing global comparative analysis deserve some emphasis. In *Polyarchy*, plausible generalizations evolved in Norway are tested in Japan and Israel and disconfirmed in Uruguay. Some countries appear especially well suited to examining the interplay of economic and political processes; others provide apt illustrations of the impact of social structure on party organization. Still others offer the occasion for reflections on the possibility of externally imposing an effectively polyarchal political order on a conquered nation.

So great is the scope of the book, and so brief the compass, that some sacrifice of complexity predictably occurs. The sacrifice is far less than one might anticipate. Dahl's concise discussion of the failure of democracy in Argentina should suffice to dispel the impression that *Polyarchy* contains no more than superficial correlations of questionably aggregated variables.[65] It is hard to think of a more pointed short discussion of the complex relationship between socioeconomic inequality and political democracy than the one Dahl offers here.[66] Some topics do suffer more severely than others from Dahl's extreme compression – particularly the chapter on the alternative historical paths of regime transformation and the discussion of the relation of levels of economic development to the prospects for political competition.[67] On the whole, however, *Polyarchy* must rank as one of the most impressive instances in the entire political science literature of saying much in little space.

It is inevitable that, in discussing the sociopolitical matrix of competitive politics in some thirty nations on six continents, Dahl should posit several disputable empirical connections. Each regional specialist will have his or her own list. It is perhaps more profitable to discuss at this point the approach Dahl employs. In essence, his strategy may be characterized as a succession of sophisticated applications of John Stuart Mill's Methods of Difference and of Agreement. Noting that polyarchal politics prevails in some countries but not in others, he seeks to account for its presence or absence by referring to the relative position of the countries under consideration on a succession of scales that middle-range theories of politics plausibly indicate to have some connection to these outcomes.

---

[63] *Polyarchy*, pp. 1–5.

[64] To me, at least, an unsatisfactory element in Dahl's delineation of his dependent variable is his insistence on employing two dimensions, one of which is patently more important than the other. "Competitive oligarchies" (e.g., Britain, 1832–1884) shade indistinguishably along a quantitative continuum into full polyarchies, whereas only Communists and Fascists would seriously contend that "inclusive hegemonies" resemble polyarchies in any critically significant sense. The problem in attributing a formally equal weight to public contestation and inclusiveness can be seen at its logically absurd extreme in Dahl's hesitancy at including Switzerland among the world's 25 polyarchies because it did not at that time grant suffrage in national elections to the female 50 per cent of its adult population (Appendix B, p. 246). Thus his diagram (p. 6) of "two theoretical dimensions of democratization" is characteristically ingenious, but utterly misleading in its formal implication that countries far along the 15° line (high in inclusiveness, low in public contestation) can properly be regarded as no farther from the 45° line than those far along the 75° line (low in inclusiveness, but high in public contestation).

[65] *Polyarchy*, pp. 132–140.

[66] *Ibid.*, Chap. 6.

[67] Thus, in his discussion of economic development, Dahl leaves himself no room to examine the independently important factor of *rate* of growth. Moreover, his insistence (*Polyarchy*, pp. 76–80) that the educational and structural requisites of an "advanced economy" generate demands for a competitive political order seems to grow mainly out of the exigencies of his general argument. It scarcely constitutes a refutation of writers who, taking into account the enhanced opportunities for deliberately controlling socialization, for monitoring increasingly sophisticated informational networks, for pyramiding economic and coercive resources, and for buying off potential dissidents, have argued otherwise in the case of the now relatively affluent Soviet and East European Communist nations.

These methods have served comparative analysts very well. They may indeed be said to comprise – as Mill was wont to argue – the core of the comparative method.[68] They invite explicit identification of the key causal and dependent variables, and explicit specification of the relationship among them. The approach tends to suffer, on the other hand, (as Mill himself quite clearly saw), from the uncertainties introduced when other theoretically relevant factors are neither totally different in all save one respect, nor totally similar in all save one. More disturbingly, it encourages examination of the variables in analytic isolation rather than as elements of organic systems.

Dahl's treatise suffers significantly from these limitations. He seldom makes use of a cumulatively refined structure of partial correlations; rather, each causal variable is separately examined in relation to his countries as part of an aggregating causal chain. Thus, in one chapter he will consider the relationship of agrarian society to polyarchy in New Zealand, Costa Rica, and India; in another, the significance of per capita income level in these countries; in a third, the degree of ethnic pluralism. But how the possibilities for competitive politics are affected in any one country by one of these variables, given the potentially synergistic effects arising from the specific levels attained by the other two in the presence of still other theoretically relevant variables, this approach does not permit us to judge.[69] The theoretical yield from his study is therefore not so much a theory (despite the title of Dahl's final chapter) as a tabulation of loosely related propositions about conditions favoring or opposing polyarchy. It is perhaps for this reason that Dahl's analysis is at its least persuasive in accounting for such anomalies as polyarchy's lack of resilience in Uruguay, its surprising durability in pre-modern Switzerland and contemporary India, and its prospective emergence with significant modification in Mexico, Kenya, and Tanzania. More contextual forms of analysis clearly have their drawbacks as well; it may well be that *all* approaches on a global scale must remain merely searchlights in the dark.[70]

Having discussed the general conditions favoring polyarchal government, Dahl evidently felt the logical obligation to extend his inquiry beyond those nations that formally and effectively institutionalize toleration for oppositional politics to those that could serve as a control for his propositions. This reasoning led him to examine those nations in which oppositions have held a precarious status as, at best, politically marginal hold-outs from the dominant party's quest for consensual governance, or – more commonly – as deplored and presumptively treasonous outgrowths the dominant political elites have lacked the resources to quash. *Regimes and Oppositions* (*RO;* 1973) records what one must hope to be merely the first fruit of this extension. As in the earlier volume on the West European democracies, the bulk of the book consists of monographs by area specialists on countries ranging, in this instance, from Japan and India through tropical Africa and Latin America to the hegemonies of Franco's Spain and the Soviet-East European Communist bloc. Dahl as editor contributes an introductory chapter in which he offers several universalistic propositions and categories that are intended to help account for the central findings concerning political opposition in the remaining chapters.

Enterprises of this nature contain many intrinsic hazards. Despite the global span of the volume – a scope that rather obviously taxes both the expertise and the powers of condensation of the authors of the short chapters on Africa and Latin America – the reader is left wondering how the plausibility of Dahl's propositions might have been qualified by studies of

---

[68] See J. S. Mill, *A System of Logic* (London: Longmans, Green & Co., 1957), pp. 575–577.

[69] Dahl is, of course, aware of this problem. He frequently diagrams the interactive potential of a complex set of variables – see e.g., the very well worked out models on pp. 56, 79, and 91. The difficulty is that to quantify them for each country, given the state of other theoretically relevant variables for that country and with due attention to threshold effects and emergent properties, would render the approach unmanageably difficult.

[70] One approach that trades some of the universalism and ready testability of Dahl's propositions for a richer interactive analysis is illustrated by such works as Barrington Moore's *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1966) and Samuel P. Huntington's *Political Order in Changing Societies* (New Haven: Yale University Press, 1968), in both of which a set of not very rigorously formulated general models is evolved out of a comparative study of the sources of fragility and strength of democracy (and other forms of government) in particular countries. A notable feature of Huntington's approach is that it points to the creative potential of political leaders and political movements as historical agents combining the specific resources of nations to build political orders – a potentiality that Dahl's cross-sectional categorical analysis obscures despite his chapter on the political *beliefs* of political activists.

Communist China, Southeast Asia, and the various Muslim states of the Middle East and North Africa. Moreover, because the oppositional movements under consideration comprise an unsystematically defined subset of a purely residual category, the structural and processual analogies between the politics of opposition in pre-1974 India and in post-Dubček Czechoslovakia (let alone between contemporary Japan and the Soviet Union under Stalin) are scarcely more striking than the differences. Dahl copes with this diversity through abstract generalizations notably less intimately in contact with the complex patterns delineated by his contributors than in his earlier, more ambitious effort to develop typologies to fit the historical experiences of the North Atlantic polyarchies. Although he brings into relatively sharp focus some dilemmas of governmental responses to manifestations of opposition in an illiberal regime, his leading conceptual arguments were already quite explicitly set forth in *Polyarchy*. There is, indeed, an uncharacteristic analytic weariness in Dahl's excessively brief introduction, a sense that he has casually taken on too much and made too little of it. Nowhere, perhaps, does this trait become more evident than in his invocation of the familiar metaphor of reinforcing and cross-cutting societal cleavages to explain the degrees of polarization in socially heterogeneous polities without any apparent concern for the corollary implication that cross-cutting cleavages at once explain the checks to polarization in pluralist America and the often catastrophic fragmentation of partisan constituencies in France.[71] It is surely premature to conclude that Dahl cannot discuss the emergent dynamics of intraparty politics and hegemonic violence in India, Tanzania, Brazil, and Yugoslavia with the same sensitive acumen he displayed in his earlier work on Anglo-American democracy. One can only record that this volume does not disconfirm that suspicion.

Size and Democracy

In his early work, as we have seen, Dahl focused on the practical obstacles to rational civic participation in democratic politics in an era of nuclear technology and far-flung international involvement. This essentially normative orientation (whatever its instrumentalist overlay) was then superseded by a prolonged engagement with conceptual clarification, with the construction of analytic paradigms, with close empirical field investigation, and with theoretical syntheses of others' research. It is, of course, those conceptual and empirical concerns that have become the hallmark of his writings, and it is probably through them that his lasting contributions to the field will prove to have been made. Yet Dahl's work now appears to be coming full circle; for even while steadily expanding his purview of comparative politics, he has simultaneously reverted, though in a more explicitly systematic form, to his earlier philosophical concerns.

"The City in the Future of Democracy" ("CFD"; 1967), Dahl's presidential address to the 1967 American Political Science Association convention in Chicago, heralded his return to the question of how best to improve the quality of democratic participation. A summer of unparalleled racial conflict in several metropolitan areas gave poignant urgency to his topic. His more general theme was, however, the spectacle of an apathetic mass public's loss of control over developments that had left America's major cities

> mean, ugly, gross, banal, inconvenient, hazardous, formless, incoherent, unfit for human living, deserts from which a family flees to the greener hinterlands as soon as job and income permit, yet deserts growing so rapidly outward that the open green space to which the family escapes soon shrinks to an oasis and then it too turns to a desert.[72]

"City-building," he observed, "is one of the most obvious incapacities of Americans."[73]

The problem so grimly symbolized by America's disintegrating megalopolises was nevertheless, Dahl reminded his audience, the embodiment of a very ancient philosophical dilemma. Pericles, Plato, and Aristotle had insisted that civic virtue thrived only in political units sufficiently small to permit full participation in the total range of governmental activities by the entire body of legally qualified adult male citizens. Two milennia later, Machiavelli and Rousseau still voiced the same argument. But since the advent of representative government, the center of gravity of philosophical discourse had shifted toward stressing the external costs that small, uncoordinated political units imposed on one another. Political writers had furthermore made much of the power of provincial prejudice in small units and the ease with which such units were dominated by their largest single actors. The pivotal figure in this shift of perspective was clearly Madison, whose cogent advocacy of large units in *Federalist* No. 10 lay midway between "Montesquieu and Rousseau, who still see the city-state as the only proper and indeed viable unit for a democratic republic . . . [and] John Stuart Mill, who in *Representative Government* dismisses as irrelevant in a single sentence at the end of a chapter, almost as an afterthought, the two-thousand-year-old tradition."[74] But changes in perspective do not necessarily insure progress; and it was Dahl's object to show that in accurately highlighting the limits of the city-state as an autonomous political unit, contemporary writers had too often failed to assess systematically the costs of its expansion.

---

[71] *RO*, p. 5. Of course, the differential impact of cross-cutting cleavages in these nations can be dealt with through introducing such flexible contextual concepts as the catalytic significance of varying political cultures; but Dahl does not enter into such complexities. Instead, at the bottom of this same page, he alternately deploys cross-cutting and reinforcing cleavages as a means of suggesting why political cohesiveness is reduced to a minimum in African politics.

[72] *"CFD," p. 964.*

[73] *Loc. cit.*

His own leading thesis begins with a crucial distinction. Political participation, Dahl suggests, may be analyzed into two categories – acts involving exchanges of opinion about what should be done, and processes designed to register and implement these preferences. The distinction is crucial because actions directed toward the latter ends (such as voting) can be carried out more or less simultaneously by an indefinitely large number of actors even within the giant nation state, whereas the necessarily sequential nature of deliberation sets severe upper limits to the size of an authentically democratic political unit. "Time's relentless arrow," he notes, "flies directly to the Achilles' heel of all schemes for participatory democracy on a grand scale."[75] Thus the acknowledged drawbacks to small-scale politics, together with the invention of representative government, have impelled us toward the creation of ever larger electoral units, but they have meanwhile blunted our sensibilities to what is lost when democratic participation is limited for most citizens to such passive threshold acts as voting. The predictable result is the spreading disorder to which the citizenry of our metropolises must now bear impotent witness.

Can the dilemma be resolved? Dahl rejects at a minimum two currently favored alternatives – the efforts to recreate the simplicity of village life (as in the flight from the central city to the suburbs), and the hopes for transferring the active forums of democracy to the workplace. The first he sees as an irrelevant exercise in nostalgia, abetted by blindness to "the oppressive weight of repressed deviation and dissent [in small communities] which, when they appear, erupt explosively and leave a lasting burden of antagonism and hatred." The second, on the other hand, turns on a misplaced effort to make the workplace co-extensive with life itself without recognizing the distinctive exigencies of corporate action that demand some form of external regulation.[76] Dahl suggests in passing that the medium-sized city in contemporary America – a city of between 50,000 and 200,000 inhabitants – may very well optimize most of the significant institutional economies of scale while providing "a political unit of . . . truly human proportions in which a citizen can acquire confidence and mastery in the art of politics."[77] But his most important and most defensible thesis is that empirically minded political scientists have so far provided us with little solid evidence on which to base any assertions whatever about the connection between societal scale and political democracy.

It is quite characteristic of Dahl that he should himself already have taken the logical next step. *Size and Democracy* (*SD;* 1973), co-authored with Edward R. Tufte, is a groundbreaking effort to operationalize the various politically relevant dimensions of jurisdictional "size" so as to make possible their correlation with the presence and quality of "democratic" political life.[78] The investigation of such correlations was clearly overdue. In a provocative introductory chapter the authors manage to gather together twenty-seven plausible but radically conflicting hypotheses – some very ancient – that they have culled from the history of political thought on the relationship between their two key variables. Their inference states itself: after some two thousand years of controversy the time has come to reduce this list through systematically using the available evidence to confront paired statements that cannot both be true.

---

[74] "CFD," p. 955.

[75] *Ibid.*, p. 962.

[76] *Ibid.*, pp. 961–962. Cf. his earlier exposition of the problems of co-determination in British industry (note 7 above).

[77] *Ibid.*, p. 967. A list of cities in contemporary America providing such opportunities would make interesting reading. It would include such centers of civilization as Fresno (California), Amarillo (Texas), Peoria (Illinois), Gary (Indiana), Scranton (Pennsylvania), and (of course) New Haven (Connecticut), while clearly excluding such metropolises as Rochester (New York), Birmingham (Alabama), Tulsa (Oklahoma), and Wichita (Kansas) on grounds of size.

[78] Though published in 1973 – three years after the "Yale Fastback" (*After the Revolution?*) discussed below – the first draft of *SD* was written in 1967 during a stay at the Center for Advanced Study in the Behavioral Sciences in Palo Alto. It seems accordingly appropriate to discuss *SD* in direct conjunction with "CFD," which draws heavily on ideas developed during the course of research for the larger volume. Since no effort is made to distinguish the contributions of the two co-authors, I have here attributed to Dahl full responsibility for all statements appearing in the book.

The two authors embark on this enterprise with great care. The guiding theoretical considerations receive axiomatic formulation. Propositions from one chapter are used to qualify and extend the hypotheses advanced in the next; indicators for elusively abstract variables are chosen imaginatively but persuasively. The scope is as global as in *Polyarchy* – more sweeping, even, for in addition to the full subset of twenty-nine "polyarchies" the authors also make comparisons among subnational levels of jurisdiction within single nation-states. The authors profitably borrow from analogous discussions by economists of market size – a practice that in Dahl's case goes back to *Politics, Economics, and Welfare* but that he nowhere employs to better advantage. They argue rigorously on theoretical grounds that no one size of jurisdiction can be held to be optimal for democracy, and that at every stage of the argument, a tradeoff must be anticipated between the opportunities for effective civic participation in politics and the capacity of the system to respond to the full range of the collective preferences of its citizens. Even so, they do not lamely conclude that there are both costs and gains to any given size of polity; they *inventory* those costs, construct plausible cost-benefit ratios containing relative magnitudes, and in some instances disconfirm widely accepted suppositions regarding the relation of size and democracy.

A review of this scope necessarily precludes detailed discussion of the resultant conclusions – largely based, it should be noted, on secondary analysis of the disparate findings of other scholars. Some are at least nonobvious (such as the finding that threshold effects for the quality of participation are so low that citizens in the very smallest nation-state democracies feel no more efficacious than those in the largest).[79] Some are paradoxical but potentially misleading (the evidence that the military costs of survival are, if anything, lower in the small democracies than in the large); some demand more discussion than the authors offer (e.g., the fragmentation and specialization of civic participation arising from indefinite proliferation of governmental jurisdictions designed to be functionally optimal for limited purposes). Dahl and Tufte are surely justified, however, in concluding that "one task of democratic theory may be to specify not an optimal unit but an optimal number of units . . . their characteristics, similarities, and differences, the nature of a good political life in each type of unit, and the proper relationships among them."[80]

Unhappily, *Size and Democracy* falls far short of this goal. Little more can be positively affirmed regarding the connection between the two than before the book appeared. The reasons have more to do with the nature of the subject matter than with the limitations of the authors. Among the more prominent difficulties are the apparent unavailability at the time of directly pertinent data beyond a single study of local government in Sweden; the recurrent indications that cross-national comparisons yield less reliable conclusions than comparisons of subnational governments within one country; the useful but essentially negative finding that size alone is seldom as important as institutional, historical, and socioeconomic factors in explaining differences among democratic regimes; and the inherent problems – already referred to in the discussion of *Polyarchy* – of any single-variable causal analysis.[81] It remains, of course, the singular merit of Dahl and his coauthor to have carried the inquiry as far as they did on the basis of such limited and qualitatively uneven evidence. They carried through their task, moreover, with a clarity of formulation that reveals the precise obstacles to a more extended pursuit of the topic. It is not, therefore, too much to say that their ultimate failure is in many ways more instructive to contemplate than other more conclusive comparative studies.

Participation and Authority

Meanwhile, the field as a whole was facing a challenge of a more inflammable nature. By the late 'sixties, the manifest disintegration of a politics of intervention abroad and pluralist conciliation at home had alarmed most thoughtful Americans. Within the discipline, these developments were beginning to evoke correspondingly strident demands for a political science of relevance and engagement. The target of these demands was, of course, the tradition of abstract and (relatively) detached political inquiry that Dahl, through his contributions, had come in considerable measure to personify.

---

[79] *SD*, pp. 61–65.

[80] *Ibid.*, 141, 142. A further virtue of the book is that it contains, in a discussion of the relation of size to system capacity, the most enviable sentence in the spectrum of Dahl's vast writings: "A small-democracy man may put his bets on the mouse outlasting the elephant and the whale; doubtless he will find takers among the Chinese, Russians, and Americans" (p. 111).

[81] On the limited conclusions that can be drawn from cross-national comparisons regarding size see, e.g., *SD*, pp. 52–53. Accepting this limitation, several students of Heinz Eulau at Stanford – among them Gordon Black – have taken up systematic empirical investigation of the consequences of size for local government inside the United States. Cf. also Sidney Verba and Norman Nie (both formerly of Stanford), *Participation and Democracy* (New York: Harper and

After the Revolution? (*AR?;* 1970) should be read with this context in mind. It is, of all Dahl's essays, the one most directly responsive to a specifically identifiable mood during a particular historical crisis. More, perhaps, than any other of his works, its agenda is set – on first reading, at least – by the issues of the hour. Its eye-catching cover, with its banners and headbands, is aimed squarely at an undergraduate clientele. From the question-begging title to the avuncular conclusion it serves as an emblem of the period it addresses.

Yet Dahl himself does not merely float with the stream. Despite the first-person mode of address, which often sounds alternately patronizing and irritable in cold print, *After the Revolution?* is an intensely serious short treatise for readers whom Dahl takes seriously. Its topical prolegomena lead quickly to more enduring matters. "Revolutions," Dahl notes with commendable acidity, now occur at the level of women's hemlines, but the authentically revolutionary potential of the democratic ideal can never be wisely discounted. After the latest "revolution" is over, the problem that troubled the ancient Greeks will remain with us – how to reconcile the exigencies of public governance with the belief that only those governments are legitimate that permit direct and continuous participation by the citizen in the exercise of their authority.

This issue has a familiar ring; and indeed, attentive readers of "The City in the Future of Democracy" and *Size and Democracy* will find little new in two of *After the Revolution?*'s three sections. In neither of the former works did Dahl elaborate quite so explicitly and so thoughtfully as in these chapters his critique of those who uncompromisingly advocate either small or large-scale governmental units. Both of the former works are, however, rather clearly predicated on the same set of theses that Dahl here summarizes as "the dilemma of primary democracy"; and, as already noted, both reach the conclusion – as Dahl does in *After the Revolution?* – that no one level of jurisdiction can maximize all the values of importance to those strict democrats who also acknowledge the inability of small-scale governments to cope with the external costs their actions engender. Insofar as there is novelty in these chapters, it lies in Dahl's application of his "Chinese Boxes" theorem to three problems of the day – the unequal distribution of socioeconomic resources in most political democracies; the quest for worker control of the "corporate leviathan"; and the somewhat analogous quest of the citizen for control of the "democratic leviathan." Yet each of the latter two topics is disposed of predictably, while the ten-page discussion of inequality soon becomes almost startlingly platitudinous. That much here is well said does not obviate the fact that it has also been said – very often – before.

The lasting claim on most readers' attention comes almost surely in the opening section. Dahl's treatise is subtitled "Authority in the Good Society," and it is here that he seeks to explicate the roots of our habitual compliance with the commands of government. He distinguishes three criteria – the criteria of personal choice, of competence, and of economy – that must be satisfied if the attempted exercise of any such authority is to be judged binding by those who must comply. His leading thesis is that the frequently conflicting implications of the three will necessitate some degree of institutional tradeoff among them to attain an optimal solution. Dahl is himself clear that any doctrine of authority that ignores or denies this proposition will prove unpersuasive to those who have understood it. His object, then, is to establish the independently binding importance of these three criteria, and to show (in the remaining two sections of the book) how acceptance of them precludes belief in the sufficiency of any single level of government.

Two of the three criteria are immediately recognizable to any student of classical political philosophy. Like Plato, Dahl insists that rational citizens will often delegate to a smaller group their authority to make collectively binding decisions in the expectation of thereby attaining a superior degree of competence in decision making from those who decide on their behalf. But like Aristotle, he suggests that those who are bound by others' decisions are generally (though by no means invariably) best qualified, as a group, to assess periodically the competence with which these decisions have been made, and like Hobbes, he assumes that all such acts of delegated authority will remain rationally persuasive to the individual citizen

---

Row, 1972), pp. 231–243, although it treats Dahl's theme only briefly.

only so long as they are seen as clearly more congruent with individual self-interest than the outcome of the unrestricted exercise of personal choice by all. The criteria of personal choice and decisional competence are therefore not necessarily in conflict, but they are always potentially so. Their reconciliation depends crucially on acceptance of the third criterion, the Criterion of Economy. It is a criterion which, Dahl observes, "seems never to have found a prominent place in classical political theory and lacks the allure of the other two criteria."[82] Yet it is, as he says, "a criterion that a reasonable man will apply to any system of authority," for it specifies that reasonable citizens will always weigh the opportunity costs of political participation – and of a system of authority heavily dependent on it – in terms of the alternative uses to which they could put the same time, personal energy, and resources. It is for this reason that, as Dahl had remarked previously, "time's arrow" so fatally indicates the limits to visions of unrestricted participatory democracy.[83]

Dahl's argument in the remaining chapters of *After the Revolution?* flows directly from these premises. A few arithmetical calculations suffice to show – surely for all time – that only in very small political units (those with memberships of only a few hundred at most) can even the most active speakers hope to engage in sequential, cumulative discussion of a severely limited agenda.[84] Yet the leading principle of participatory democracy – one that Dahl calls The Principle of Affected Interests – holds that everyone who is affected by the decisions of a government should have the right to participate in that government. Assuming that "participate" means something more than the right to indicate a readily countable reaction to a slate of proposals formulated in other forums, the dilemma is at once evident. The participatory corollary of The Principle of Affected Interests presupposes collectivities sufficiently small in their number of citizens to permit sequential interaction with reference to the terms of a governmental decision. Yet the Principle of Affected Interests implies that even the boundaries of the giant nation state exclude vast numbers of people who should be entitled to participate in formulating governmental policy regarding pollution, war, and developmental assistance.[85] Acceptance of the principles most instantly appealing to advocates of participatory democracy consequently leads to an immediate impasse. The impasse occurs because political jurisdictions sufficiently large to cope effectively with the problems of the age, and to encompass the people affected by them, are by those very criteria vastly too large to allow for the preliminary civic interchanges required to give their actions the necessary moral authority.

Dahl can offer only one response to this dilemma. Once again he cautions against elevating any one form (size) of governmental unit to a position of pre-eminent legitimacy. His response (as he quite openly acknowledges) does not resolve the underlying problem of how to formulate an alternative conception of democratic legitimacy. Fragmenting governmental jurisdiction along functional lines merely diffracts attention and encourages special-interest thinking. It undermines the elemental human attachment to the governments we now possess.

But a prior question is whether Dahl's essay actually addresses itself to its subtitle. The criteria for "authority" do not appear in these pages as the test of the *rightness* of a government. They focus on its capacity to facilitate achievement of one's interests. Does a governmental process insure that the resulting decisions correspond to our personal choices? Does it secure the services of those who are seen by us to be peculiarly qualified by knowledge or skill to render a correct judgment? Does it economize in the consumption of the scarce hours we would otherwise put to other highly valued uses? These are questions any instrumentally oriented consumer would rationally wish to ask of a public service agency. They do not differentiate decisional processes we support because they are effective in meeting our needs from those we regard as legitimately binding without prior assessment of their outcomes. In pressing the very defensible thesis that no government can retain the support of its politically relevant constituency without meeting these three criteria to some extent, Dahl further implies that these necessary conditions for the acceptance of the legitimacy of governmental authority are in some sense also sufficient.

After the Revolution? is in many respects a courageous book. Rather than labeling the positions he criticizes, Dahl analyzes them. Courage – and also self-discipline – are required to listen attentively to new formulations of old arguments, and to restate these formulations accurately and connectedly. It requires a clear head and much courage to rethink one's own principles in their light, and yet to affirm without fear of fashion those elements of one's previous commitments that have survived this test. Dahl's brief and timely essay is a work of notable integrity. Yet in that integrity lie also its limitations.

---

[82] *AR?*, p. 55.

[83] Cf. note 75.

[84] Technology has no bearing on the problem; for while advanced technology can link together people who are spatially dispersed, it cannot – or at least, cannot yet – reduce the time required by any one participant to process, assimilate, and respond to the ideas conveyed by the immediately preceding speaker, and therefore cannot overcome the limits to the number of sequentially interdependent exchanges that can take place within any given period of time.

[85] *AR?*, pp. 59–67, 82–88.

Dahl on Democracy

Any synoptic view of Robert Dahl's contributions to the discipline of political science should suffice to establish one point. To label Dahl a "pluralist," to identify him with the "behavioralist" faction in political science, to write as if he was author of a single book on politics in New Haven is to judge him narrowly and misleadingly. Dahl is all these things. He is also very much more. He is – to put the matter summarily – the most knowledgeable, the most persistent, the most rigorous, the most methodologically varied, and the most prolific student we have of contemporary democracy. In the full history of our discipline, no member has formulated so ingeniously so many central questions about the subject, and none has indicated so clearly and so authoritatively how they might be answered.

To seek some overall assessment of his contributions to our understanding of democracy is therefore a hopeless enterprise. He asks too many questions in too many ways to give validity to summary generalizations. Proper appraisal can only be rendered contextually. This long review has already pursued that end. Even so, his favored paradigms for inquiry do not defy all characterization. And like all such characterization, to highlight is implicitly to suggest the costs and gains of the approach.

Thus in reviewing the totality of Dahl's works one notes repeatedly his nominalist tendency to make more of differences than of similarities. Rigorous and subtle in thought, and keenly sensitive to the texture of human motives and objectives, he is inclined to differentiate where others synthesize. It may seem odd to apply this characterization to a social scientist far better known for his simplified categories and diagrams than for his interest in – or flair for – contextual historical narration. Yet it accounts, I believe, for a rather predictable division of perspective between Dahl and his critics. Where they see capitalism and socialism, he sees a mixed assortment of control techniques. Where they see social structures, he writes of classless actors, or actors bearing categorical background traits, who pursue multiple objectives with limited resources. Their ruling elites become, by his criteria, contingent coalitions identified through detailed investigation of particular issues. When they treat the phenomenon of "power," he asks "over whom, with respect to what, and with what probability of compliance?" Dahl's formulations are generally by far the more logically compelling in their analytic rigor. Neither Dahl nor his critics, however, have explicitly and persuasively addressed themselves to the perhaps ultimately unanswerable problem of how to decide at what point "marginal" differences become "fundamental" similarities. Accordingly, it seems likely that Dahl's critics will continue to attach more significance than he to the structural elements defining the political arenas he chooses to investigate.

This decompositional – literally analytic – propensity is undoubtedly linked to Dahl's commitment to methodological individualism. Like the economists, whose approach to social science he quite evidently finds congenial, he is most in his element when discussing the rational responses of individual actors to a parametrically established environment. This commitment gives his work a solidity of motivational grounding and a sharpness of focus that much sociological theory so conspicuously lacks. At the same time, he has not yet found a theoretical paradigm for discussing systematically the aggregate consequences of individual choice that result in the emergence of new properties at the systemic level. In this respect his work lags behind the achievements of both classical and contemporary economic theory, and even behind the contributions of the rational behavior theorists in political science. It is partly also for this reason that he is at his least illuminating in discussing at the systemic level such phenomena as power deflation, regime breakdown and transformation, the dynamic relation between coercion and persuasion, and the like.[86] Recently, Dahl has taken up aggregate analysis in his comparative studies of democracy; but in doing so, he has adopted a relatively conventional correlational approach that obscures many vital questions of systemic functioning.

---

[86] For one example among many of the use of rational behavior analysis to identify the emergent properties of large-scale interaction, see Mancur Olson, *The Logic of Collective Action* (Cambridge, Mass.: Harvard University Press, 1965). On the systemic properties of power, see Parsons, "On the Concept of Political Power," on regime transformations, see David Easton, *Systems Analysis of Political Life.* Cf. fn. 55.

Perhaps it is also this affinity for economic reasoning that accounts for the curious status of value formation in Dahl's works. When not altogether ignored, the binding force of a commitment to publicly defined values is either discounted without argument or reduced to a formula for realizing private preferences. Dahl's political actors pursue fixed goals derived from their personal utility curves; and while their behavior is a function of the resources available to them, of the degree to which their interests are affected, and of the manipulative skills of their competitors and opponents, they do not appear to change their course of conduct in light of an emerging understanding of their shared responsibilities as members of a collective social order. Thus the U.S. Supreme Court becomes, in one of Dahl's early essays, merely a decision-making agency whose members express preferences that may be overridden by other power groups whose vital interests are affected. Later, the relative stability of the various democracies is accounted for through configurations of interests and resources, and authority itself is discussed in terms of personal needs and individual calculations of opportunity costs. Seen in these terms, "authority" becomes scarcely distinguishable from "influence," and is exercised in much the same manner in relation to very similar objectives. Citizens are consumers who experience no schizophrenia regarding their public and private roles because they have never undergone the transformation described by Rousseau. Such an approach substitutes a welcome realism concerning enduring human motives and calculations for the question-begging terminology in which the concept of political authority is too often discussed. Its price is that of making the game of politics appear far simpler than it is.

These paradigmatic elements in Dahl's work continually inform his analysis of contemporary democracy. His nominalist sensitivities, together with his sympathy for the organizing behavioral premises of traditional economic theory, preclude his acceptance of theories of modern large-scale democracy in which the actions of democratic governments are presented as the enactment of a consensual popular will. Like Joseph Schumpeter before him, he sees the concept of a unified, rationally informed, disinterested, operational, unambiguously articulated popular will as a will-o'-the-wisp in complex societies, an artifact conjured up by populistic demagogues and dictators to cover the lack of mechanisms for authentic public consultation.[87] Such ideational entities have no motive force within his analytic framework. Government in any modern society, he insists, is necessarily predicated, one way or another, on the enduring reality of differences of opinion and interest. Democratic governments are to be distinguished from other regimes by the legitimacy they grant to the expression of such differences, and by their commitment, in undertaking collectively binding actions that unavoidably favor some interests over others, to adhering to formulas that enhance continuing cooperation among people who do not agree.

In Dahl's view, moreover, modern, large-scale democratic (or polyarchal) government is, like other governments, a government by elites. It rests on a political division of labor in which some voices are heard more loudly than others. It is crucially differentiated from other forms of elite government by the effective guarantees it offers to oppositional elites who publicly contest the continued tenure in office of incumbent teams of leaders. For it is mainly such contests, Dahl has contended with unmistakably Schumpeterian logic, that enable citizens to make the choices necessary to enforcing some degree of correspondence between their preferences and the politics of the governing elites. Not by the values a government espouses, nor yet by its class composition, can we judge the authenticity of a democratic order, but rather by the political provisions it makes for holding governmental leaders effectively accountable to the led.

Dahl's major treatises reflect without exception this emphasis on process. *Congress and Foreign Policy* and *Domestic Control of Atomic Energy* explored the problems of democratic accountability in governmental policy sectors in which not only citizens but also lay politicians were explicably apathetic and uninformed. The final chapter of *Who Governs?* develops the paradox of elite protection of democratic values neither comprehended nor shared by the general public, while *Pluralist Democracy in the United States* amplifies substantively the processes balancing equality with intensity first described in *A Preface to Democratic Theory*. *Politics, Economics, and Welfare* presents polyarchy as one of four principal techniques of social control. The social conditions retarding or promoting the institutionalization of this technique are then explored comparatively in *Political Oppositions in Western Europe, Regimes and Oppositions, Polyarchy,* and *Size and Democracy* before the technique itself is offered a philosophical defense against the strictures of egalitarian participatory democrats in *After the Revolution?*

---

[87] See Joseph Schumpeter, *Capitalism, Socialism, and Democracy* (London: Allen & Unwin, 1943), Part IV. Dahl shows somewhat greater optimism than Schumpeter regarding the possibilities of rational self-clarification through participation in political discussion.

The centrality to Dahl's thinking of an institutional conception of democracy – of democracy (polyarchy) as an arrangement requiring political elites to seek the legitimation necessary for governance through a relatively fair and open competition for popular support – is thus beyond serious question. It is a concept that presupposes conflict rather than transcendent consensus; that focuses on actors and coalitions rather than on cybernetic equilibrations; that pays more heed to rational strategies to attain determinate goals than to unconscious psychic needs that may have been fulfilled in the process. It asserts the intrinsic importance of political institutions independently of the class interests that may at any particular time be served by the outcomes. Dahl's theoretical understanding of democracy assumes that democratic politics is more a process for implementing interests than for transforming values, and it is generally inclined to discount rather heavily the controlling efficacy of sentiments of sharing, caring, and mutual trust. As the defining contours of its principal lines of thought were already visible in *The Federalist Papers,* it will, in all probability, remain subject to the kinds of praise and criticism that that treatise on republican government has long evoked.

Dahl's choice of paradigms, and the implications of his choices, are of exceptional significance to all students of politics because, as was suggested at the beginning of this article, his outstanding contribution to the discipline is as its teacher by example. It is through the problems he selects, through the questions he asks, and through the criteria he establishes in answering them, far more than through the findings he reports or the research methods he has developed for obtaining them, that he commands our continuing attention. By and large, he has proved an admirable teacher. He has set a standard of rigor and accuracy, of universalistic theoretical speculation and relentlessly subtle and detailed analysis, that few have matched and none surpassed. In an era of esoteric specialization and turbid expression, of casual scholarship too often propelled by rancorously partisan rhetoric, these qualities cannot readily be overvalued. One must also acknowledge the limits they necessarily entail.

Trends in German Political Science: A Review Essay*

PETER H. MERKL
University of California, Santa Barbara

German political science has come a long way, both in numbers and in sophistication, from those days in the mid-'fifties when a Berkeley political scientist returning from a trip to the Federal Republic could say: "There are fewer people teaching political science in all of Western Germany than in this [Berkeley] department alone." Those were the days when German universities had to be dragooned by their Rektorenkonferenz into establishing at least one chair of political science each and frequently left the discipline precariously dangling between the well-entrenched Faculties of Law, Economics, and Philosophy (history). Many universities indeed appointed to the new chairs constitutional lawyers or historians such as Gerhard Leibholz or Karl D. Bracher who then excelled in constitutional law or history.[1] Others were fortunate enough to find returning German emigrés, mostly from the United States, who at least were well-trained political scientists or sociologists and sometimes contributed a distinctive Anglicized-German vocabulary to the discipline. And then there was the peripatetic Carl J. Friedrich, equally at home at Harvard and at Heidelberg, whose influence on the discipline was and continues to be profound.

Today German political scientists are far more numerous. They seem to have overcome the generational problem of recruiting and training well-qualified newcomers. They no longer need to lean on other academic disciplines nor be afraid of their intramural rivals. A few of them even became *Rektoren* (university presidents), *Oberbuergermeister*, *Bundestag* deputies, or Ministers of Culture and Education, a source of status as well as of power for the discipline. Yet they are also deeply dissatisfied with themselves and with each other – a circumstance which has created extraordinary intellectual ferment and an unusual awareness of the problematic nature of political knowledge and research. The student revolt and the resulting upheaval in German university life unsettled and continues to challenge the very

*Klaus von Beyme, *Die politischen Theorien der Gegenwart* (Muenchen: Piper, 1972).
Wilhelm Hennis, *Die Missverstandene Demokratie* (Freiburg: Herder, 1972).
Fritz Scharpf, *Demokratietheorie zwischen Utopie und Anpassung* (Konstanz: Universitaetsverlag, 1970).
Juergen Habermas and Niklas Luhmann, *Theorie der Gesellschaft oder Sozialtechnologie?* (Frankfurt: Suhrkamp, 1971).
Bernd Guggenberger, *Wem nuetzt der Staat?* Kritik der neomarxistischen Staatstheorie (Stuttgart: Kohlhammer, 1974).
Claus Offe, *Strukturprobleme des kapitalistischen Staates* (Frankfurt: Suhrkamp, 1972).
Juergen Habermas, *Legitimationskrisen im Spaetkapitalismus* (Frankfurt: Suhrkamp, 1973).
Helmut Schelsky, *Die Arbeit tun die anderen.* Klassenkampf und Priesterherrschaft der Intellektuellen (Opladen: Westdeutscher Verlag, 1975).
Juergen Dittberner and Rolf Ebbighausen, eds., *Parteiensystem in der Legitimationskrise* (Opladen: Westdeutscher Verlag, 1973).
Ossip K. Flechtheim, ed., *Die Parteien in der Bundesrepublik* (Hamburg: Hoffman & Campe, 1973).
Heinz Laufer, *Das foederative System der Bundesrepublik Deutschland* (Muenchen: Bayerische Landeszentrale fuer politische Bildung, 1973).
Werner Kaltefleiter, *Die Funktionen des Staatsoberhauptes in der parlamentarischen Demokratie* (Opladen: Westdeutscher Verlag, 1970).
Richard Loewenthal and Hans-Peter Schwarz, *Die zweite Republik: 25 Jahre Bundesrepublik Deutschland, eine Bilanz* (Stuttgart: Seewald Verlag, 1974).

---

[1] This review essay could not cover all the current specialties of German political science. I decided, in particular, to leave out international and comparative studies while drawing the line toward sociology rather loosely. The following colleagues generously responded with suggestions for books to include in this review: Kendall Baker, Klaus von Beyme, David Conradt, Charles Foster, Wilhelm Hennis, John Herz, Hans D. Klingemann, P. C. Ludz, Ralph Mendershausen, Franz Pappi, and Kurt Sontheimer.

foundations of their understanding of politics and of their role as political scientists. This anguish and uncertainty is reflected in their continual debate over principles and methodology as well as in their formation of antagonistic groups.

Some efforts have been made recently to understand and place in the appropriate context the variety of approaches now current in West Germany, including some in English. David P. Conradt in a recent review article testified to the growing methodological sophistication of a sampling of empirical studies.[2] But this represents only one of several schools which can be categorized along historical lines or by academic subfields. The "historical schools of political science and sociology" were recently identified by Peter Christian Ludz as follows: (1) the empirical-analytical (Cologne) school of René König and Erwin K. Scheuch from which the Mannheim school of current empirical studies (Rudolf Wildenmann) and Ludz's work on the social structure of the DDR are derived; (2) the critical-dialectic (Frankfurt) school of Max Horkheimer and Theodor W. Adorno, which numbers Jürgen Habermas and many neo-Hegelians and neo-Marxists among its current disciples and has stressed cultural criticism and the analysis of "class rule" in the Federal Republic; and (3) the normative-institutional school of Helmut Schelsky which includes political theorists such as Wilhelm Hennis and many others.[3] German scholars have long tended to form schools, and another advantage of categorizing their work in this fashion lies in supplying the appropriate philosophical context for what might otherwise seem like isolated, even obscure investigations or arbitrary methodologies. On the other hand, these historical schools have also tended to intermingle under conditions of high academic mobility, proliferation, and differentiation.

A new generation of German political scientist-sociologists is now clashing over new issues and problems and producing a spate of publications far too numerous to cover in a single review article. One of the most prolific, Klaus von Beyme[4] (Heidelberg) in a recent book, *Die politischen Theorien der Gegenwart,* proposes distinguishing conflict theorists vs. integration systems theorists, depending on their underlying concepts. But he also starts out with the three "theories of science" enumerated above ("ontological-normative," "analytic-scientific," and "dialectic-critical") and, by relating them to various "approaches" (historical-genetic; institutional; behavioristic; functional-structural; and comparative), depicts a quasi-American pluralism of methods that in its interconnections is unlikely to be acceptable to any single writer or school among von Beyme's German colleagues.[5] Moreover, the bitter debates of the 1960s between the American-style empiricists on the one hand and the dialectic and normative antipositivists on the other soon gave way to the highly political confrontations between the cadres of the New Left and the liberal establishment at German universities. For political scientists in particular, the challenge of the rebels calls for the "democratization" of the whole West German political system, whose painful emergence from the ravages of National Socialism and cold war confrontations they had witnessed, was impossible to ignore. The neo-Marxist critique of *Spaetkapitalismus* (the advanced industrial state) has been a second decisive challenge. Furthermore, a great debate between Niklas Luhmann and Habermas addressed the questions of functional systems or dialectical analysis which still occupy the social sciences in West Germany. Yet all the while, the intellectual ferment generated by these conflicts also left its imprint on the other, more conventional pursuits of German political scientists who have been producing better and more solid monographs on parties and institutions than ever. I shall attempt to review representative works on various subjects, but the reader must understand that in each category of book reviewed there are many more that are equally interesting.

---

[2] "The Development of Empirical Political Science Research in West Germany," *Comparative Political Studies,* 6, No. 3 (October, 1973), 380–391. Cf. the simpleminded dichotomy between empirical political science and historical-institutional-philosophical approaches underlying the content analysis of David Pfotenhauer, "Conceptions of Political Science in Germany and the US, 1960–1969," *Journal of Politics,* 34, No. 2 (1972), 555–601.

[3] For further names and details, see Peter Christian Ludz, "Focus on Human Behavior: The present state of sociology and political science in the Federal Republic of Germany," *Bildung und Wissenschaft,* No. 10–74(e), available through Inter-Nationes, Bonn.

[4] Among von Beyme's recent books are *Die politische Elite in der Bundesrepublik Deutschland* (München: Piper, 1971); *Interessengruppen in der Demokratie* (München: Piper, 1974); books on Italy and on Spain, and his monumental comparative study of European parliamentary systems. See also his new series of translations from German political science with Sage Publications.

[5] Von Beyme, *Die politischen Theorien der Gegenwart* (Munich: Piper, 1972), esp. pp. 320–322. To be sure, there is more separation than cross-connections among the three theoretical schools when we consider individual scholars and subfields and, especially, if we formulate the "approaches" and "basic concepts" with the caution befitting a sociology of sociology and political science.

The Challenge of Democratization

"In every segment [of our society]," Wilhelm Hennis[6] said in an address in 1969, "the general tenor of all demands for change in our social environment can be summarized most concisely in the word 'democratization.' We can state that it epitomizes the most universal socio-political demand of our time." He went on to enunciate what can be considered the consensus underlying two decades of the democratic state of Bonn, namely a pride in a notion of democracy based on Western liberal constitutionalism and belligerently opposed to Nazi as well as Communist totalitarianism. This sense of pride also includes the consensus between the two largest parties, CDU and SPD, on this democratic system, a consensus that "has only recently been challenged as a lack of genuine alternatives." The sweeping demands for "democratization," however, went far beyond political democracy, in the political parties, in local government, or in public administration, and addressed themselves to "the power structures" of schools or universities, business enterprises, and the family.[7] The prevailing German practices in all of these institutions were frequently characterized by the rebels as typical remnants of Germany's authoritarian past.

Hennis's major argument against the advocates of "democratization," at least in 1969, was a flat rejection of the indiscriminate use of the concept of democracy outside of the political sphere. Drawing upon Aristotle and the whole pre-Rousseauean tradition of political philosophy, Hennis argued that it is a fundamental error to confuse the social order of a family or any other nonpolitical organization with the *political* order and its constitutional-democratic remedies. The demands for democratizing the structure of the traditional German university by giving representation in university governance to the students and to the nonprofessorial academic staff *(Mittelbau)*, as well as the professoriate, naturally occupied a prominent place in Hennis's critique of the demands for democratization. But he was content to point out that this demand for representing these three elements equally *(Drittelparität)*, had corporatist overtones rather than those of a model case of extrapolitical democratization. "Democratizing" the nonpolitical sphere, to him, was a sure way of politicizing schools, universities, economic production, or the family. "Listening to all parties, participation, even codetermination should be practiced wherever possible – but none of this could be legitimized with 'democracy.' "[8]

The controversial nature of the concept of democracy is discussed with more detachment, but along essentially similar lines, by Fritz Scharpf (Konstanz), who received some of his legal training and teaching experience at Yale Law School. Scharpf also locates the Western understanding of democracy in its historical context of antifascism and the cold war, and then relates normative democratic theory to different concepts of the political system. Noteworthy are Scharpf's well-aimed asides directed at the contemporary German critics of parliamentary democracy, who often borrowed their ammunition from Carl Schmitt and other authoritarian jurists and theorists of the state. It is a cheap shot at the reality of representative government today, he states, to judge it by the standards of 19th-century liberalism and thus to condemn it for having fallen prey to party discipline, lobbies, executive dominance, and public relations.[9] The author thoroughly discusses the American debate over pluralism and "democratic elitism" whose critical attitude has been eagerly adopted by German neo-Marxists frustrated by the resistance of their own pluralistic system to radical change.[10] After wrestling bravely with the paradoxes of political participation, the author concludes that the complex nature of contemporary democracies requires a complex democratic theory which is "empirically informed" *and* normative, but not utopian. It must spring from the kinds of historical traditions and situations that created a responsive two-party system capable of absorbing a pluralism of goals and interests in a centralized and authoritative procedure for decision making.

---

[6] "Demokratisierung, zur Problematik eines Begriffs," reprinted in Hennis, *Die missverstandene Demokratie* (Freiburg: Herder, 1973), 26–51, at pp. 26–27.

[7] Hennis supplies a good sampling of citations from the press and notable documents, such as the Frankfurt (Friedeburg) memorandum of several professors concerning principles for a new university law and manifestoes of the German SDS (Sozialistischer Deutscher Studentenbund). See also the discussion in von Beyme, *Die politischen Theorien*, pp. 210 and 218ff.

[8] Hennis, p. 47. See also his *Die deutsche Unruhe* (Hamburg: C. Wegner, 1969).

[9] *Demokratietheorie zwischen Utopie und Anpassung* (Konstanz: Universitätsverlag, 1970), p. 23.

[10] *Ibid.*, p. 55. See also, for example, J. Agnoli and P. Brückner, *Die Transformation der Demokratie* (Frankfurt: Fischer, 1968), and Scharpf, *Die politischen Kosten des Rechtsstaates* (Tübingen: Mohr, 1970).

Von Beyme's review of the bitter debates over the concept of democracy since the beginning of the protest movement carefully disects the various models of democracy historically and analytically. Von Beyme stresses particularly the democratizers' emphasis on participation in the existing structures of state and society rather than on the old ideal of self-governing communes. Thus he sees particular significance in the interest of political scientists such as Frieder Naschold[11] in the democratization of political subsystems such as parties and mass organization and in the organizational and communications aspects of subsystem participation and efficiency. But the increased participation should not keep the organizations and agencies from increasing their effective output at a time when the public makes ever greater demands on governmental services.[12] Participation also has to take account of a number of other peculiarities of the organizations to be democratized, such as the multiple levels and hierarchies of status which often defy the simple dichotomies so popular with the neo-Marxists. Von Beyme shies away from Naschold's assertion that participation increases meaning in the life of the individual or that it is an end in itself. The author concludes that democratic theory must avoid the extremes both of romanticizing participation *and* of seeing absolute limits of participation in the old division of state and society.[13]

The Great Debate: Luhmann vs. Habermas

The democratization issue had barely gotten under way, when there ensued a great intellectual debate which has enthralled large numbers of students and scholars. Much of its significance obviously stemmed from the upheaval at German universities in the late 1960s and beyond – an upheaval which put the two antagonists on opposite sides of the fence. Niklas Luhmann, with his sociological approach, had filled a crucial gap in the field of jurisprudence.

He had also written on the sociology of organizations.[14] His books *Soziologische Aufklaerung, Aufsaetze zur Theorie sozialer Systeme*[15] and *Legitimation durch Verfahren*[16] are particularly relevant to "the great debate." In the former, he addressed himself critically to the writings of the critical-analytical (Frankfurt) school, leading some to comment that his "sociological Enlightenment" was really a *Gegen-Aufklaerung* (counterenlightenment), an attempt to enlighten the academic public about the "Enlighteners" of the Frankfurt School and their intellectual manipulations. In *Legitimation durch Verfahren*, Luhmann argued that in a complex modern society, legitimacy is produced by "correct procedures" which, because of the complexity of bureaucratized society, make for public acceptance of the decisions of public agencies regardless of the motives or persons involved. Thus Luhmann's conservative "social technology" clearly made him the defender of the established system against all challengers and democratizers.

Jürgen Habermas had long been recognized as a chief figure of the Frankfurt School by virtue of his writings on sociological theory and politics.[17] He became identified with the movement for German university reform as one of the signers of the Friedeburg Memorandum on reforms in 1969 and through his writings.[18] The great debate was initiated by him and took the form of a joint seminar at the University of Frankfurt in about 1969. From this issued the co-authored book *Theorie der Gesellschaft oder Sozialtechnologie – Was Leistet die System-*

---

[11] See Frieder Naschold, *Organisation und Demokratie* (Stuttgart: Kohlhammer, 1969), and, by the same author, *Probleme der Demokratie* (Opladen: Westdeutscher Verlag, 1971).

[12] See von Beyme, *Die politischen Theorien*, pp. 218–220.

[13] *Ibid.*, pp. 223–226.

[14] Niklas Luhmann's other writings include *Theorie der Verwaltungswissenschaften* (Berlin: Grote, 1966), *Funktionen und Folgen formaler Organisation* (2nd ed., Berlin: Duncker & Humblot, 1972), *Rechtssoziologie* (Hamburg: Rowohlt, 1972), *Rechtssystem und Rechtsdogmatik* (Stuttgart: Kohlhammer, 1974), and others cited below.

[15] Luhmann, *Soziologische Aufklärung*, 3rd ed. (Opladen: Westdeutscher Verlag, 1973).

[16] Luhmann, *Legitimation durch Verfahren* (Neuwied: Luchterhand, 1969).

[17] See especially Habermas's *Strukturwandel der Oeffentlichkeit*, 3rd ed. (Neuwied: Luchterhand, 1968); *Theorie und Praxis, Sozialphilosophische Studien*, 3rd ed. (Luchterhand, 1969), (available in English as *Theory and Practice* [Boston: Beacon Press, 1969]; Beacon Press also published his *Legitimation Crisis* [1975]). See also his *Erkenntnis und Interesse* (Frankfurt: Suhrkamp, 1968).

[18] Habermas, *Protestbewegung und Hochschulreform* (Frankfurt: Suhrkamp, 1969); *Technik und Wissenschaft als Ideologie*, 2nd ed. (Frankfurt: Suhrkamp, 1969); and *Zur Logik der Sozialwissenschaften* (Hamburg: Zerschlagt das burgerliche Copyright, no date, 1969?).

forschung?[19] which has already sold the astounding number – for the German academic market – of more than 100,000 copies and given birth to several supplement volumes in which other scholars have argued the merits of the Luhmann-Habermas debate.[20] Lest the reader conclude that this debate was a dramatic confrontation between the revolutionary demands for *Systemveraenderung* (system change) in Western Germany and Luhmann as the "great white hope," we should note that there was rather less to the debate than meets the eye. The two principals, in fact, became quite friendly and continue to meet and to exchange their manuscripts for comment.[21]

The book of the great debate, *Theorie der Gesellschaft oder Sozialitechnologie – Was Leistet die Systemforschung?* consists of an essay on systems theory and on meaning by Luhmann, an elaborate critique of all of Luhmann's writings by Habermas, and a rebuttal by Luhmann. Luhmann's systems theory is broadly philosophical and owes much to Edmund Husserl's phenomenology as well as to Hegel, Max Weber, Heidegger, and Bertalanffy. It sets off the social system (and subsystems) from the outside world by boundaries at which the complexity of the world is "reduced" by the selective human understanding that creates systems of meaning *(Sinn)*. As societies evolve toward greater complexity, they separate persons from the increasingly differentiated and interrelated roles which constitute their structures and expectations of behavior. Luhmann finds Talcott Parsons's general theory of action problematic as a theory of the social system and prefers instead to base his systems theory on subjective meaning in the sense indicated above, as the "form" in which people selectively "experience" certain aspects of the world. Their "reduction" of the complexity of the outside world operates by excluding some of the many possible alternative systems of meaning, thus "constituting" their particular social system.[22] Luhmann relates meaning further to problem-related *functions* in decision processes[23] and calls his approach "functional-structural" with appropriate stress on functions.[24]

Habermas prefaces his critique with "preliminary remarks on a theory of communicative competence," a seemingly esoteric essay on linguistic theory which suddenly turns into a pointed discussion of democratic legitimacy and consensus. True discourse among rational equals, without manipulated barriers to communication, the author appears to say, is the only way to legitimize action on behalf of the community. "Communicative action" presupposes rational understanding in the people affected by it, and "the expectation of [its] legitimacy is based on the assumption that the general principles of [such action] must be capable in the eyes of the actor of justification by uninhibited . . . discussion" (p. 119). Ideologies, for example, can be barriers to communication that make a fiction of the rational individual and create a false sense of legitimacy. No genuine consensus can be formed without a common sense of truth, a rational ability to distinguish reality from appearance, and an absence of coercion of any sort. This is the Kantian foundation of Habermas's "critical social theory."

According to Habermas, Luhmann's central thesis, namely that functional analysis is the only admissible route toward rational decision is wrong, and his formulation of legitimacy in highly complex societies is a technocratic and scientistic attempt to stifle all tendencies toward democratization. Habermas further charges that Luhmann does not posit the rational individual as the source of legitimacy, but recognizes instead all kinds of other sources such as systems efficiency. Habermas also doubts that a system theory based on meaning *(Sinn)*, can ever achieve the precision of theories of mechanistic or organic systems. He questions in particular Luhmann's prescription for *ad hoc* crisis management divorced from any analysis of the causes of social crises.[25] But even where Luhmann applies his macrotheory of the social system, it appears to this writer, it can serve to manage crisis and conflict just as the macrosocietal theory of the New Left is designed to aggravate them. Either way society is "steered" by a macrosocietal understanding of its structure and process. The two antagonists, in fact, also share, with a bow to Husserl's phenomenology, a skepticism about the universal claims of a behaviorally oriented social science which does not recognize the structuring of society by meaning *(Sinn).* Their concepts of ideology, on the other hand, differ in that Luhmann's is based on systemic function while Habermas's is based on the deception and manipulation of the individual, who is thereby prevented from grasping his or her true interests. The nub of their disagreement, in any case, remains Luhmann's technocratic concept of legitimacy:

---

[19] *Theorie der Gessellschaft oder Sozialtechnologie* (Frankfurt: Suhrkamp, 1971).

[20] See, for example, Franz Maciejewski, ed. *Theorie der Gesellschaft oder Sozialtechnologie,* Theorie-Diskussion, Supplement 2, Beitraege zur Habermas-Luhmann-Diskussion von Wolf-Dieter Narr et al. (Frankfurt: Suhrkamp, 1974).

[21] Since 1971 Habermas has been doing research and theoretical work on change and crisis in postindustrial societies at the Max Planck Institute in Starnberg together with several other representatives of the Frankfurt School. In these pleasant surroundings he is nonetheless painfully cut off from normal student and academic contacts. The University of Munich denied him a teaching appointment and has refused him the normal courtesies of academic cooperation.

[22] Meaning, according to Luhmann, is not simply transmitted but mutually actualized in intersubjective communication. It is carefully distinguished from mere information and distinguished from simplistic models of communication. On the "reduction of social complexity," see also Luhmann's *Vertrauen, Ein Mechanismus der Reduktion sozialer Komplexitaet* (Stuttgart: Enke, 1968).

[23] On Luhmann's concept of function, see his "Funktion und Kausalitaet" (1962), reprinted in *Soziologische Aufklaerung,* pp. 9ff.

[24] For a systematic review see also Heinrich Busshoff, *Systemtheorie als Theorie der Politik* (Pullach: Verlag Dokumentation, 1975).

> No one is in a position to form convictions for all the decisions [facing his society]. A concept of legitimacy by conviction misreads the extreme complexity, variability, and contradictory nature of the subjects and premises of the decisions that need to be dealt with in the political-administrative system of modern societies. This complexity of modern societies requires the *generalization* of acceptance of decisions, less by well-motivated convictions than by an acceptance free of motive and independent of the idiosyncracies of particular individuals.[26]

To Habermas, this is an obscurantist ideology which battens on the depoliticization of the public and reverses all representative principles of democracy – in short, it is a "social technology" as the title of the book puts it.

There is no need to go into great detail about Luhmann's rebuttal: Habermas's critique, he says, is thorough and sophisticated but far too dichotomized into pro- and anti-establishment perspectives to do justice to the complexity of modern social and administrative problems. The kind of democratic discourse among rational individuals on which Habermas would like to base legitimacy and even truth is a myth, says Luhmann, and bears little resemblance to what people in modern society actually engage in when they speak with each other.[27] Habermas's whole approach to legitimacy, in fact, stems "from a time which was not yet familiar with the problem" of communication in complex modern society (p. 355), presumably the Enlightenment or at the latest, the nineteenth century. Luhmann does not mean to question the historic quest for individual emancipation and democratization so much as their traditional link to individual rationality or the triumph of reason. Functional-structural systems theory, the author hopes, is beyond these traditional points of reference and beyond the confrontations of establishment and opposition. The "great debate," however, has remained close to these important questions of modern political theory in the years it has continued at German universities.

The Neo-Marxist Challenge

The rapidly changing directions of left-wing opposition at German universities produced a fine vintage of neo-Marxist theories after most of the activists had wandered off into the partisan Young Socialists *(Jusos)* or Young Democrats*(Judos).*[28] Any discussion of neo-Marxist thought in Germany requires an explication of *Spaetkapitalismus* (advanced capitalism), as well as of the theory of "state monopoly capitalism" *(Stamokap)* and other criticisms of advanced capitalism.

The label *Spaetkapitalismus* for organized, interventionist, or advanced capitalism is problematic in suggesting an all-too-familiar, transparent kind of word manipulation: "capitalism in its late phase" simply *asserts* its imminent demise, instead of demonstrating the necessity of such a development. Translating the bad label as "late capitalism"[29] compounds the problem to ludicrous proportions, for the mission of all these neo-Marxists is precisely to explain why the patient, in spite of recurrent crises and afflictions, is still as far from being "the late" capitalism as ever. A second key concept is that of "crisis," which occurs in many titles of books and articles of this genre today. A much-quoted collection by Martin Jaenicke[30] even purported to present a new area of comparative "crisis research," by roaming far and wide among explanations of the "crisis" of political decision systems (Deutsch), developing areas (Rittberger), international systems (Hermann), and capitalist systems (Offe, Altvater), with predictable analytical vagueness. It is particularly disappointing to see the carelessness with which a powerful tool of comparative analysis, such as Marxist theory, is used by today's neo-Marxists, not to mention their seeming unwillingness to compare in detail the striking differences among various advanced capitalist systems and the effect of these differences on system legitimacy.[31]

---

[25] Luhmann proposed this in *Zweckbegriff und Systemrationalitaet, Ueber die Funktion von Zwecken in sozialen Systemen* (Tuebingen: Mohr, 1968), p. 224. Theories of crisis are particularly important to critical analytic theory as will be seen below.

[26] Luhmann, *Legitimation durch Verfahren*, p. 32. Earlier in the same book, the author speaks of a generalized willingness to accept even decisions of undetermined content within certain margins of tolerance." *Ibid.*, p. 28.

[27] Here Luhmann cannot pass up the temptation of describing in an aside the dogmatic and contentious nature of discussions in "supposedly free, democratic groups dedicated to the critique and reform of social ills." *Theorie der Gesellschaft oder Sozialtechnologie,* p. 340; see also pp. 391–392 and 399–401.

[28] On these young groups see below, and the sources cited in footnote 45.

[29] See Habermas's note "What Does a Crisis Mean Today? Legitimation Problems in Late Capitalism," in *Social Research* (reprint), no date.

The various neo-Marxist schools differ also in how they visualize the crisis of *Spaetkapitalismus* which, after all, is the source of their vitality, of their legitimacy, and of their own theory-making. The chief differences relate to the relationship between the economic and the political system. Orthodox and neo-orthodox Marxism and certain *Stamokap* (state monopoly capitalism) theorists continue to expect the great crisis chiefly in the economic realm,[32] for they view the state as merely the political manifestation of the economic contradictions and the instrument of individual capitalist interests. At the other end of the scale are those such as Claus Offe and Habermas who foresee the coming crisis as a political breakdown, a critical loss of legitimacy of the state and the dysfunctional politicization of the masses, led by revolutionized students and intellectuals rather than by the leaders of the proletariat.[33] The intimate interpenetration of economics and politics no longer permits the neo-Marxists to deny the importance of the state or its relative autonomy as a factor that at least temporarily has been able to save capitalism from its long-predicted collapse. On the other hand, some neo-Marxist writers exhibit at times a curious nostalgia for the simple days of nineteenth-century capitalism when Marxist analysis was not yet bedeviled by an interventionistic state.

Bernd Guggenberger, a very perceptive critic of the neo-Marxist literature, directs attention to the striking differences among the prescriptions of the various schools. Even where these schools agree, say, on the role of the financial oligarchy and the great monopolies in directing the state, their proposed solutions differ in telling ways. Guggenberger has little patience with the East German school,[34] which, according to him, merely proposes to replace the oligarchic, parasitic elite there with another more benevolent, even vaguely democratic technocratic elite.[35] He thinks more highly of the French Communist theoreticians who would follow up the takeover with a decentralization of economic decisions to producers' organizations and with the democratization of planning, a program similar to the ideas of industrial democracy achieved through workers' councils. In the prolonged discussions among *Stamokap* theorists and various *gauchiste* groups of the German Young Socialists *(Jusos)*,[36] all the participants have striven in their own ways to "turn around" the West German SPD while it is still in control. Some prominent *Jusos* have questioned especially the simplistic "undialectical" assumption that a complex social organization such as the state can ever be a mere instrument in the hands of anybody.[37] Hence the *Jusos*'s "double strategy" combined the political mobilization of the public "at the base" with attempts to influence policies and the distribution of power at the top. The inevitable linkage of these theories to the political strategies of the day make these perspectives more perishable than the reinterpretations made by Offe and Habermas, who refrain from proposals of strategy and action.

---

[30] Jaenicke, ed., *Herrschaft und Krise* (Frankfurt: Suhrkamp, 1972). This book is the outcome of a 1972 conference on "crises" in Berlin.

[31] Habermas's use of analogy between the crisis of *Spaetkapitalismus* and the body's crisis or the "crisis" in Greek tragedy is hardly a tool of social science analysis.

[32] A good guide through the current German literature is Bernd Guggenberger, "Oekonomie und Politik – die neomarxistische Staatsfunktionenlehre," *Neue Politische Literatur* (1974), 20, No. 4, pp. 425–471 whose work is discussed more fully below. See also von Beyme, *Die politischen Theorien,* pp. 64–87, and the sources cited there. Examples of this literature are Claudia von Braunmuehl, ed., *Probleme einer materialistischen Staatstheorie* (Frankfurt: Suhrkamp, 1973); Wolfgang Muller and Christel Neusuess, "Die Sozialstaetsillusion und der Widerspruch von Lohnarbeit und Kapital," *Sozialistische Politik II,* (June, 1970), no. 6/7, pp. 4–67; and Ernest Mandel, *Der Spaetkapitalismus* (Frankfurt: Suhrkamp, 1972).

[33] Guggenberger also suggests, as a third, in-between group, writers such as R. Miliband, N. Poulanzas, A. Gorz, P. Sweezy, and P. Baran.

[34] Guggenberger points out that their theories of West German imperialism and capitalism originated among the members of the Institute of Social Sciences of the Central Committee of the East German Communist Party (SED) and that their advocacy by the Soviets, the SED, and the West German Communists (DKP) is strongly colored by the current popular front strategy.

[35] The avoidance of true democratization, of course, corresponds to the bureaucratic-authoritarian model of Soviet and East German socialism. See also Margaret Wirth, *Kapitalismustheorie in der DDR* (Frankfurt: Suhrkamp, 1972).

[36] Guggenberger covers them well in his *Wem Nuetzt der Staat? Kritik der neomarxistischen Staatstheorie* (Stuttgart: Kohlhammer, 1974), pp. 67–75.

[37] As the author points out, there are significant links here with the theories of the state of Hermann Heller of the Weimar Republic whose writings have recently been reissued. Guggenberger, *Wem neutzt der Staat?,* pp. 135–136, fn. 210 and 213.

Claus Offe's *Strukturprobleme des kapitalistischen Staates*[38] is a highly original political sociology of *Spaetkapitalismus* which consists of several separate essays on various aspects of the relationship between the political-administrative system and economic and social policy. It has filled a major gap between the literature on planning and administration on the one hand and the insufficiently state-oriented neo-Marxist literature on the other. Offe's thorough acquaintance with American social science and systems theory and his critical-analytical training make him a potent analyst of critical problems of legitimacy, technocracy, and public policy in advanced capitalism.

The strength of Offe's and Habermas's "revisionism" (as compared to the *Stamokap* and neo-orthodox Marxists) lies in its sophisticated analysis of the weaknesses and incongruities of the "political-administrative system" of contemporary democracies. Its weaknesses, on the other hand, owe much to the cumbersome imprecisions of the Master – Marx himself – and to the circuitous logic that sometimes characterizes his disciples. Thus they may at once open themselves to accusations of taking liberties or being selective with the writings of Marx and Engels, and of taking them too seriously as a point of departure of actual historical development.[39] The reader, for example, may find Offe's "dynamic interpretation" of Marx unacceptable, or may balk at Offe's treating the "deviations" of actual capitalist development (such as the rising role of nonproductive labor) from the original nineteenth-century model as further "contradictions" of capitalism. But thereby hangs Offe's tale of how the contradictions in the structures of evolving *Spaetkapitalismus* will in time exceed the capacity for "social integration" of a society.

By combining the integrative-system approach with the neo-Marxist theory of structural conflict, Offe is able to deal with the resistance of pluralistic systems to structuralist analysis. The legitimacy of the early capitalist state, according to Offe, required none of the current efforts of the regimes under *Spaetkapitalismus* to ingratiate themselves with the public. Even the market mechanism and the relation between capital and labor provided a kind of imminent social justice which today has been usurped by the administered prices and arbitrary interventions of the state. The state of *Spaetkapitalismus* thus has all of the old social conflicts and many new ones lying undigested in its greatly extended belly. Its ability to steer society knows few limits, and yet its need for legitimizing its steering power (and specific policies) was never greater. Technocracy and planning, Offe argues along Habermasian lines, have destroyed the bourgeois institutions of public consensus and thereby their own source of legitimacy just when they needed it the most. Their new forms of legitimizing strategies are short-run, and the class or other bias of the policies easily seen through. *Spaetkapitalismus* can give the masses bigger handouts and better living conditions and yet forever falls behind their rising demands. Qualitative emancipation rather than the "reproduction of labor" or proletarian impoverishment is the well-springs of public discontent.

Habermas in his *Legitimationskrisen im Spaetkapitalismus*[40] goes beyond analyzing the level of technological and functional mastery to discuss the crises of *meaning* in modern industrial life, including what Daniel Bell and Herbert Marcuse have called the "cultural crisis" of capitalism. Thus he also goes beyond Marx and the orthodox premises of Marxism toward additional definitions of "contradictions" and "crises": crises of the ecological balance, of human alienation, of the international system, and most of all, of the legitimacy of the political-administrative system. The state has a hard time meeting all the demands, he argues, since it has to act as the "ideal capitalist" representing many particular and frequently conflicting capitalist interests, as the state capitalist with enterprises and responsibilities of its own, and as the representative of public interests and demands readily backed up at the ballot box. Short of the "administrative manufacture of meaning," or deceit by manipulation of opinion, the state will find the cultural sphere rather resistant to its quest for legitimacy. It is even less able to safeguard the values of bourgeois culture which once upon a time were the values behind its own smooth operation. The crisis of bourgeois culture, exemplified by fascist regressions, scientism, and modern art, according to Habermas, also erodes the cultural basis of capitalism, leaving the field to counter-cultural revolt.

---

[38] Offe, *Strukturprobleme* (Frankfurt: Suhrkamp, 1972).

[39] The extraordinary spell which Marxism has cast over many young German intellectuals can be illustrated by an incident during a conference on comparative fascist movements in Bergen, Norway, in 1974. There was great puzzlement over the fact that in many fascist movements transport workers were among the few proletarians to join when a young German participant interjected: "I have a plausible explanation. These transport workers were at the intersection between the process of production and the process of circulation. *Da musste doch etwas schiefgehen!* (So something was bound to go wrong.)"

[40] See also the critical remarks of Wolfgang Fach about Habermas's "legitimation logic" in *Zeitschrift für Soziologie*, 3 (1974), 221ff. Another one of the landmark debates involving Habermas and the late Theodor Adorno was the discussion over the role of "positivism" in sociological method which pitted Karl Popper and Hans Albert against the Frankfurt School. See also Hans Albert and Ernst Topitsch, eds., *Werturteilsstreit* (Darmstadt: Wissenschaftliche Buchgesellschaft, 1971).

The reader must not imagine that the wave of West German prophets of the New Left went unchallenged. Countless debates and counter-manifestoes, as well as personal struggles in the SPD, at the universities and schools, and among the bureaucrats have been waged in order to "excommunicate the excommunicators." The New Left has mounted campaigns against SPD politicians and other prominent figures of the establishment, and now the courts, state government administrators, and educators are conspiring to bar their left-wing enemies from positions in the system.

The Conservative Rejoinder

The last word on the intellectual ferment of our time in Germany, at least for the time being, fell to Schelsky, book with the revealing title *Leave the Work to the Others: The Class Struggle and Priestly Rule of the Intellectuals.*[41] To Schelsky, anyone engaged in "ideological critique" today is really trying to shift our attention from the problems of social change and group antagonism to questions of faith or meaning which ultimately tend to promote rule by the "ideology-producing class" – the intellectual high priests – over those who produce goods and services. With a bow to Max Weber and Georges Sorel, the author draws contemporary parallels to hierocratic rule. The expectations of salvation and the perception of current miseries preached by the "intellectuals"[42] are capable, he says, of destroying all dedication to working in the here and now of the industrial societies or the Third World. Instead they mobilize the masses against their alleged oppressors and in pursuit of an elusive millennial goal which is strictly of their own making. The current miseries are mostly "borrowed" from faraway situations such as "death in Vietnam," repression in Spain, or hunger in Africa, and transferred emotionally to the tender sensibilities of the upper-middle classes to motivate the intellectual grab for power.

Schelsky treats the rationalistic critical method of the Frankfurt School as harshly as he does any political or university reform movement. He describes them all as a species of Anabaptist revivalism generating new meanings and salvational goals, promising to free people from the daily "pressure to achieve," claiming an absolute monopoly on truth, and attributing a "false consciousness" to anyone who does not agree with them. Since the producers of ideology, or meaning, are not subject to the same instant refutation as are people involved in production and marketing, there is no natural corrective for producing bad ideology and, at best, only the competition of different ideologies which may be no better. The ideological goal may be an elusively formulated "democratization" or the battle of "intellectualized women" over West German abortion laws (Criminal Code Paragraph 218). It may involve seeing the enemy in science and technology, in the governing elites, or in other established authorities. But it is always based on the postulation of an enemy and on a claim to leadership and power. Schelsky particularly deplores the defamation of the achievement motive among the German intellectuals (following Herbert Marcuse). This German "intellectual class," says Schelsky, seeks to control the leisure-time activities of society. Its strength is based on its practical monopoly of socialization (education) and information, on its power over words, and over the language. It attempts to conquer society by the "transvaluation of words," by changing the commonsense meaning of such terms as "democracy" or "public opinion."

Schelsky's rejoinder ends with a bitter diatribe against the "treason of the intellectuals" who have tied their services to society to a bid for power and respond with a "great refusal" when this bid is not readily granted. His major target is the sociologists and political scientists (or, in German nomenclature, the political sociologists), in other words, his own profession. His criticism is a systematic assault on the foundations of the discipline itself, an *"Antisoziologie"* aimed at "sociological enlightenment." He also comments searingly on the "enlighteners" in education, among the purveyors of the social gospel, and on *engagé* writers and journalists. In Schelsky's angry vision, the news magazine *Der Spiegel* becomes a "class struggle sheet" and Nobel prize-winning author Heinrich Böll the "cardinal and martyr" of social salvation.

---

[41] Schelsky, *Die Arbeit tun die anderen: Klassenkampf und Priesterherrschaft der Intellektuellen* (Opladen: Westdeutscher Verlag, 1975). The author, among other things, accuses German political science and sociology of a one-sided emphasis on power and conflict research (especially peace research) rather than on democratic legitimacy, pp. 25–26.

[42] Schelsky devotes considerable space in his book to a systematic description of seven categories of intellectuals, ranging from the technical and administrative intelligentsia, artists, social scientists, teachers, journalists, and public relations people to the priests of established religion, pp. 98–117.

Studies of Institutional Legitimacy

The challenges of neo-Marxism and "democratization" to the legitimacy of West German institutions, and Schelsky's doubts may suggest that the system is on the verge of collapse, but this is not the case, nor has there been any reduction in the quantity and quality of institutional studies. On the contrary, as a few selected examples will show, the current West German literature on political parties and institutions can easily stand comparison with the flowering of political thought and of the behavioral school in Germany. As the editors of a major symposium on the West German party system put it, the core problem for investigation is "how far the democratic challenge of the extraparliamentary opposition (of 1966), the student movement (of 1967/1970) . . . and especially of the youth organizations of the major parties . . . has been taken up and translated politically by the party system."[43] Since the major parties of the West German "party state" wield a monopoly of power, the literature on German parties[44] had already tended to raise questions of internal party democracy in connection with the debates of the early 'sixties over the law implementing the constitutional mandate of political parties, (art. 21) the electoral reform, and public campaign financing.

When the crises of the late 'sixties began to pile up, therefore, students of German parties were already deeply involved in probing relevant aspects of the internal life of the parties — their social composition, the activities and attitudes of their members, and the career paths of party politicians. These earlier projects are well-represented in the Dittberner and Ebbighausen symposium, as are studies of interest groups within the two largest parties, e.g., labor in both the CDU and SPD, and the Economic Council (business) of the CDU. They are, moreover, integrated with a searching and fairly exhaustive analysis of the Young Socialists *(Jusos)*, Young Democrats (FDP), and the Young Union (CDU),[45] as well as the newly (1968) admitted Communist party (DKP). All party studies, of course, are quickly out of date. Within a few years, much of the "system-changing" potential of the *Jusos*, and of the DKP and more recent groups, appears to have spent itself, even though the wave of politicization they represent is still felt in local mass actions and "citizens initiatives." Juergen Dittberner's cautiously expressed judgment that the West German party system has shown astounding stability throughout the upheaval of the late 'sixties and early 'seventies is likely to endure, even if recent elections did not usher in a period of SPD dominance comparable to Adenauer's dominance in the late 'fifties. In this respect, then, stability under fire spells legitimacy, because it demonstrates that the procedures of formal democracy have indeed taken the place of the traditional sources of legitimation in Germany. The pluralism of groups and interests which the Weimar Republic was unable to absorb is now well within the integrative capacity of the system. Even the vulnerability to economic crisis and political extremism which has so often characterized West German democracy has not so far imperilled the survival of constitutional democracy there.[46]

---

[43] *Parteiensystem in der Legitimationskrise,* ed. Juergen Dittberner and Rold Ebbighausen (Opladen: Westdeutscher Verlag, 1973), p. 31.

[44] The introduction by Ebbighausen carefully puts the state of German party studies into the appropriate context of German postwar history and international comparative party studies, including the contributions of American scholars.

[45] The activities of the Young Socialists, as the paper about them points out, generally remained well within the traditional range of issues in the SPD, but strengthened the Old Left elements at the expense of the dominant center-right coalition which has supported the SPD leadership. On these young partisan organizations, see also Helmut Bilstein et al., *Jungsozialisten, Junge Union, Jungdemokraten* (Opladen: Leske Verlag, 1971). Attention should also be drawn to the excellent collection of materials on West German parties by Ossip K. Flechtheim, ed., *Die Parteien in der Bundesrepublik* (Hamburg: Hoffman & Campe, 1973).

[46] Dittberner and Ebbighausen, *Parteiensystem,* pp. 469–475. Dittberner, however, still attributes a considerable share of the integrative capacity to the economic prowess of the system to generate and distribute prosperity, which implies that a more massive economic failure than has been experienced so far in West Germany could be fatal. On the other hand, the author leaves little doubt about his low estimation of the neo-Marxist analysis of the structural potential for crisis, pp. 481–488.

The faith in the institutions of West Germany also shines through in varying degrees in works such as the studies of German federalism of Heinz Laufer, who seems less concerned that a political threat will arise than that West Germans may not understand and appreciate their federal institutions. He would like to see more information about the federal-state relations taught in the schools and transmitted by the media. His work on the federalism of the Federal Republic exemplifies the research of many well-trained young German political scientists and some older ones who share a firm grounding in constitutional law and administrative practice.[47] Laufer has also published research about other little-known institutions, such as the important, semi-official *Laender* offices in Bonn, which constitute a link between the *Bundesrat* delegations, the *Land* governments, and the federal government.[48] This particular study skillfully combines institutional knowledge, constitutional history, and interviews to penetrate the bureaucratic mysteries of the *Bundesrat* type of federalism, in which lobbying practices and administrative relations are inextricably intertwined. Another example of such institutional studies is *The Head of State in Parliamentary Democracies*[49] by Werner Kaltefleiter, who is better known from his election studies. A classical comparative study, *The Head of State* compares the parliamentary systems of Great Britain in the 1930s, Sweden, Denmark, Norway, Belgium, France under the Third and Fourth Republics, Italy, Finland, Austria, and Weimar Germany in order to create a functional model for the interpretation of the office in the Bonn Basic Law. The Federal Presidency of West Germany embodies dangerous contradictions of function which so far have not surfaced only because its first incumbent, Theodor Heuss, took an exceedingly narrow view of his role and because Konrad Adenauer, in 1959, decided not to run for the office. The author went the painstaking, classical route of checking all his comparative material with colleagues in the countries in question. This is a typical example of the practical, or if you will, policy relevant, purposes of institutional comparison.

These institutional studies show neither the critical ideological bent of the challengers nor the subtle subversions of the spirit of political science research alleged by Helmut Schelsky. Even freer of bias is the symposium *The Second Republic* edited by Richard Loewenthal and Hans-Peter Schwarz;[50] this tome ranges over a comprehensive list of topics, from the origins, institutions, and parties of the Federal Republic to social change and a variety of policy topics. The editors and most of the contributors emphatically maintain that the Federal Republic, after a rather unpromising start, has developed a "new legitimacy" and, to everyone's surprise, has turned into a "political miracle" (Loewenthal). This view differs from the uncertain assessments made only a decade ago, when many observers saw in the Federal Republic a "fair-weather democracy lacking responsible democrats."

The Second Republic presents a succession of tableaux and scenarios, beginning with an analysis of the situation of occupied Germany by Hans-Peter Schwarz and detailing the curious mixture of traditional, especially bureaucratic, elements and the makings of a conservative, post-totalitarian democracy, German-style. Schwarz, who has written a classic work on the origins of the Federal Republic,[51] refutes much of the revisionist German literature and its claims that the conservative path of the Federal Republic was chosen in 1949 as a result of political intrigues and occupation pressures. It was the voters who opted against socialism and in favor of the West, thereby creating a new political entity and identity. The major party alignments actually antedated the republic, and their historical positions at the outset were undoubtedly crucial to the political development to come: the SPD in revulsion from Communist domination on the one hand and from German rearmament in NATO on the other; the CDU/CSU between the devil of the Nazi past of the national bourgeoisie and the deep blue sea of unprecedented European and trans-Atlantic entanglements. As prosperity grew and the cold war waned in the 'sixties, social and generational change gave birth to the new pressures that erupted into new forms of political extremism of the right and the left with the economic and political crisis of 1966–1967. The new ferment, including the "sociological enlightenment" of rebellious youth, tended to benefit the SPD at the expense of the CDU, and was probably accelerated by the change at the helm of the state.

---

[47] Laufer, *Das foederative System der Bundesrepublik Deutschland* (Munich: Bayerische Landeszentrale fuer politische Bildung, 1973) is noteworthy also for its exhaustive coverage of the financial aspects and cooperative practices of West German intergovernmental relations.

[48] Laufer and Jutta Wirth, *Die Landesvertretungen in der Bundesrepublik Deutschland* (Munich: Goldmann Verlag, 1974). He also wrote books on the parliamentary state secretary and on the constitutional Court.

[49] Kaltefleiter, *Die Funktionen des Staatsoberhauptes in der parlamentarischen Demokratie* (Opladen: Westdeutscher Verlag, 1970). A recent example of the author's election studies is his *Im Wechselspiel der Koalitionen* (with several co-authors, Cologne: Heymanns Verlag, 1970), which combines an extensive investigation of the internal and coalition policies of the parties with the analysis of the 1969 elections.

[50] Loewenthal and Schwarz, *Die Zweite Republic.* 25 Jahre Bundesrepublik Deutschland, eine Bilanz (Stuttgart: Seewald Verlag, 1974). The contributors include well-known professor-politicians like Alfred Mueller-Armack, Hans Maier, and Carlo Schmid, and many prominent academics such as Theodor Eschenburg, Karl D. Bracher, Rainer Lepsius, Rudolf Wildenmann, and Ernst-Otto Czempiel.

[51] Schwarz, *Vom Reich zur Bundesrepublik* (Neuwied: Luchterhand, 1966).

Where will the Federal Republic go from here, for its second quarter of a century? Loewenthal emphatically discounts the Cassandra cries of polarization and impending civil war from today's right-wing observers, just as he discounted similar left-wing warnings of a proto-fascist development of the Grand Coalition of a decade ago. Like his co-editor, he is confident of the democratic consensus among the major parties, and he feels this consensus is the guarantee of future legitimacy regardless of which party happens to have the popular mandate to carry out its choice of policies. He also hopes for "democratization" so long as it does not induce institutions to get out of the control of the democratic society, or even to be hostile to it. Schwarz, on the other hand, sees the key to future legitimacy in the continuation of the successful marriage between political democracy and a thriving industrial economy. He also expects the aversive experiences of Nazi and Stalinist totalitarianism to benefit West German dedication to democracy for at least another decade until generations take over who never witnessed either form of tyranny. Strong liberal democracies, he feels, can put up with a certain amount of anarchy and violence. Schwarz does not discount the crisis potential of the German division (or related aspects) or of a neutralization of Central Europe heralded by the European Conference on Security and Cooperation, nor does he make light of the dangers of a domestic political crisis, to be sure. In fact, he sketches several scenarios of a right- or leftward swing, and especially of a popular front of East German Communists and their West German allies among the DKP and its Young Socialist and New Left friends. But, aside from a Swedish type of development based on a moderate SPD majority in West Germany, none of the scenarios appears to be in itself capable of success without strong countervailing forces that would right the balance. It remains a matter of judgment whether the Swedish road would be a "road to serfdom." And so he ends the book on an upbeat of hope, as long as West German internal affairs remain undisturbed by worldwide crises.

Any attempt to assess a whole profession in a single review article is bound to give at best a partial view, no matter how wide the net is cast. In this case, the limited coverage and the state of war within the discipline cannot but leave a distorted and superficial image. The different schools of thought, the great debates over democratization and systematic approaches, and most of all the burning concern on all sides with some of the most basic issues of politics and political science – the kind that hardly ever get raised in our own compartmentalized discipline – make German political science appear vibrant and dynamic. It may not be logically satisfying to see the obvious noncongruence of the questions asked and the answers given by different schools, even though their methods have obviously become more sophisticated. The heated confrontations frequently generate more noise than real communication. Nevertheless, the sympathetic observer finds himself wishing that the years of upheaval in American political science had left more of this real concern with the vital issues of politics.

On Social Psychological Handy Work: An Interpretive Review of *The Handbook of Social Psychology,* Second Edition*

RICHARD M. MERELMAN
University of Wisconsin-Madison

How should one evaluate a collection of forty-five long articles on every aspect of social psychology from the behavior of "infra-human animals" to "laughter, humor, and play," from collective behavior to aesthetics? The reviewer cannot help but blanch at the variety of material available in these five volumes. An additional impediment is the passage of time. Although this new edition of the *Handbook* appeared in 1968–69, most of the articles were written years earlier, and most of these articles draw on *other* research conducted in the 1950s and early 60s. One is therefore hard put to know whether the "new Handbook" is really new at all – or at the very least how representative it is of social psychology today. In any event, the discerning contemporary reader will notice what appear to be significant gaps in the *Handbook.*

It should also be admitted that no human being can read 4100 pages of demanding text and the results of literally thousands of studies with unflagging attention, particularly when (as in the case of cognitive dissonance theory), he encounters many of the same considerations in overlapping contexts. Eventually one finds oneself wondering as much about one's own health as about what is being reviewed. Obviously, one urgently requires some guidelines.

Luckily, such guidelines become easier to devise once we realize that social psychology is hardly an esoteric field to most political scientists. Knowingly or unknowingly, every political scientist incorporates social-psychological assumptions into his research. The student of election campaigns applies social-psychological assumptions about attitude change and formulation; the student of legislative coalitions applies social-psychological assumptions about leadership behavior in small groups; the student of comparative politics applies social-psychological assumptions about the interplay of culture and personality. And so on, *ad infinitum.* The fact is that social psychology is the very skeleton of political science, the scaffolding upon which we hang our more specialized investigations. Because it seems reasonable to assume that the corpus of political science can be only as strong as its social-psychological skeleton, most political scientists can benefit from greater familiarity with social psychology; the new *Handbook* seems an obvious place to begin. Specifically, I will attempt to discover how well the *Handbook* familiarizes the political scientist with social-psychological ideas that might be of service to him or her in research and teaching.

The organization of the new *Handbook* lends itself to the sort of review I have outlined. Volume I explicates leading social-psychological theories, many of which underlie the various subfields of political science. Volume II deals with socio-psychological methodologies, many of which we have borrowed from social psychologists. Volumes III to V deal with the major substantive fields and findings of social psychology, many of which overlap the interests of political scientists. Therefore, I shall first discuss theory, then methodology, and finally, the substance of social psychology.

Since there is no one "best way" to evaluate a project as massive as the *Handbook,* my review strategy will be to extract as many examples of the *Handbook*'s uses as I can. I believe this to be the most fruitful way for political scientists and their graduate students to employ the *Handbook* in research and teaching.

Theory in Social Psychology

The current hierarchy of social-psychological theories is fairly accurately reflected by the relative quality of the articles contained in Volume I of the *Handbook*. Specifically, the best chapters in Volume One are those on cognitive theories, reflecting perhaps the dominance of these theories over those of the S-R learning theorists and of the Freudians.

**The Handbook of Social Psychology*, 2nd edition, eds., Gardner Lindzey and Elliot Aronson (Reading, Mass.: Addison-Wesley, 1968–69, 5 volumes. Price: $16.95 each volume.)

Zajonc's chapter on "Cognitive Theories in Social Psychology"[1] and Sarbin and Allen's piece on "Role Theory"[2] are the pick of the lot in Volume I. These two chapters well display the strengths of cognitive theory, of which perhaps the greatest is its middle-of-the-road stance between the environmentalism of classical learning theory and the residual instinctualism of the Freudians. Cognitive theorists have managed to turn the tension between person and context to their own theoretical advantage.

But there are two other advantages of cognitive theory which also stand out. First, as Zajonc shows, the cognitive approach has generated interesting, nonobvious propositions about recognizable people. Second, cognitive theorists have invented ingenious ways of subjecting their propositions to test. The result has been a closer blend of theory and data than that which has been achieved by either the learning theorists or the Freudians.

For comparison, consider the Freudian approach. At least as it is described by Hall and Lindzey,[3] psychoanalytic theory remains far too closely bound to the interplay between ego, superego, and id as they vie for supremacy within the mental economy. Hall and Lindzey's exposition is depressingly archaic and quite irrelevant to contemporary research in social psychology. And, of course, there is an additional issue of evidence, which constitutes a second basic problem with Freudian theory. As the Hall and Lindzey chapter suggests, psychoanalytic theorists have generally been unsympathetic to theory testing, a position which, rightly or wrongly, has not stood them in good stead with most social psychologists.[4]

In fairness, it should be said that there is more to psychoanalytic theory in social psychology than is hinted at by Hall and Lindzey. Their article poorly represents the place of psychoanalytic theorizing in contemporary social science. Only scant attention is paid to ego psychology in its many contemporary forms,[5] no attention at all is paid to Maslowian and other "third force" theorists,[6] and even Erikson's work is underplayed.[7] Strangely, Freud's own applications of psychoanalytic insights to sociopolitical phenomena are also generally overlooked.[8] Contemporary research into psychohistory suggests resurgence of Freudian theory in the social sciences.[9] Unfortunately, the inquisitive political scientist will find himself wondering how this could have occurred if he relies solely on the Hall and Lindzey discussion for illumination. Indeed, the Hall-Lindzey chapter is in some respects an illustration of how much psychoanalytic theory has had to *overcome* in order to attain its newly respectable position.

The problems of classical learning theory are very different from those of Freudian analysis. Unlike the Freudians, the early learning theorists positively reveled in applying their hypotheses to experimental test. But, again unlike the Freudians, the inventors of S-R theory found it difficult to generate ideas that would move their paradigm away from simple dyadic relationships and into the heart of group phenomena. Berger and Lambert's chapter represents a brave attempt to take S-R theory beyond the dyad.[10] Despite their attempts, however, they ignore much that is politically interesting in S-R theory.

We can remedy this deficiency, however. Let us briefly consider the following findings from the Berger-Lambert chapter. First, rewards are usually more effective than punishments as a means of inducing learning.[11] (An explanation for this phenomenon may be that punishment creates anxiety, and anxiety, in its turn, disorients behavior.)[12] Second, intermittent rewards are usually more effective than unfailing rewards.[13] Third, behavior that has been learned through *mixtures* of reward and frustrations (in the form of delays) resists eradication more stubbornly than behavior learned in other ways.[14] To summarize colloquially: the carrot is more powerful than the stick; a few carrots are better than a great many; difficulties in obtaining carrots make people value them highly.

---

[1] Robert B. Zajonc, "Cognitive Theories in Social Psychology," I, 320–411. All references to articles in the new *Handbook* will refer to volume and page number, unless special circumstances arise.

[2] Theodore R. Sarbin and Vernon L. Allen, "Role Theory," I, 488–567.

[3] Calvin S. Hall and Gardner Lindzey, "The Relevance of Freudian Psychology and Related Viewpoints for the Social Sciences," I, 245–319.

[4] But, as Kline's recent study suggests, psychoanalytic theory is actually not as resistant to verification as one might assume. See Paul Kline, *Fact and Fantasy in Freudian Theory* (London: Methuen, 1972).

[5] One of the most influential forms is that outlined by Jane Loevinger, "The Meaning and Measurement of Ego Development," *American Psychologist,* 21 (1966), 195–206.

[6] For influential applications of Maslowian theory to political science see Jeanne M. Knutson, *The Human Basis of the Polity* (Chicago: Aldine-Atherton, 1972); and Stanley A. Renshon, *Psychological Needs and Political Behavior: A Theory of Personality and Political Efficacy* (New York: The Free Press, 1974).

[7] Erik Erikson's seminal work is, of course, *Childhood and Society* (New York: Norton, 1958).

[8] For a good treatment of this aspect of Freud's work, see Paul Roazen, *Freud: Political and Social Thought* (New York: Knopf, 1968).

[9] For a stimulating recent application, see Michael Paul Rogin, *Fathers and Children: Andrew Jackson and the Subjugation of the American Indian* (New York: Knopf, 1975); a useful overview is Robert Jay Lifton, ed. *Explorations in Psychohistory: The Wellfleet Papers* (New York: Simon and Schuster, 1975).

[10] Seymour M. Berger and William W. Lambert, "Stimulus-Response Theory in Contemporary Social Psychology," I, 81–178.

These findings have particular relevance to the study of political leadership. For example, leaders may be unable to secure compliance and support indefinitely because they cannot apply these principles consistently. Especially is it difficult for leaders to hit upon the optimal blend of rewards and punishments in securing compliance. Temperament alone may bias a leader in favor of either indulgences or deprivations. Berger and Lambert are unable to extend their findings to these sorts of questions, partly because they are still victimized by the dyadic strictures of S-R theory. Yet, inadvertently, they provide us with the rudiments of a useful political model.

That Berger and Lambert may have unintentionally sold S-R theory short becomes more evident when we realize that, in recent years, S-R theory – like Freudianism – has staged something of a comeback. Exchange theory in political economy and sociology,[15] Skinner's applications of learning theory to politics,[16] behavior modification within psychology itself[17] – all testify to the vigor of the S-R tradition. Berger and Lambert's treatment gives little presentiment of these recent developments. Their discussion remains locked for the most part within the confines of the dyad, confines which S-R theory in its current resurgence has finally begun transcending.

These considerations should clarify the basis of cognitive theory's current preeminence over S-R theory and Freudianism in social psychology. Cognitive theory has succeeded because it has managed to combine the creativity of the Freudians with the experimental rigor of the S-R theorists, thus producing nonobvious, testable propositions about recognizable social actors. No other theoretical tradition in social psychology can claim as much. Yet who would deny the suggestiveness of such propositions as "... the more the individual undertakes unrewarding tasks the greater will be his reluctance to give such tasks up"?[18] And, further, who can dispute the richness of the experimental tradition that runs from Acsh's elegant manipulations of social judgments to Schachter's more formidable elaborations of attribution theory.

Zajonc's chapter on cognitive theories in social psychology does full justice to this now-dominant tradition. His review bristles with findings and propositions that many political scientists could find rewarding. Moreover, he is careful to indicate – perhaps too briefly – that cognitive theories include more than just cognitive dynamics.[19] Thus he at least leaves the door open to Piaget, as well as to the usual spokesmen of cognitive theory. I might have chosen many different parts of Zajonc's chapter to illustrate its utility for the political scientist, but let me concentrate on three findings.

First, Zajonc demonstrates that the cognition of influence structures is both ubiquitous and biased toward closure. We attempt to locate transitivity and asymmetry in influence structures, and when we confront ambiguous influence formations – such as families – we *impose* leader-follower definitions on them.[20] Surely it is not accidental that we always arrange silhouettes of family members with fathers at the extreme left of the group, where they dominate the family scene.[21] In short, we import cognitive biases involving power into many different situations. Political scientists interested in the vicissitudes of power should at least be aware of these cognitive propensities.

---

[11] *Ibid.*, pp. 88–89.

[12] *Ibid.*, p. 89.

[13] *Ibid.*, p. 91.

[14] *Ibid.*, p. 95.

[15] For an attempt at synthesis, see Sidney Waldman, *Foundations of Political Action: An Exchange Theory of Politics*(Boston: Little, Brown, 1972).

[16] B. F. Skinner, *Beyond Freedom and Dignity* (New York: Knopf, 1971). See the critique of Skinner by Meredith Watts, "B. F. Skinner and the Technological Control of Social Behavior," *American Political Science Review*, 69 (March, 1975), 214–227; Skinner responds in the same issue, pp. 228–229.

[17] The major impetus here was provided by Albert Bandura, *Principles of Behavior Modification* (New York: Holt, Rinehart, and Winston, 1969).

[18] This is the sort of counterintuitive proposition that runs through much work in cognitive dissonance. For an application of the proposition, see Leon Festinger, Henry Riecken, and Stanley Schachter, *When Prophecy Fails* (Minneapolis: University of Minnesota Press, 1956).

[19] Zajonc, I, 332–335.

[20] *Ibid.*, p. 336.

[21] *Ibid.*, p. 337.

But there is a second cognitive bias that should interest the political scientist. According to Zajonc, studies based on Heider's balance theory concur in concluding that people find it easier to balance *positive* than *negative* relationships.[22] Relationships of hostility, repulsion, and suspicion apparently create cognitive disruptions which prevent the development of balanced structures. Could it be that the cognitive rules governing political hostility are different from those governing agreement? If the "rational man" maps political cooperation and political hostility differently, can he be said to be comparably "rational" in both cases?

Finally, as Zajonc notes, the theory of cognitive dissonance yields interesting findings about disconfirmed expectancies. People who feel that they have been rewarded beyond their just deserts apparently strive extra-hard in order to match their achievements to their rewards.[23] Findings of this sort have relevance not only to theories of political ambition and career pursuit, but also to theories of justice. Perhaps reciprocity and fairness are intrinsically attractive to people, leading them toward "just" distributions of rewards. If so, we need not discount conscience as a guide to political action.

Yet cognitive theory is limited in several important ways. For one thing, the cognitivist's experiments are sometimes excessively sophisticated and artificial. This criticism extends particularly to experiments in the dissonance tradition.[24] Second, the dynamics of cognition remain every bit as much a matter of inference and assumption as is the psychoanalytic dance between id, ego, and superego. Dissonance, for example, is inferred from its presumed effects, not from direct measurement. Third, cognitive theory, like S-R theory and most varieties of Freudianism, essentially hypothesizes a tension-reduction system of behavior. There is little room in such theories for genuine innovativeness or cognitive restructuring of the political sort described by, among others, Peter Sperlich.[25] Fourth, and finally, cognitive theory is usually a bit uncertain about the psychological generation of social norms.

Indeed, a realistic account of the way social roles emerge is a problem for most of the theories reviewed in Volume I of the *Handbook*. Role theory, as described by Sarbin and Allen,[26] perhaps comes closest to capturing the intricacies of person-environment interaction in the real world. But, role theory is still preoccupied with descriptive typologies and role dimensions.[27] True, role norms have been generated under some experimental conditions, but such experiments usually exaggerate the flexibility of the environment, a fact which Patrick Heine implies when he notes, ". . . to conform or not to conform is scarcely ever a question in real life, but a condition of it. Thus, whether we do or do not ally ourselves with the majority in any given situation illuminates neither character nor conformity generally."[28] Perhaps a judicious blending of symbolic interactionism, Piagetian psychology, and attribution theory could provide a fresh viewpoint on the problem of how role norms are generated in the real world. But this task represents a continuing problem for social psychology.

Methods in Social Psychology

Volume II of the *Handbook* cannot help but impress the reader with the variety of social-psychological methodologies. Interestingly, it is the chapters on techniques little used by political scientists that are most engaging. The articles on techniques that already form part of the political scientist's armory seem dull and stolid by comparison. Indeed, in at least a couple of cases, these latter chapters are sadly incomplete and outdated.

Consider, for example, Mosteller and Tukey's piece on data analysis.[29] The authors concentrate on some extraordinarily arcane and complex procedures for establishing the reliability of sample means. The techniques described (including binomial probability paper, Bayes Theorem, the "jack knife") seem far too complex for even statistically advanced political scientists;[30] worse, Mosteller and Tukey never really convince us that it is necessary to adopt the techniques they describe. At the same time, techniques that are at the heart of contemporary political science – linear regression, causal modeling, factor analysis, smallest space analysis – simply do not appear. A clue to the emptiness of the chapter is the fact that the authors' examples are as arcane as their techniques. Mosteller and Tukey's techniques may be useful in helping researchers decide whether Hamilton or Madison wrote disputed parts of the *Federalist* papers, but neither political scientists nor social psychologists usually pursue this sort of problem in their research. Political scientists interested in a relevant and comprehensive discussion of contemporary data manipulation had best look elsewhere.

---

[22] *Ibid.*, p. 351.

[23] *Ibid.*, p. 378.

[24] The original criticism was made by N. P. Chapanis and A. Chapanis, "Cognitive Dissonance: Five Years Later," *Psychological Bulletin*, 61 (1964), 1–22.

[25] Peter Sperlich, *Conflict and Harmony in Human Affairs* (Chicago: Rand-McNally, 1971).

[26] Sarbin and Allen, "Role Theory," I, 488–568.

[27] This is indicated by the large proportion of the Sarbin and Allen chapter given over to these topics and the small proportion devoted to actual *propositions* about role behavior in the real world.

[28] Patricke J. Heine, *Personality in Social Theory* (Chicago: Aldine, 1971), 33.

[29] Frederick Mosteller and John W. Tukey, "Data Analysis, Including Statistics," II, 80–203.

[30] I have found these techniques used specifically by just one political scientist. See Edward R. Tufte, "Determinants of the Outcomes of Midterm Congressional Elections," *American Political Science Review*, 69 (September, 1975), 812–826, 819 n.

Other discussions of methodologies will be more useful for political scientists. For example, William Scott's piece intelligently advocates a multimethod approach to attitude measurement as a means of coping with problems of reliability and validity.[31] Scott also does a good job of exposing the assumptions underlying cumulative versus additive scaling techniques. But even so there are problems. Scott has little to say about the relationship of attitudes to behavior, even though contemporary research demonstrates convincingly that many attitudinal constructs are heavily domain-specific behaviorally.[32] In addition, Scott limits his discussion of response biases to extremity sets, acquiescence sets, and "faking good" sets. But subject sensitivity to attitude measures these days includes a good deal more than these biases (e.g., racial, ethnic, and class cues). Finally, Scott gives us little reason to foresee the emergence of the "nonattitude" debate that is currently dividing political scientists.[33] In sum, Scott's discussion reflects a perhaps overly optimistic view of attitude measures in social science.[34]

A similar judgment can be entered about Cannell and Kahn's treatment of interviewing.[35] Again the chapter as a whole is quite straightforward and clear, with many helpful hints thrown in along the way. Especially useful is Cannel and Kahn's review of studies which focus on memory failures in the interview situation.[36] Not surprisingly, the more threatening a bit of information is to the respondent, the less accurate is his report. Political scientists often are forced to guess about the accuracy of their respondents' memories; they need not do so quite so blindly if they read Cannell and Kahn.

But again Panglossianism looms. Cannell and Kahn blithely counsel the would-be interviewer to teach the respondent his "role" and to "motivate" him to take part in the interview.[37] One can only wonder if respondents who require such urgings contribute much to social research in the long run. Surely tactics of the sort advocated by Cannell and Kahn must inflate the large amounts of error commonly found in interview studies. Social scientists who condemn the "artificiality" of the psychological experiment might ask themselves about the "realism" of the interview techniques advocated by Cannell and Kahn.

Among the outright omissions in these chapters is any discussion of how one identifies populations for surveys, for interviewing, or for attitude measurement. Nor is the logic of sampling described. It is almost as if these subjects – each of which occupies the margins of data analysis, interviewing, and attitude measurement – fell between the cracks.

Finally, significant lacunae appear in another area of social-psychological methodology familiar to political scientists – cross-cultural research. Because John Whiting comes at this topic very much from the perspective of a social anthropologist, most of his examples will have little relevance for the comparativist in political science.[38] Still, the chapter is redeemed by Whitings's brilliant discussion of one of the chief theoretical difficulties underlying all comparative research – namely, the problem of inferring whether common processes in two cultural units are the result of diffusion from one culture to the other or the result of the sharing of a common historical cultural source. This is Galton's problem, first outlined in

---

[31] William A. Scott, "Attitude Measurement," II, 204–356.

[32] For a review, see Steven Jay Gross and C. Michael Niman, "Attitude-Behavior Consistency: A Review," *Public Opinion Quarterly*, 39, Fall, 1975, 358–369.

[33] See John C. Pierce and Douglas Rose, "Nonattitudes and American Public Opinion," *American Political Science Review*, 68 (June, 1974), 626–649, with subsequent comments and rejoinder by Philip Converse and Pierce and Rose, *Ibid.*, 650–666.

[34] For a hard-hitting, if somewhat erratic, critique of social psychology in these terms, see Serge Moscovici, "Society and Theory in Social Psychology," in *The Context of Social Psychology: A Critical Assessment*, ed. Joachim Israel and Henri Tajfel, European Monographs in Social Psychology, 2 (London and New York: Academic Press, 1972).

[35] Charles F. Cannell and Robert L. Kahn, "Interviewing," II, 526–595.

[36] *Ibid.*, p. 546–548.

[37] *Ibid.*, p. 576, ff.

[38] John W. M. Whiting, "Methods and Problems in Cross-Cultural Research," II, 693–728.

1889.[39] The relevance of this issue in current studies of political diffusion is obvious. For example, does the fact that adjoining American states share legislative innovations indicate structural-functional similarity, diffusion, or the effect of common historical origins? And, if the last of these three alternatives is the case, what is the source culture? Need culture be "real" in the anthropological sense or might it be a common storehouse of memories, perhaps even a common imagined past (such as the myth of a master race)? To my mind no political scientist interested in diffusion has firmly grasped the ramifications of Galton's dilemma.[40] Whiting's chapter would be a good place to begin.

The real strength of Volume II, however, lies in its discussion of methodologies which are comparatively unfamiliar to political scientists. Three methods in particular bear comment: experimentation, observational analysis, and measures of social choice and attraction. As we shall see, together these three chapters shed interesting new light on the current state of political science.

The most impressive single methodological contribution to the *Handbook* is Aronson and Carlsmith's piece on experimentation.[41] It is hardly any secret that political scientists have generally shunned experimental formats, even though experimentation is the bedrock of social psychology, and particularly of cognitive theory. The political scientists' arguments against experimentation are too well known to require rehearsal. It is therefore all the more striking for us to consider Aronson and Carlsmith's precise discussion.

Aronson and Carlsmith's major contribution lies in their discussion of several rarely noted advantages of the experimental method. They do not slight the usual focus upon the experimenter's capacity to control and vary stimuli, the advantages of randomly sorting subjects, and the merits of a control group. But they go further. They argue that the apparently damaging artificiality of the experimental situation can actually be turned to good account. All depends on whether the experimenter *uses* artifice cleverly, thereby denuding the subject of his usual resistances and eliciting spontaneous, *natural,* but unusually intense behavior.[42] Here Aronson and Carlsmith hit on the most obvious, but least remarked advantage of experimentation: a good experiment engrosses and exposes as real life rarely can. Therefore, a good experimenter can lay bare what may otherwise remain hidden.

It is all very well for the political scientist to dismiss the possibility that the political world can be reduced to credible experimental terms. But this anti-experimental bias ignores three salient factors: First, experimentation is the only method currently available that permits straightforward *causal* inference in the social sciences. Therefore, unless political science adds experimental methods to its standard repertoire, it will lack the scientific power to which it aspires. Second, experimentation permits careful examination of the conditions, boundaries, and limits of social relationships. This feature gives it a degree of precision unmatched by other methods. Finally, experimentation permits the observation and calibration of actual behavior. "So what," we may ask, but only until we realize that most "behavioral" research in political science is not composed of behavioral observations at all. The bulk of such research either relies upon verbal reports (in sample surveys, for sample) or upon indicators (roll-call voting, for example).

Yet experimentation rarely permits the observation of behavior in the real world. Careful observation outside an experimental format may appear hopelessly beyond the social scientist's grasp; such pessimism, however, is not justified. Weick's chapter on "Systematic Observational Methods" describes a number of standardized coding systems for the description of social interaction.[43] The multiplicity of such systems will perhaps come as a surprise to political scientists, most of whom believe that the Bales Interaction Recorder marks the high point in observational techniques. But there now exist standard methods for recording not only individual data, such as body movements, facial expressions, and language quirks, but also much subtle phenomena of group interaction. It is true that many political decisions are inaccessible to the observer, but at least some decision making takes place in public forums. Inferences about power and influence under these conditions underlie many theories in political science. Most measures of power which have been applied to these situations – indices developed by game theorists or legislative analysts, for example – rest upon untested assumptions about how participants in decisions interpret their own behavior as well as the behavior of others. Observational techniques of the sort described by Weick can be used to flesh out these assumptions. Close observation of how people react to each other when power and influence are being exercised could add a subjective side to our more formal measures of power.

---

[39] *Ibid.*, p. 703.

[40] But see the at least implicit realization of the problem in Virginia Gray, "Innovation in the States: A Diffusion Study," *American Political Science Review,* 67 (December, 1973), 1174–1185, at 1180.

[41] Elliot Aronson and J. Merrill Carlsmith, "Experimentation in Social Psychology," II, 1–79.

[42] *Ibid.*, pp. 43, 56.

[43] Karl E. Weick, "Systematic Observational Methods," II, 357–451.

[44] *Ibid.*, pp. 373–376.

But shouldn't we worry about the validity of such observational measures? Aren't they unbearably intrusive? Weick suggests ways by which this problem can be managed.[44] More important, he makes us realize that observations of behaviors which actors themselves consider consequential promise to be at least as valid as interpretations of behaviors, such as responses to survey instruments, which have little real payoff for actors.

Lindzey and Byrne's comprehensive discussion of sociometric measures provides a filip to these considerations.[45] The advantage of sociometric techniques is that they permit the mapping of a set of social relationships as seen from the perspective of participants themselves, and not from the perspective of an outsider. Surely it would be useful for the political scientist to have such a map at his or her disposal in attempting to infer the meaning and consequences of a political act in an organized setting. Unfortunately, for most political scientists sociometry is *terra incognita*. The Lindzey-Byrne piece is a useful corrective.

The common – and refreshing – element of these three chapters is that they converge on the microanalytic side of social behavior. For ours is a science that has been preoccupied with the macro side of behavior – not its microcosmic variations and meanings. We skim the surface of social behavior; we rarely look beneath. Of course, it *is* hard to produce a theoretically satisfying match between the micro and the macro. But that isn't really our problem. The fact is that we have yet to describe the micro sufficiently well even to grasp the contours of the match problem. Our immediate task might be to remedy this situation with the help of the social psychologist.

The Substance of Social Psychology

Volumes III and IV of the *Handbook* treat the major substantive fields of social psychology, and will be of greater interest to political scientists than will Volume V (describing such applied areas of social psychology as mental health, religion, etc.). Focusing on Volumes III and IV, we see again the appeal of social psychological ideas that are comparatively unfamiliar to political scientists. This is true not only because familiarity blunts our response, but also because several of the articles on subjects familiar to political scientists are flawed by incompleteness, by normative bias, or by a lack of attention to important structural and institutional influences on behavior.

Consider, for example, the articles on socialization (by Zigler and Child),[46] on national character (by Inkeles and Levinson),[47] and on leadership (by Cecil Gibb).[48] Each of these three literatures has found its way into political science – socialization by way of research into political learning, national character by way of comparative politics, and leadership by way of research into political recruitment, decision making, and empirical democratic theory. Yet each of these chapters is flawed by at least one of the problems mentioned above.

Zigler and Child's piece suffers mainly from incompleteness and misplaced emphasis. Heavily devoted to a regurgitation of the voluminous literature on child-rearing and character structure, it virtually ignores important topics like extrafamilial socialization and post-childhood learning. The approach is anomalous, not least because the child-rearing literature itself is so inconclusive.[49] The fact is that very few reliable *or* interesting conclusions can be drawn about the effects of specific child-rearing techniques. Therefore, why Zigler and Child should have pursued this line so singlemindedly is something of a mystery.

By contrast, Inkeles and Levinson's consideration of national character suffers from a failure to confront structural problems that command the political scientist's concern. The problem revolves around the authors' definition of national character as "relatively *enduring personality characteristics and patterns that are modal among the adult members of the society.*"[50] The authors adopt this deliberately narrow definition in order to avoid imputations about the links (if any) between national character and specific political institutions. Understandably, they wish to make such linkages a matter of empirical investigation rather than definitional fiat. The approach thus avoids the pitfalls of the early national character literature, which often asserted rather glibly that a particular sort of "modal personality" was "functional" for a particular set of institutions.[51] So far, so good. The problem is that, once having made this necessary distinction, Inkeles and Levinson never really get around to the relationship between national character and political institutions. Instead, they confine themselves to a discussion of recent methods for uncovering modal personality. And, occasionally, they fall back into the morass from which they have attempted to extricate themselves. Their conclusion that traditional Chinese culture generated "ideal congruence" between personality and institutions[52] does little to explain the triumph of Maoism, which, in many respects, contradicted such "congruent" traditional values as filial devotion, ancestor worship, and relativistic morality. Either the congruence of which Inkeles and Levinson speak was imaginary or else modal personality is more labile than the authors would care to admit.

---

[45] Gardner Lindzey and Donn Byrne, "Measurement of Social Choice and Interpersonal Attractiveness," II, 452–525.

[46] Edward Zigler and Irvin L. Child, "Socialization," III, 450–589.

[47] Alex Inkeles and Daniel J. Levinson, "National Character: Modal Personality and Socio-Cultural Systems," IV, 418–506.

[48] Cecil A. Gibb, "Leadership," IV, 205–282.

[49] Zigler and Child virtually admit this. See pp. 508, 549, 555.

[50] Inkeles and Levinson, IV, 428.

Finally, Gibb's discussion of leadership is flawed by normative bias. Gibb argues that "the term leadership applies only when this [influence] is voluntarily accepted or when it is in a 'shared direction.' The relation between master and slave, teacher and pupil, and frequently that between officer and men is characterized by a type of unidirectional influence which few people would want to call leadership."[53] While there is obviously warrant for distinguishing empirically between positional and unidirectional components of leadership on the one hand and reciprocal and affectual components on the other, we would not want to rule the former two elements out by definition. Worse, Gibb's view encourages us to think that leadership is always consensual and *legitimate*. But this is not so. Often leaders find themselves forced to combat dissension harshly. Moreover, Gibb's characterization deemphasizes the dynamism of the leadership process. After all, if leadership is always voluntarily accepted, it is hard to know why leaders ever face real challenges at all, much less actually fall from power.

Ultimately, however, Volumes III and IV of the *Handbook* can best be used not by focusing on single chapters, but by extracting bits and pieces from several articles in order to shed new light on processes of interest to political scientists. Let me show how this strategy can apply to the subjects of political reasoning, political compliance, and political decision making.

The Study of Political Reasoning. For a long time the study of political reasoning was dominated by descriptions of the limited amounts of political information, the cognitive and affective inflexibility, and the lack of settled political ideologies which allegedly characterized the mass public. This research tack was perhaps epitomized by the syndrome of working class authoritarianism, explicated in the 1950s. During this period political man – as cognizer – cut a sorry figure, indeed.[54]

Unfortunately, scarcely anyone noticed that these conclusions rested upon survey techniques which rarely probed the reasoning process deeply and upon theory which rarely included social-psychological variables. Of course, recent research in mass political behavior has provided us with somewhat more optimistic assessments of man's political reasoning abilities.[55] But this research too is comparatively uninformed by social-psychological insights. Our debates on political reasoning, therefore, display a certain incompleteness. And the problem is only compounded by discussions of "rationality" – a concept that seems almost impossible to handle from the social-psychological point of view.

Interestingly, research in social psychology as far back as the 1950s provides us with a far more complex view of reasoning than that which has informed the work of most political scientists. For one thing, we seem consistently to have overstated the importance of personality appeals, cultural stereotypes, and source credibility in information processing. The *Handbook* demonstrates that people can do a better job of evaluating political arguments on their merits than many of us have previously assumed. An example comes from the work of

---

[51] For a recent criticism of this view, with specific reference to colonial America, see James A. Henretta, "The Morphology of New England Society in the Colonial Period," in *The Family in History*, ed. Theodore Rabb and Robert Rotberg (Cambridge: MIT Press, 1971), 191–211.

[52] Inkeles and Levinson, IV, 481–482.

[53] Gibb, IV, 212–213.

[54] For a useful recent discussion, see Nevitt Sanford, "Authoritarian Personality in Contemporary Perspective," in *Handbook of Political Psychology*, ed. Jeanne M. Knutson (San Francisco: Jossey-Bass, 1973), 139–170.

[55] Perhaps the best single source is Norman Nie, with Kristi Anderson, "Mass Political Belief Systems Revisited: Political Change and Attitude Structure," *Journal of Politics*, 36, (August, 1974), 540–591.

Rokeach and Triandis, who discovered that when blacks and whites find themselves together, whites are more willing to associate with blacks who share their views on segregation than with other whites whose views on segregation they oppose.[56] Now, of course, blacks and whites in the real world are rarely thrown together in quite the way Rokeach and Triandis arrange; yet, the Rokeach-Triandis research suggests that ideological communities may have the capacity to challenge ascriptive communities. The power of ideas should not be underestimated.

Of course, merely demonstrating the appeal of ideas *per se* tells us nothing about the way ideas form, nor about how ideas are connected to each other. Again, however, social-psychological research questions conventional wisdom among political scientists. For one thing, there is no convincing evidence that selective perception dominates the communication process; on the contrary, as Sears and Freedman pointed out back in 1965, people are usually willing to seek out novel political arguments.[57] Even cognitive dissonance does not severely restrict flexible information seeking, and anxiety – usually seen as an inhibitor of communication – can occasionally open up the information-seeking process.[58] Thus, there appears to be both scope and opportunity for political intelligence to operate.

Part of our misapprehension in these respects seems to spring from an assumption that logical and emotive appeals represent poles on a single continuum. Such an assumption is obviously convenient in allowing us to label people as "rational" or "irrational." Unfortunately, however, the continuum itself appears not to exist. According to McGuire, ". . . as regards psychological judgment, logical and emotional are not at opposite ends of a single continuum but tend rather to be separate . . . dimensions."[59] If this is so, political judgment would appear to be far more complex than most political scientists have so far appreciated.

Nor are people easily manipulated by the *sequence* in which they receive information. Instead, the literature shows that people average out positive and negative elements in arguments, thus resisting the distortion of halo effects. When push comes to shove, people stick stubbornly to the overall weight of the evidence.[60]

Finally, work in the areas of conceptual development and socialization reveals a normal growth process of synthetic capacity operating throughout the life span. Piaget's research in this regard is too well known to bear describing.[61] Less well known, though perhaps equally promising, is the work of Harvey, Hunt, and Schroeder, who have outlined four developmental stages, each of which includes both affective and cognitive elements. Each stage is characterized by a distinctive conceptual and personality style, ranging from intolerance of emotional and cognitive ambiguity in the early stages to a balance between affiliative and independent tendencies in Stage IV. Although people in the early stages seem to be vulnerable to external manipulation, those at levels III and IV probe autonomously for information, while resisting coercive or emotive appeals.[62] Research corroborating the Harvey, Hunt, and Schroeder scheme now exists in several areas.[63] Could it be that the *absence* of well-integrated political views, not their presence, constitutes a sort of developmental aberration?

In sum, social-psychological research reveals political thinking to be significantly influenced by the *content* of ideas, to be comparatively free of crippling perceptual biases, to be a complex mix of cognitive and emotive elements, to be informed by a logical weighting system, and to be driven by a need for synthesis and flexibility. Obviously there is ample room for individual variation within this characterization, but the nature of the general model differs considerably from that which has been commonly employed by political scientists.

The Study of Political Compliance. Whatever its relevance on social contract theory and the notion of consent, the liberal theory of politics also posits a willingness on the part of ordinary citizens to be suspicious of and even occasionally hostile toward their leaders. After all, liberal theory originally served as a rationale for challenges to royal authority. The liberal believes that vigilance is an indispensable means of preventing leaders from taking undue advantage of their followers. Moreover, liberal theory also includes the hope that leaders will be representative of their followers; otherwise it is unlikely that followers will feel themselves equal to the task of judging and controlling their leaders, and leaders may well become contemptuous of their followers. Controlled hostility within a framework of representation is a vital part of liberal democratic thought.

---

[56] William J. McGuire, "The Nature of Attitudes and Attitude Change," III, 136–314, 188.

[57] Leonard Berkowitz, "Social Motivation," III, 50–135, 98.

[58] *Ibid.*, pp. 88–89.

[59] McGuire, III, 202.

[60] *Ibid.*, p. 154; see also the more exhaustive review in Seymour Rosenberg, "Mathematical Models of Social Behavior," I, 179–244, 187–193.

[61] The fullest treatment is John Flavell, *The Developmental Psychology of Jean Piaget* (Princeton: Van Nostrand, 1963).

[62] Zajonc, I, 334–335.

[63] *Ibid.*

How does liberal theory construed in this way fare in the face of social-psychological research? The answer is, "Not too well." If anything definitive can be said, it is that even under very dubious conditions people appear altogether too deferential to authority. What we might call a "presumption of acceptability" displays itself across many different areas of social psychology. A case in point is the fact that people seem willing to endure "unbalanced" cognitive fields in order to remain friendly to each other.[64] Being disagreeable is perhaps more costly psychologically than many people can bear.

Sometimes the presumption of acceptability reaches absurd proportions. The best example, of course, is Milgram's demonstration of the ease with which experiments can elicit counternormative and even proscribed behavior from subjects.[65] But there are other examples. As early as 1944 Frank demonstrated that experimental subjects would willingly consume great amounts of soda crackers under the most transparently ridiculous pretexts. More recently, Orne succeeded in getting his respondents to engage in such noxious activities as filling papers with random digits, grasping a dangerous reptile, plunging their hands into acid, and even throwing acid at an assistant.[66] It would be easier to dismiss these unpleasant findings as the freakish offspring of artificial experimental situations if it were known that the experimenter had been perceived to be wholly benevolent in these situations. But, alas, anyone familiar with social experiments knows that, if anything, the opposite is the case. My guess is that people are more suspicious of experimenters than they are of most "authorities."

Even this evidence of quiescence would not be wholly damaging to liberal theory were it not for the characteristics of leaders themselves. The problem is that leaders are usually more flexible, less neurotic, and more self-confident than their followers.[67] Undoubtedly it is partly for these reasons that leaders *become* leaders in the first place. Hence, leaders begin with a two-fold advantage: they can count on both the docility of their followers, and, their own capacity to take advantage of the situation. It is all very well to argue that self-confidence, flexibility, and healthy psychological functioning conduce to *democratic* leadership, but this is small consolation.[68] There is ample opportunity for the processes of leadership to be perverted.

Nor is the presumption of acceptability confined to the laboratory. Deference to leadership is even embodied in language. As Miller and McNeill indicate in their chapter on psycholinguistics, diminutives and familiar nouns almost always signify the legitimate presence of hierarchical relationships.[69] And, as these authors point out, "students make appointments with professors, professors do not make appointments with students. . . ."[70]

It thus appears that the liberal democratic experiment has much to overcome. Without specific barriers against the bias toward hierarchy and deference, democracies would seem constantly at risk.

How can one square the optimism generated by our discussion of political reasoning with the pessimism of our view of compliance? The answer lies in the fact that forces conducive to compliance are absent from the research on reasoning we have cited. The moral seems to be that, left more or less to themselves, people display an admiral flexibility in political reasoning. When, however, they find themselves stuck in situations where authoritative cues prevail, their discrimination flees. In sum, making ordinary people politically "comfortable" would appear to be a priority item for liberal democratic polities.

The Study of Decision Making. Much political decision making occurs in groups. This is unavoidable in a world of specialized information. Yet, as Irving Janis points out, group decision making has its dangers.[71] Particularly in light of our Vietnam misadventure, it would appear that both policy makers and political scientists need to know more about the forces that can maximize the strengths of group decision making as well as about the pressures that occasionally turn groups into political monstrosities. Contemporary research in social psychology is useful in this regard. Let me sketch some key factors.

---

[64] *Ibid.*, p. 347.

[65] Barry E. Collins and Bertram H. Raven, "Group Structure: Attraction, Coalitions, Communication, and Power," IV, 102–204, 179. See Milgram's own account in Stanley Milgram, *Obedience to Authority* (New York: Harper and Row, 1974).

[66] Collins and Raven, IV, 179.

[67] Gibb, IV, 218–228.

[68] A related argument may be found in Paul M. Sniderman, *Personality and Democratic Politics* (Berkeley: University of California Press, 1975), 313–315.

[69] George A. Miller and David McNeill, "Psycholinguistics," III, 666–794, 755–756.

[70] *Ibid.*, p. 757-758.

[71] Irving I. Janis, *Victims of Groupthink* (Boston: Houghton-Mifflin, 1972), chap. 1.

Beginning with George Homans, students of social interaction have revealed over and over again that group members strive to attain a balance between their feelings and the demands of the situation. As a result, people *do* tend to like those with whom they must interact, and to interact more with those they like.[72] Indeed, were this process wholly unchecked one could visualize a gradual polarization of groups across society and a hardening of group boundaries.

The picture would not be a pretty one; after all, groups are hard on deviants. Group members seek reassurance from others and demand conformity to decisions once made. Many therefore punish deviants, even where decision rules do not require unanimity and where the deviant in fact possesses the correct answer to a problem.[73] Nor are group characteristics easily changed; group culture can persist over several "generations" of members.[74]

The forces which push groups toward consensus and conformity would be less worrisome if it were not for the "risky shift" phenomenon – the frequent tendency of groups to adopt more dangerous and chancy political alternatives than group members individually prefer, when polled outside the group setting.[75] There are no persuasive explanations for this phenomenon. Indeed, it may even have physiological underpinnings. As Shapiro and Crider point out, people in competitive relationships within groups display levels of physiological arousal that are far in excess of those they exhibit when making similar decisions under noncompetitive, extragroup conditions.[76] Perhaps when the politician plays out his role within group settings his need to gain and hold power may so arouse him physically that he becomes willing to take chances he would otherwise find unacceptable.

But groups *can* perform more effectively than individuals, especially when group members are complementarily deficient (i.e., when they suffer from different incapacities.)[77] We also know, however, that group decisions usually take longer than individual decisions.[78] This and much other readily available information could be of use in formulating new models of group decision making in politics.

Studies of information diffusion and communication nets also provide useful perspectives on group decision making. As Collins and Raven show, the main contrasts are between wheel shaped, circle shaped, and all-channel communication nets, each of which has fairly regular patterns of speed and accuracy in decision making.[79] This literature should at least sensitize the political scientist to the spatial and communication properties of the groups he is investigating. While many political decisions are not made by spatially static groups, some *are;* and *all* groups have some regular communication properties. The challenge thus seems how to utilize the social-psychological literature flexibly.

Social Psychology and Its Policy Implications

Volume V of the *Handbook*, which focuses on applications of social psychology to various policy fields, is somewhat less useful than are Volumes III and IV for two major reasons. The first is the variability of fit between social psychology and the policy field in question; the second is the simple passage of time, which takes a particularly heavy tool on areas of policy application. Illustrations will again be helpful.

Consider, for example, Etzioni's piece on the social psychology of international relations.[80] The article admirably treats threat and escalation in international confrontations. Not surprisingly, Etzioni comes down on the side of those who feel that gradual de-escalation through unilateral initiatives is feasible and desirable. Yet, despite his best efforts, he does not really succeed in making social psychology relevant to his argument. His piece succeeds not *because* of but *regardless* of its social-psychological origins. Here is a case in which the distance between social psychology and the field of application does not seem bridgeable.

---

[72] Berkowitz, III, 94.

[73] Harold H. Kelley and John W. Thibaut, "Group Problem Solving," IV, 1–101, 71–72.

[74] Henri Tajfel, "Social and Cultural Factors in Perception," III, 315–394, 356.

[75] Kelley and Thibaut, IV, 78–84. A recent reconsideration of the risky shift phenomenon, with a strong support of its importance, is given by Robert Steven Baron, Gard Royce, and Penny H. Baron in "Group Discussion and the Stingy Shift," *Journal of Personality and Social Psychology,* 30 (October, 1974), 538–545, esp. 538.

[76] David Shpairo and Andrew Crider, "Psychophysiological Approaches in Social Psychology," III, 1–49, 16.

[77] Kelley and Thibaut, IV, 66.

[78] *Ibid.*, pp. 73–74.

[79] Collins and Raven, IV, 137–155.

[80] Amitai Etzioni, "Social-Psychological Aspects of International Relations," V, 538–601.

Furthermore, a few of the articles are somewhat dated. An article on prejudice and ethnic relations without data on ghetto violence,[81] an article on the mass media without a consideration of the Surgeon-General's Report on Media Violence,[82] an article on education without reference to the Coleman Report[83] – these papers lose much of their contemporary relevance. Sears's piece on political behavior also suffers in this respect.[84] It cannot consider recent data on "issue voting" and the rational calculus of voting. Nor does it hint at the increased volume of socialization research which, according to Sears himself in a recent publication, has "increased at a geometric rate" from the time he wrote his *Handbook* chapter.[85]

Nevertheless, several of the articles bear reading by political scientists interested in the policy areas addressed. Two in particular deserve mention. The first is Freeman and Giovannoni's careful discussion of the medical, psychological, and social meanings of "mental health."[86] The authors discuss all the issues – including the political and legal – that surround this difficult concept. They also carefully review the weaknesses and strengths of the medical model of mental illness. In so doing they are able to chart the legitimate as opposed to the repressive uses of the medical characterization. For example, they report no association between a person's symptomatic state and his occupational performance,[87] a finding which undercuts any who might like to use psychological diagnoses as rationalizations for essentially social and political purposes, such as the perpetuation of their own arbitrary hiring and firing practices.

Finally, Vroom's discussion of industrial relations provides strong support for worker participation in corporate decision making. Vroom elaborately demonstrates that worker motivation to execute decisions picks up with greater involvement in decision making, and that workers who feel responsible perform with greater efficiency.[88] This is the sort of information that is indispensable for those who support power redistribution at the work place – or in the polity, for that matter.

---

[81] John Harding, Harold Proshansky, Bernard Kutner, and Isidor Chein, "Prejudice and Ethnic Relations," V, 1–76.

[82] Walter Weiss, "Effects of the Mass Media on Communication," V, 77–195. For the Surgeon-General's report, see U.S. Surgeon General's Scientific Advisory Committee on Television and Social Behavior, *Television and Growing Up: The Impact of Media Violence* (Washington: Government Printing Office, 1972); recent corroboration of the impact of media violence in a very different setting is contained in Bradley S. Greenberg, "British Children and Televised Violence," *Public Opinion Quarterly*, 38 (Winter, 1974–75), 531–547.

[83] J. W. Getzels, "A Social Psychology of Education," V, 459–537.

[84] David O. Sears, "Political Behavior," V, 315–458.

[85] Sears, "Political Socialization," in *Handbook of Political Science*, ed. Fred I. Greenstein and Nelson W. Polsby, II (Reading, Mass: Addison-Wesley, 1975), 93–153, 94.

[86] Howard E. Freeman and Jeanne M. Giovannoni, "Social Psychology of Mental Health," V, 660–719.

[87] *Ibid.*, p. 665.

[88] Victor H. Vroom, "Industrial Social Psychology," V, 196–268, see pp. 213, 233.

BOOK REVIEWS

Documents on Political Thought in Modern India, Volume I. Edited by A. Appadorai. (New York: Oxford University Press, 1974. Pp. 547. $13.75.)

The political thought of a nation is as much a commentary on the social and political context conditioning that thought as it is an exposition of fundamental assumptions and doctrines *per se.* In this first of two volumes of documents on modern Indian political thought, Professor Appadorai has provided a monumental collection to serve both these ends.

The temporal scope of the volume is a little more than a century from the British assumption of direct rule in 1857 to the death of Nehru in 1964. The 274 essays, articles, tracts, letters, and other selections include many familiar and essential names: Gandhi (150 items); Gokhale; Tilak; Pal; Banerjea; Ranade; Naoroji; Nehru; Bose; and others, but there are also several less familiar writers whose inclusion helps make this a much broader and more comprehensive collection of Indian political thought than any previous attempt.

The topical organization of the volume enhances its usefulness. Instead of focusing upon specific political thinkers or abstract concepts like "liberty" or "equality," Appadorai has classified his selections according to four broad themes. The first two of these ("A Dependent People's Conception of Imperial Rule" and "Self-Governance is Essential for a People") place the political thought in historical context and suggest cross-cultural comparisons with Jefferson, Payne, Marcus Garvey, Frantz Fanon, and others.

Treatment is balanced, with inclusion of materials in favor of British imperial rule as well as those opposing it. A great range of issues is considered – the right of the imperial power to legislate on social matters, the economic impact of British rule, political versus social reforms, and others. Unfortunately, the coverage is apparently limited to the political thought of British India. The nature of the imperial impact upon the indirectly ruled princely states which constituted roughly two-thirds of Indian territory, and the parallel response by rulers and subjects is largely if not entirely neglected.

"Political Methods" is the next major theme, comprising roughly a third of the volume. Constitutional agitation and terrorist methods are discussed only briefly, with much greater emphasis placed on the nonviolent techniques of protest pioneered by M. K. Gandhi. Though Gnadhi's thoughts on nonviolence are compiled elsewhere in less expensive volumes, Appadorai's thematic treatment maximizes the possibility of comparing and contrasting Gandhian thought with its predecessors and competitors.

The final section, "What is a Nation?," includes materials on communalism as well as secular nationalism. Here too, the organization and presentation of the materials points to fundamental political questions and helps to tie modern Indian political thought to that found in other places and periods.

One minor shortcoming of the work is the postponement of biographical notes on the authors until the appearance of the second volume. In general, the format and presentation of the material are of an exceptionally high quality. This volume is recommended to scholars of political thought as well as to South Asian specialists.

WILLIAM L. RICHTER

Kansas State University

Ideology and Participation. By Douglas E. Ashford. (Beverly Hills, Ca.: Sage Publications, 1972. Pp. 300. $11.00, cloth; $7.00, paper.)

The concept of ideology has been one of the major building blocks of political science. Douglas Ashford believes, however, that it is time to reexamine the conceptual underpinnings of the relationship of ideas to political behavior. He argues that most studies of ideology take one of two forms. First, existing empirical studies attempt to assess the influence of ideology on behavior. Often, the assumption is made that people perceive different political systems in similar enough terms to justify comparison across cultures. Such studies, Ashford maintains, do not consider the different circumstances and institutional structures involved. An equally distasteful alternative is the assumption that the importance of ideology has waned (the so-called "end of ideology" perspective) as other, more important cleavages have arisen.

Ashford argues that there are a variety of perspectives on ideology which relate to behavior and which have not been fully explored. Consequently, he attempts to show that ideology does account for important variance at the individual and institutional levels, using a review of many studies and brief analyses of the political climates in a wide variety of countries in different geographical areas as a data base. His focus is so broad, however, that at times it appears to be more of a review of the major psychological approaches to the study of political behavior than a study of ideology *per se*.

The problem becomes clear in the introductory chapter when he searches for a definition of ideology. It becomes a catchall for "ideas" or even "attitudes and opinions" which are distinguished by a programmatic aspect, a worldview framework, and a simplification of reality to enable the individual to cope with the environment. In addition, ideologies have both cognitive and affective components. Within this broad definitional umbrella, he then proceeds to review a wide variety of studies and insights into the workings of institutional norms and the mind sets of individuals.

In subsequent chapters, Ashford focuses on two principal elements: the range of permissible variance in (1) individual beliefs and (2) institutional norms when investigating the problem on a country-by-country basis (evaluated in a dichotomous fashion as "high" or "low"). This enables him to differentiate between such types as "pragmative modernizers" (who rely on individual initiative to change society) and "intensive modernizers" (who use ideology as the basis of social change).

One of the crucial issues faced is why ideologies arouse such intense reaction among followers. He ranges widely in his search for answers: psychoanalytic constructs; authoritarianism and ethnocentrism; and the institutionalization of dependency relationships as manifested by colonialism. He concludes that emotional insecurity and the inability of people to fulfill their psychological needs make them natural targets for mobilization through emotional appeals. Such hypotheses, however, are still unproven, and further research is required.

A similar analysis is provided for the basis of individual cognition. Among the perspectives reviewed are: need theory, learning theory, the achievment motive, the self-image perspective, and the belief systems approach of Rokeach and Eysenck. He introduces a third dimension of ideology, the evaluative aspect, but this never receives the attention given to affect and cognition.

In assessing the role of institutional norms, Ashford asserts that there is great variation between ideological doctrine and "guidelines for hierarchical relationships" (p. 169) in most societies. Structural differentiation is introduced along with the problem of newly independent countries whose ideologies promise more than their institutions can deliver. The general psychological principles reviewed seem secondary, however, to the extended scope of the examples cited. "So many factors bear on the formation of a political system that it seems unlikely that any universal explanation is likely to be found" (p. 199).

Finally, the cultural underpinnings of an ideology are explored. In this context, Ashford relies most heavily on the work of cultural anthropologists. Considerable attention is given to smaller configurations or "subcultures," primarily the peasant subculture. The problem of coordinating a national-level ideology with rural beliefs is a problem facing most newly independent nations. The difficulty of making a "working ideology" viable, i.e., "providing content to national values at both the individual and collective levels" (p. 283), is a major problem yet to be faced by political scientists.

In summary, Ashford provides a valuable synthesis of current work on psychological approaches to politics in a variety of cultural settings. He also uses psychological variables to interpret political occurrences, past and present. His lack of precision in defining ideology, however, makes the study more an examination of attitudes and opinions than of ideology. He might have spent more time on the following questions which are crucial to his stated concerns: (1) given the pivotal role of affect in ideological attachment, why not investigate the symbols literature for possible answers (there were only a few fleeting references to it)?; and (2) given his concern with bridging the individual and collective levels, what are the methodological problems involved when dealing with variables that link perception and behavior at both levels?

ROGER W. COBB

Brown University

Knowledge and Belief in Politics: The Problem of Ideology. Edited by Robert Benewick, R. N. Berki, and Bhikhu Parekh. (New York: St. Martin's Press, 1973. Pp. 327. $14.95.)

Since the late eighteenth century, social analysts have focused on two facets of ideology: the philosophical and action components. In the 1790s Destutt de Tracy formulated the term "ideology" to denote a science of ideas derived not from the discredited principles of faith and authority associated with the Catholic Church and the monarchical state but rather from more objective impressions gained from the physical senses. For de Tracy, ideology was essentially a scientific means for perceiving the world and also a guide to political action. During the next century, however, Karl Marx took a somewhat different position. He contrasted scientific socialism with unscientific ideologies based on false consciousness. In his view, ideology constituted both a rationalization for actions taken on nonideological grounds – the search for power, wealth, and status – and a highly subjective blinder which distorts an understanding of the social situation.

The fourteen authors whose essays appear in this volume examine the classic issues of the meaning and relevance of ideology. Originally presented at the University of Hull by professors teaching in Great Britain, these essays fall into two general categories. Six authors deal with the philosophical or more specifically the epistemological dimensions of ideology: How does the analyst come to know and understand the political world? How can we reason about the universe? Can our knowledge be objective and universally valid? In answering these questions, W. G. Runciman, Adrian Cunningham, and Bhikhu Parekh use linguistic analysis to interpret ideology. Since ideological perceptions derive from language, which constitutes a limiting cultural mechanism for making universally valid propositions about the world, ideas must necessarily reflect the social context in which the analyst finds himself. Probably Bhikhu Parekh, one of the editors, best states the theme of this set of papers when he describes the main "problem of ideology" (p. 61). Whereas the Greeks viewed reason in terms of pure ends, most contemporary philosophers believe that rationality dictates the means by which we attain our desires. If reason is thus located in the passions, desires, and interests – that is, in a specific historical context – then how can the philosopher attain an objective, accurate, universally valid interpretation of events?

The second set of essays explores the relationships between ideology and political practice. Does ideology function as a guide to political actions; that is, do general ideas shape specific decisions? Or does ideology serve as a rationalization for actions made on essentially nonideological grounds – specifically, to advance the power, wealth, and status of the group which the ideologue represents? Most of the authors take the latter view. For example, in the most comprehensive analysis of the functions performed by ideology and the possible relevance to political actions, Robert Dowse argues that in Nkrumah's Ghana (1961–1966) socialist ideology served mainly as a smokescreen for the pursuit of status, self-interest, and economic security by the Convention People's Party members, the cocoa traders, and the rural villagers. Although it stressed the need for hard work, discipline, and saving, Nkrumah's socialist ideology did not change behavior. By examining the social structure of Ghana during this period, Dowse suggests some convincing reasons behind the failure of the ideologues to secure a behavioral commitment to socialist ends. Taking a somewhat similar view, Harold Wolpe analyzes the relationship in the Republic of South Africa between the racist ideologies of segregation and apartheid, on the one hand, and the behavior of the state and capitalist economic firms, on the other. Although he undervalues the importance of the rural heritage of the Afrikaners, their Calvinist churches, and their schools and presents no information about class stratification within the white population, Wolpe does make a strong case for linking racial policies to class considerations. According to this Marxist perspective, both the pre-1948 segregationist and the post-1948 apartheid ideologies have served similar functions. By rationalizing the need for cheap African labor and subsistence wages, ideology masks the state's advancement of capitalism.

Although most of these fourteen articles present insightful interpretations of ideology in a variety of social contexts, the volume needs greater intellectual coherence. First, the authors share no common agreement on the meaning of ideology. Indeed, only four authors bother to define the concept. (See pp. 178, 229, 245, 257.) Even they conceive of ideology rather simply as a set of ideas, attitudes, and values which are more or less integrated, explicit, and persuasive. What is the relationship between ideology and belief system? How does a specific ideology differ from the more general belief system? The purpose should not be to formulate or discover the single correct definition of ideology but instead to clarify meanings and thereby establish boundaries for discourse. When belief systems as divergent as social science, pluralism, British Labourism, apartheid, and American utopianism in foreign policy are all subsumed under the label "ideology," then the concept lacks a clear, precise interpretation.

Second, the themes stated throughout the volume remain implicit, rather than explicit. Since these essays derived from a conference held at the University of Hull, perhaps the editors could have prepared a position paper clarifying the various meanings attributed to ideology and articulating a series of explicit themes to explore at the conference. True, the English have become famous for the science of "muddling through" without any overall comprehensive plan, but the organization of papers around a few central themes would have provided greater theoretical unity.

Finally and most important, the volume needs a general introductory or concluding essay which integrates the fourteen diverse papers. A two-page preface does not suffice. By clarifying the meanings of ideology, showing the relationships among diverse interpretations, and explicating the central themes relevant to ideology, epistemology, and political decision making, an integrating essay would have added greater intellectual coherence to the volume.

CHARLES F. ANDRAIN

San Diego State University

The Morality of Consent. By Alexander M. Bickel. (New Haven, Conn.: Yale University Press, 1975. Pp. vii, 176. $10.00.)

The Morality of Consent is based on the late Alexander M. Bickel's William C. DeVane lectures delivered at Yale. Like its author's life, the book is both noble and incomplete, yet through it we see more clearly than before who Bickel was and what he believed. Bickel, a law professor at Yale who died in 1974 at the age of 49, combined the roles of historian, lawyer, and social philosopher. The book confirms that the lawyer's role constituted for two reasons the center panel in his professional tryptich.

First, Bickel here describes a model of government based solidly on the legacies of Holmes, Brandeis, and Frankfurter. A respect for process, for the use of reason to discover the limits of reason, and for the precedents of history dominates the model. It eschews moral absolutes. The model is, to use some of Bickel's adjectives, Burkean, Whig, Lincolnian, skeptical, prudential, and complex. He carefully differentiates his perspective from that of Rawls and from modern liberalism, which he equates with the belief that we can and should translate moral imperatives directly into public policy. Bickel's elaboration of this model, derived from our legal traditions, is the book's great strength.

This great strength, the reason why all students of democratic theory and American politics as well as of the legal process should read the book, arises not from originality as much as from the tremendous intellectual importance of the basic assertion: Moral absolutes have no place in public policy, and Dred Scott decisions and Vietnam wars happen when substantive morality intrudes. For public purposes we possess only Burke's "computing principle" by which policy accommodates the relative weights of tentative values and beliefs. Bickel writes: "We had better recognize how much is human activity a random confusion and that there is no final validity to be claimed for our truths" (p. 77). "There are no absolutes that a complex society can live with in its law" (p. 88). We have nothing lexically prior to society itself. Bickel illustrates best in a long discussion, in Chapter 2, of the absence in our law of a firm, philosophically grounded doctrine of citizenship. He warmly applauds this absence and prefers middle-level factual analysis of a sort that, for example, bases the eligibility of resident aliens to receive standard welfare benefits not on the abstract nature of citizenship but simply on the fact that we have subjected resident aliens to the draft.

The second reason for perceiving the centrality of law in Bickel's work is that he adopts his epistemology and methodology from law. Bickel orders his world by reasoning logically and by example in support of a clear conclusion and not through the precise formulation of plausible rival hypotheses. Here as always in his writing, Bickel is an advocate. While his model possesses great strength and nobility, his legal method of argument is the source of the book's significant, though hardly fatal, weaknesses, or at least its incompleteness. The book contains some "I get it all but the therefore" problems which will frustrate readers who, perceiving some differences between legal and scientific methods, prefer the latter.

Let me illustrate. Bickel insists that incremental, pluralistic, shortsighted politics most closely approximate his model. Courts must play a limited role in policy because they are removed by several orders of magnitude from a pragmatic political world that succeeds in making tentative, nonprincipled, "computing" decisions yet retains its legitimacy, i.e., our consent. Judicial legitimacy rests ultimately on its craftsmanship, its resolution of policy questions on the basis of principles that are fundamental if not absolute. Since such principles are rare, the courts' policy work must therefore be rare, also. Bickel may be right, but plausible hypothetical alternatives consistent with his model do exist. Much in our judicial structure and common-law tradition, despite the linguistic trappings of certainty, makes the process tentative and changeable. Perhaps all consent depends primarily on the breadth and depth of public values that support the substantive results of government, so that courts may preserve legitimacy through imperfectly reasoned results, e.g., "one man, one vote," that the culture accepts.

I sense that the difficulty in Bickel's position on the judicial restraint question arises because Bickel never fully elaborated his view of the nature of reason itself. The lesson from law that all our assumptions are uncertain and that we must constantly reexamine our conclusions forms the heart of his model. But in his analyses of the Supreme Court's work, Bickel, who loved the skeptical, complex, exhaustively detailed reasoning of law, did not always practice what he preached. He criticizes the Court's First Amendment distinction between speech and conduct because "very little conduct that involves more than one person is possible without speech" (p. 63). He strongly disputes the correctness not of the Court's abortion policy but of the Court's making an abortion policy where "moral philosophy, logic, reason, or the materials of law can give no answer," no justification for its content.

But if Bickel's model correctly describes the nature of reasoning, then he is wrong here, for reasoning *can* give an answer. Perhaps the speech/conduct distinction is, as a general matter, wise because prosecutions for conduct will depend on more reliable testimony than prosecutions for speech, on observations of physical actions rather than perceptions of what was said. Perhaps the recognition that some arbitrary lines must be drawn in policy is itself a sufficiently reasoned justification for the abortion decision's line-drawing. Perhaps not. The point is that as an advocate, Bickel does not (and probably could not) entirely avoid moralizing about the evils of moralistic analysis.

On balance, however, the virtues here far outweigh the shortcomings. Bickel eliminated some earlier analytical weaknesses, e.g., the simplistic view of the role of electoral majorities in *The Least Dangerous Branch.* The holistically critical view of the Warren Court gives way to criticism of some cases and justices (particularly Justice Black) and praise for others. Students of Bickel himself will gratefully discover a complete bibliography of Bickel's published writings, especially the large quantity of periodical pieces. More important, they will detect here a shift away from Brandeis, the subject of his first book, and toward Holmes, the subject of one of his last books. Most important the argument itself may take on historical value, for Bickel may have articulated a convincing philosophical center around which the reported current mass movement away from New Deal liberalism can and will revolve. At least in the attempt we are cruelly reminded of the unfairness of his loss.

LIEF H. CARTER

University of Georgia

Measures for Psychological Assessment: A Guide to 3,000 Original Sources and their Applications. By Ki-Taek Chun, Sidney Cobb, and John R. P. French, Jr. (Ann Arbor: Survey Research Center, Institute for Social Research, 1975. Pp. xxiv, 664. $15.00.)

In the idealized view, empirical research in the social sciences is a continuous, cumulative, collective, and integrated process. However, the editors of *Measures for Psychological Assessment* present evidence suggesting that in sociology and psychology, at least, the empirical research endeavor is "fragmented, individualistic, discontinuous and wasteful" (p. x). For example, between 1954 and 1965, there were 2,080 different scales and measures used in the four leading sociology journals. Only 2.5 per cent of them were used more than six times, and 72 per cent were used only in one study. In psychology, the figures are equally dismal. A 20 per cent sample of all issues of the fifteen measurement-related psychology journals published between 1960 and 1969 revealed that about 2,500 different scales were in use, but only 3 per cent of them were used more than ten times and 63 per cent were used only once. It is my impression that the percentages for political science would be as bad or worse.

This situation would not be a source of concern if the tiny fraction of scales in repeated use were the highly valid and reliable scales while the little-used scales represented low quality chaff which the winds of empirical and theoretical rigor blew away. But Chun, Cobb, and French found that the most popular scales are no more reliable and valid than a sample of the stillborn scales. They argue that accessibility and ease of use rather than quality were the factors winnowing the popular from the unpopular. To correct this state of affairs, they have published this volume of references to more than 3,000 measures of psychological assessment.

Because of the wide range of measures included, the thoroughness of the editor's research, and the manner in which the scales are indexed, this compilation should prove very helpful to many different types of political scientists, not just to those interested in political psychology. Although attitudes toward specific political actors and specific political issues are excluded, this volume references scales of authoritarianism, self-esteem, leadership ability and effectiveness, racial attitudes, need for power, alienation, and persuasibility, as well as political conservatism, liberalism, cynicism, interest, radicalism, ideology, and orientation.

These scales were indexed as the result of a very thorough search of the twenty-six leading psychology and sociology journals in which scales, tests and measurements are published or used in research. Therefore, after referring to this volume (which covers material from 1960 to 1970), a political scientist in search of a properly validated and reliable scale needs only to consult the political and other social science journals published after 1970, before choosing a scale or deciding to create and validate his or her own scale.

The volume is relatively easy to use. There is an author index, a scale topics index, and two lists of sources. A "primary sources" list gives the location where the scale was first published, a summary of available validation information, and an index to subsequent uses of the scale. The second list includes all of the secondary sources, i.e., articles in which the various scales have been used and sometimes interrelated.

JOHN B. MCCONAHAY

Duke University

Prophets and Patrons: The French University and the Emergence of the Social Sciences. By Terry Nichols Clark. (Cambridge, Mass.: Harvard University Press, 1973. Pp. 282. $12.00.)

There is nothing counterintuitive in the good common sense of Clark's "model": we must examine ideas in their institutional context, and, focusing on France, we must understand how a system of patronage – "clusters" of teachers and scholars under the protection of a Parisian professorial patron – acted critically either to institutionalize or hinder the institutionalization of a school of thought. Reduced to essentials, Clark's theory of "clusters" asserts that in such a highly centralized, hierarchical structure, with limited jobs, it is essential to control or influence the center and to have the exercise of one's special intellectual talents considered a *desideratum* of the curriculum. "Prophets" cause a brief stir, but only "patrons" with institutional skills allow their "clusters" to survive. Genius and proper traditional credentials (à la Durkheim) help, but institutional arteriosclerosis (or, indeed, we are reminded from time to time, chance) can chill a nascent "patron-cluster" bloom before its roots are strong enough. In such a system, social science has taken a long time to come into its own.

Such a construct is more a good description of the French educational system than a serviceable model for isolating, let alone weighing, its elements and mechanisms, but Clark fails to tap even its full potential for illumination. He offers no comparison of the "institutionalization" of social science with that of a new natural science (or any other discipline, for that matter). He does not examine patiently, with attention to details, the functioning of "clusters." He does not study the critical debates and processes by means of which sociology was kept out of the *lycées* for so long. He neither identifies nor analyzes closely the French social-scientific community as it established itself in the universities and research centers after World War II. Instead, with no clear research design, Clark traces (with only cursory attention to contemporaneous intellectual dialogues and with little parallel focus) the fate of diverse groupings of social scientists: three early groups (Comtians, Le Playists, and the anthropologists around Broca); the "social statisticians," who "institutionalized" outside the walls of academe; the "international sociologists" (grouped around René Worms and Jeanne Weill), whose partisans failed to meet "university standards"; and the Durkheimians, who penetrated but ultimately could not secure their survival within the structures of the educational system. The work concludes with a rapid survey of "continuities" and "discontinuities" since 1914, asserting (p. 201) that "no clear new alternatives had succeeded the old patterns," despite the dramatic shift of funding towards the social sciences since 1960.

Apart from its nondisconfirmable model, its looseness of categories, its lack of control groups from outside the social sciences, and its research gaps, what is wrong with this as history? Above all, Clark fails to distinguish between "institutionalization" of ideas in the persons of their original carriers and "institutionalization" of ideas by means of intellectual cooptation. Thus, he primarily traces the careers of figures who established societies, *écoles*, and journals, but who did not succeed in acquiring university chairs, or new names for university chairs, or adequate curricular changes in terms of well-defined new disciplines. In chapter after chapter, however, we meet professors from the Sorbonne, the Collège de France, and the university system in general who are associating with the societies, *écoles,* and journals of the extramural social scientists. What were they doing there? How did this association alter their perceptions, their assignment of tasks to and evaluation of their students and later potential colleagues? If he had pursued such questions, Clark perhaps could have explained to his readers the real significance of the *deus ex machina* introduced briefly to account for the *richesse* of contemporary French social science: the *VIème Section* of the *Ecole Pratique des Hautes Etudes,* "Sciences Economiques et Sociales."

In fact, by the 1920s and 1930s, far from the attention of Clark's investigations, young historians such as Marc Bloch, Lucien Febvre, and their disciple, Francois Braudel already were profoundly impressed by the methods and accomplishments of "social science," viewing these as means by which the historians more systematically and cogently could study the past. These scholars created their own interdisciplinary institutions, transformed the practice of what they termed "the human sciences," and made the French educational system safe for social scientific thought. It was indeed this new breed of historians, grouped around the journal *Annales,* who more than any other figures "institutionalized" social science in France, and they are a group virtually ignored by *Prophets and Patrons.* The *VIème Section,* which links sociologists, economists, demographers, human geographers, and historians, has been directed since its founding in 1947 by two "patrons," *both historians* (Febvre and Braudel), and Clark simply failed to understand the importance of such a configuration. What, in fact, had taken place? The discipline of history, academically well entrenched, became animated and was soon led by scholars convinced that the "analytic" social sciences were vital to "historical synthesis." It was these historians who became the patrons of social scientists just as the wealth was beginning to expand. Intellectually, social science won the minds of French historians, and these historians became the agents of the institutional legitimation of the social scientific disciplines. If Clark had devised a valid plan of research, he would have had, if nothing else, an exciting tale of intellectual and institutional change to tell.

ALAN CHARLES KORS

University of Pennsylvania

Nietzsche's View of Socrates. By Werner J. Dannhauser. (Ithaca, N.Y.: Cornell University Press, 1974. Pp. 283, $15.00.)

The subject matter of this book is of central significance for those who study political philosophy. Socrates is the primal figure for that uniquely western activity. More than any other modern thinker, Nietzsche placed Socrates at the center of Western history as the creator of rationalism, and claimed in his own thought to have overcome that rationalism. Therefore, in Nietzsche's view of Socrates we are near the center of thinking about the nature of political philosophy. For somebody from outside the U.S. (such as myself) it is a happiness to find that a professor of government at Cornell should devote his thought to so central a subject for our Western self-understanding.

I am even happier to say that Professor Dannhauser's book is very well done. He has read Nietzsche's writings carefully and comprehensively; he has also read the accounts of Socrates which are given us in Plato, Xenophon and Aristophanes. Because of the political and economic conflicts between the English-speaking and German peoples in the last generations, English-speakers have not paid sufficient attention to modern German philosophers – the greatest of whom is Nietzsche. Now that Nietzsche is becoming powerful in our society (particularly among the young) his influence often comes to us in the form of a quick and one-sided apperception of his teachings. Indeed, Nietzsche's enchanting (if terrible) rhetoric encourages that one-sidedness. Therefore, Dannhauser's care must be highly praised. He has obviously read Nietzsche over many years and knows the extensive and complex corpus intimately. This is a scholarly book in the best sense of that term. The form of this book is to trace carefully Nietzsche's view of Socrates from the early writings, when Nietzsche was taken up with positivism and modern science, to the last writings where his final encounter with Socrates is put before us. Dannhauser wisely leaves to a separate chapter what Nietzsche says by implication about Socrates in "Thus Spoke Zarathustra." He treats with proper caution the endless irony of the ambiguous speeches which largely make up that work.

One great quality of Dannhauser's book is that it is unpretentious. By that I mean that his personality does not get in the way of laying before us clearly and carefully what Nietzsche is saying. The book is concerned with Socrates and Nietzsche, not with himself. This is a pleasure these days, when so many books about politics that pass beyond the simply "objective" seem to be mostly an exposition of the author's personality and problems. If one wants to follow accurately the details of Nietzsche's changing view of Socrates, this book holds the subject together with accurate care. This is what the book is: a lucid exposition of a central (perhaps the central) issue of modern philosophy and therefore of political philosophy.

The present influence of Nietzsche in the U.S. owes much to Professor Walter Kaufmann of Princeton University, because of his extensive translations and his long book about Nietzsche's life and thought. This has been useful in the sense that Kaufmann has helped to free English-speakers from the picture of Nietzsche as a wild poet of nihilism, who politically was the precursor of the horrors of National Socialism. Kaufmann has destroyed amongst us, surely finally, the impression that Nietzsche somehow took part in the tradition of German anti-Semitism. He lays Nietzsche before us as the chief influence upon European existentialism and as a thinker who laid the foundations of what Freud was later to think. In that sense, the influence of Kaufmann's work has been salutory. On the other hand, Kaufmann has presented Nietzsche as if to make the latter's writings open and palatable to the social democratic world of American academia. He has done this by presenting Nietzsche first and foremost as the expositor of what is assumed in modern society, rather than as the thinker who wants to overcome what stands against human nobility in the modern world. Kaufmann treats vaguely what Nietzsche took as his "most abysmal" teaching, "the external recurrence of the identical." In short, Kaufmann does not take Nietzsche altogether seriously.

What makes Dannhauser's book welcome is that he takes Nietzsche's thought at the highest level of seriousness. For example, he confronts the fact that "the eternal recurrence" is claimed by Nietzsche to be at the center of what he teaches, and therefore cannot be put aside as some kind of ecstatic extra. This is to say that although Dannhauser is finally less persuaded by Nietzsche than is Kaufmann, he nevertheless treats Nietzsche as a greater thinker than does Kaufmann. It is indeed because Dannhauser (as he shows himself in his last pages) is a lover of Socrates, that he is able to treat Nietzsche deeply – namely as the most lucid of all teachers of historicism. As Socrates is the supreme Western figure who taught that historicism can be transcendent, Nietzsche's account of Socrates must be understood, if one is to look at the problem of historicism clearly. As historicism appears to be the fundamental presupposition of the dominant North American social science, Dannhauser's book is important in our present intellectual situation. I mean by "historicism" the teaching that all thought is determined by belonging to a concrete dynamic context. If our contemporary academia is to face its main task, which is to try to understand the present state of our intellectual tradition, this must include the understanding of Nietzsche's writing as it is, in all its greatness, as the work of a thinker who not only assailed Western rationalism, but claimed to have overcome it. Because Dannhauser's book deals with these central issues in Nietzsche's view of Socrates, it takes us to a more philosophic stage of Nietzschean scholarship in the U.S. His work is on the level of the best writing about Nietzsche in Germany and France since 1945.

When one admires a book, it is likely to sound ungrateful to mention extra things one wishes the book had. Nevertheless there are three points I could wish had been developed further:

(a) Dannhauser does not seem to discuss sufficiently what Nietzsche says about Socrates and Platonism in "Beyond Good and Evil." Dannhauser's chief acknowledgment is to the late Leo Strauss as a teacher. One of Strauss's last published writings was an article on how to read "Beyond Good and Evil." Perhaps it is Dannhauser's respect for that writing which has made him decide to give "Beyond Good and Evil" a less central place than in my opinion it deserves.

(b) I wish Dannhauser had said more about Heidegger's work, *Nietzsche.* In my opinion this is the greatest book of commentary on a philosopher which has been produced in our age. Dannhauser refers to it several times, but does not deal with Heidegger's interpretation of Nietzsche at length. Perhaps this would have made his book too complex, and taken away from the fine clarity of his writing. It would have become a book not only on Socrates and Nietzsche, but on Heidegger, Socrates, and Nietzsche. Nevertheless one is aware from this book that Dannhauser is capable of writing about Heidegger's interpretation, and one wishes that he had. After all, Heidegger's historicism is the supreme contemporary understanding of what is implied in the acceptance of that teaching.

(c) Most important. In the last pages of his book, where he makes clear that he is a follower of Socrates and not of Nietzsche, Dannhauser writes: Nietzsche's "critique of dogmatisms and systems may hit Hegel, but it misses Socrates and Plato; and many of the attacks on reason, rationality, and rationalism may hit Descartes, but they miss Socrates and Plato" (p. 272). I wish Dannhauser had greatly expanded this passage, and said why this is so. However difficult such questions are, it is to be hoped that Dannhauser will turn to them in subsequent writings. In this book he presents us with plenty of evidence that he is capable of casting light on these extraordinarily complex issues.

GEORGE GRANT

McMaster University, Ontario

Marx and Mill: Two Views of Social Conflict and Social Harmony. By Graeme Duncan. (New York: Cambridge University Press, 1974. Pp. 386. $15.50.)

"Poverty in itself" Hegel tells us, "does not make men into a rabble; a rabble is created only when there is joined to poverty a disposition of mind, an inner indignation against the rich, against society, against the government, etc" (*Philosophy of Right,* Par. 244 Addition). Professor Duncan compares Marx's and Mill's attitudes to this possible emergence of a "rabble." Marx hoped for a "rabble" which would have transformed itself into a class-conscious proletariat, the bearer of history's meaning; Professor Duncan indicates the large measure of faith underlying this hope, and its somewhat shaky foundations. Mill, on the other hand, feared the rise to power of an unreconstructed "rabble"; Professor Duncan indicates the unacknowledged class interests and economic conditioning which set horizons to Mill's timid sincerity.

The long section on Marx, fruit of an impressively thorough reading of the whole corpus, is not only the main but also the best part of the book. Professor Duncan usefully brings out the extent to which certain problems about Marx's thinking and tactics – for example, violence versus peaceful change, revolution versus parliamentary pressure – can be settled only in concrete circumstances and not *a priori* (pp. 96, 180 ff.). He also stresses how revolution is to come about not through some *deus ex machina* but through the imminent, contradictory development of capitalism itself (p. 57). But, he argues forcefully, Marx does not prove that capitalism's contradictions are absolutely incapable of some kind of resolution, nor does he show how they will necessarily lead to the envisaged alternative society. His central problem about Marx is how a proletariat, conditioned as Marx asserts by the "muck of ages" (p. 176) and specifically by the muck of capitalism, can transcend its conditioning, and do so, moreover, in the direction of the communist ideal.

The final chapter on Marx equally raises central and thorny difficulties, this time about the quality of life and consciousness after the revolution. On what he himself calls a "rather pessimistic" theory, the author argues that class conflict is simply a crucial case of group conflict, which in turn is but a case of social conflict, and suggests that "inequality, division and conflict are not traceable to capitalism alone" (p. 202). These arguments have particular force in view of his earlier well-founded assertion that, for Marx, property divisions are neither the only nor the synoptic social ill: division of labor also is "a primary determinant of men's actual natures and capabilities" (p. 79). He draws together arguments, and adds some of his own, about why the abolition of class conflict will not necessarily purify all of man's passions, so that violence and domination may persist, may even indeed flourish. Here as elsewhere my main criticism is of Professor Duncan's silences. If he had narrowed his range and deepened his discussion (although he does clearly say that this is *not* his intention), he would have produced an excellent rather than a good book. For example, while his criticisms of Marx are sound and provocative so far as they go, the reader can hardly fail to be disappointed by a mere few pages in which we are treated to inferences from "the ruthless internal logic of revolutions" (p. 199).

In the shorter section on Mill, Professor Duncan argues against some misconceptions, such as that of Mill as an eclectic: Mill's enterprise of synthesis is shown as more solid and consistent than is often thought. A more important misconception is that of Mill as a liberal individualist *par excellence;* this image, we are told, "ignores some of his basic themes and concerns" (p. 272). While rejecting those views which would assimilate Mill to Marx, Professor Duncan shows that some elements of Mill's critique, as for example his discussion of cooperative enterprise, are as radical, at least in ideal terms, as much of Marx. But Mill had a fatal ambivalence toward the rabble: circumstances, he feared, had excluded them from the intelligence and objectivity which the ascendant middle classes had taken over from the now decadent aristocracy. The irony of liberal heart-searching is that it is an élite pondering the value of élite leadership; the "rabble" could not have a fair trial. For all his sincerity, we are told, Mill could not see that what was "objectivity" to an enlightened bourgeois was itself a particular, conditioned ideology. "There is little perception of the interests which underlay its ideas, or of the illusory elements in them" (p. 223). Mill's politics is characterized as combining short-term defense against the potential destructiveness of mob rule with a long-term hope "that men could be liberated from class by reason" (p. 230). The long term was to be pretty long: however desirable universal suffrage may be, "it cannot be either good or practicable now" (quoted p. 231), a theme which ran throughout Mill's works. Professor Duncan himself comes near to Marx's judgment, in another context, on "those hypocritical friends who declare their agreement with the principle, but who are doubtful of its feasibility because the world is not yet ripe for it" (*Neue Rheinische Zeitung,* 2nd June 1848).

But be that as it may, the concluding chapter on "Marx, Mill and Modern Society" shows that a large part of the predictive honors go to Mill, whose notions on gradual moral and intellectual adaptation explain what Marx missed: the extraordinary persistence of an at times very shaky capitalist edifice. I would, however, have liked these comparisons to be more and deeper, at the expense, say, of the long "Preliminaries" (pp. 1–54). While Marx's historical materialism and Mill's opposed theory, which we might call "historical ideationalism" are neatly characterized and in some points contrasted, the confrontation could usefully have been sharper and more detailed. The intriguing clue of Mill's distinction between "constitutional" and "real" power might well have been set against Marx's notions of how, and why, political regimes lose their foundation. The book's central topic revolves around the Third Thesis on Feuerbach – about the need to educate the educators – yet, while it is mentioned in the section on Marx (p. 175), the discussion of this problem in Mill heroically avoids mentioning it (pp. 250–257). We then slide from the comparison of the two authors to a brief critical account of modern pluralist/consensual theories of social integration. This discussion is long enough to state some important arguments, but it does not significantly further what is a fairly widespread debate in the political science community.

I must repeat that Professor Duncan disclaims the ambition of making original points about either Marx or Mill (although in the case of Marx he is far less than fair to his own contribution). But if he is to stick rigorously to that intention, we are entitled to ask for more in the way of comparison and criticism. There are many strands of discussion – on Marx, on Mill, on revolutions, on pluralism and so forth – which are broached but, because of the wide aims of the book, do not get off the ground. I would have preferred a narrower but tauter book, but I found the book as written interesting throughout, and at times challenging and fresh.

JOHN MAGUIRE

Nuffield College, Oxford

The Modern Law of Treaties. By T. O. Elias. (Dobbs Ferry, N.Y.: Oceana Publications, 1974. Pp. 272. $19.50.)

The author of the volume under review is Chief Justice of Nigeria and a distinguished specialist in international public law. Because of his academic background (former Professor and Dean of Faculty of Law at the University of Lagos) and professional experience (member and consequently President of the United Nations International Law Commission), he was elected Chairman of the Committee of the Whole Conference on the Law of Treaties in Vienna. The reviewed book is a byproduct of the part Mr. Elias played in the making of the Vienna Convention.

The volume is divided into fifteen chapters dealing with various aspects of both the text and procedures of the Vienna Convention and Conference. The first two chapters entitled "Conclusion of Treaties" define some basic terms as well as the scope of the study. Since the Vienna Convention applies only to the treaties between states, Mr. Elias does not cover either agreements between states and international organizations or between other subjects of international law. Chapter III deals with observance and application of treaties, chapter IV with the effect of treaties upon third states. Chapter V is an account of rules and means of interpretation of treaties, Chapter VI discusses treaties' amendments and modifications. Chapters VII–XI cover various problems connected with termination and suspension of the operation of treaties. The four concluding chapters are devoted to the controversial *Jus Cogens* (XII), to procedures for judicial settlement, arbitrarion and conciliation (XIII), to consequences of invalidity of treaties on certain grounds (XIV), and to technicalities such as the function of depositaries, and to registration and publication of treaties (XV). At the end of the book, the full text of the Vienna Convention on the Law of Treaties with the Annex, bibliography, table of cases, table of conventions, and the index are reproduced.

The book is a brilliant *legalistic* treatise of the law of treaties. The author discusses all relevant legal aspects of the recent evolution of contemporary international law in a logically organized manner. His comments on controversial issues (or cases) are well argued. Mr. Elias is impartial in the best tradition of great international lawyers. This is why his study is a valuable contribution to public international law. But the author's strength – i.e., his rigorous legalistic approach to the subject – has caused certain problems. Take, for example, the question of conflicting ideologies for the interpretation of treaties: Mr. Elias rightly suggests that "there is a strong presumption that the terms used in each language version (of a treaty) are deemed to have been intended by the parties to have the same meaning throughout" (p. 86). This logical statement may not necessarily apply to such general terms as sovereignty, aggression, noninterference in internal affairs, etc. even if they are used in one language only. Here one may see the limits of the legal logic for the resolution of conflicts caused by doctrinaire approach to international law. Or to give another example, suppose one uses the formulation, "a treaty is invalid if its conclusion is procured by the threat or use of force in violation of the Charter of the United Nations." An impartial lawyer might suggest that such a formulation would make the Article applicable to member states of the United Nations only, whereas if one used the formulation, "a treaty is invalid if its conclusion is procured by the threat or use of force in violation *of the principles* of the Charter of the United Nations," it would be of general application today (p. 170–171). There might be still another distinction between the two formulations, however. The Charter of the United Nations is much more concrete than mere *principles* of the Charter. The Charter itself represents a definite text, while its *principles* are open to different ideological interpretations. I believe that most textual nuances in international law today have not only legal but also strong ideological implications. So long as this is true, modern international law literature ought to recognize this new aspect and build it into legalistic approaches.

MILADA SELUCKA

Carleton University, Ottawa

Utopias of the Classical World. By John Ferguson. (Ithaca, N.Y.: Cornell University Press, 1975. Pp. 228. $11.50.)

The aim of John Ferguson's *Utopias of the Classical World* is to "tell the tale of ancient Utopianism" without the psychological or sociological apparatus found in the works of such writers as Marcuse and Mannheim. Rather, Ferguson is eager to let "readers draw their own conclusions" (pp. 7–8). This aim may be noble in a field where the factual basis for such analyses is severely limited and at best speculative, but in his desire to allow the tale to speak for itself, Professor Ferguson has failed to give direction to the reader who is eager to know what the author means by "utopia" and "utopianism," what is unique (or not unique) about the utopias of antiquity, or even what a modern reader might learn from the study of these utopias of the past. Nowhere in the first hundred pages of this book are we given a definition of utopia any more specific than that it is an "enthusiasm" (p. 7). From the Preface we are led to believe that this could be an enthusiasm for a horseless carriage going twenty miles per hour just as well as an enthusiasm for social or sexual equality. As a result, throughout the book there is no explicit criterion for determining what will be discussed and what omitted. Thus, we have chapters devoted to Antisthenes and to Diogenes, but not even a passing reference to Aristophanes, who wrote the *Birds* and the *Ecclesiazusae.* For the most part, though, Ferguson errs on the side of inclusion rather than exclusion and offers a potpourri of visions of the desirable – be it a bountiful Nature whose sheep graze with their wool already dyed (Vergil, *Ecologues*), or philosopher kings, or the peace of St. Augustine's city of God.

Ferguson does not limit himself to the literary expressions of utopias. "Political theory which ignores the hard facts of material existence is in the last resort sterile, and wishful thinking no substitute for the working out of sound economic structure" (p. 176). His concern with "material reality" results in extensive treatments of the political history of Greece and Rome. Consequently, we find in less than two hundred pages of text a survey of Greek and Roman political history from the Mycenean Age to the fall of Rome in 410 A.D., alongside a survey of Greek and Roman political philosophy from Homer to St. Augustine, including a chapter on Judaic Messianism. Even the exceedingly small print does not allow the author to do justice to all these topics. Ferguson justifies the inclusion of this mass of material by emphasizing the interaction between vision and reality, though it is clear throughout that he is far more interested in the effect of vision on reality than of reality on vision. Both political actors and political philosophers are evaluated according to their relative success in applying their visions to reality. Zeno's Republic is castigated as an "ideal; he gave no indication of transition from the present to the ideal" (p. 115). A major section of the chapter on Plato is devoted to his relationship with Dion and Dionysius II of Syracuse and to the activities of students at the Academy as advisers to the political leaders of the age. Similarly, Ferguson stresses the visions which may have directed the practical activities of famous political leaders. Thus, he considers whether Alexander was a pragmatist or visionary, though with good judgment he refrains from trying, in the space allotted, to resolve the continuing debate on this point. Thus, Tiberius Gracchus acting under the influence of the Stoic Blossius is distinguished from his brother Gaius who showed "some indifference to the more distant implications of his policies" (p. 154). Thus, Agis IV and Cleomenes of Sparta, influenced by yet another Stoic, are visionaries, perhaps prompted by Stoic universalism, and more important to the history of utopias than Spartacus who, leading the slaves in revolt with no philosophic tutor, is "but a practical man with limited objectives" (p. 157).

This conjoining of the history of political thought with the political history of antiquity does pose for us the problem of how one is to view the utopian thought of such writers as Plato and Aristotle (the latter of whom Ferguson does not consider utopian (p. 80). Must we look at these utopias as only "a vision to guide and reform *contemporary* politics?" (p. 115; italics added). Does Plato become important because of his questionable attempt to institute a philosopher king in Syracuse, because members of the Academy became political advisers, because many centuries later Plotinus may have proposed actually founding a Platonopolis? Are we to take utopias or visions of the desirable as plans to be implemented? Certainly, the dreams of a bountiful Nature with which the book begins cannot fit into this category. Or must we take the literary and philosophic utopias as attempts to tell us about the nature, potential, and limitations of political society, contemporary or otherwise? Is not the importance of Socrates's vision not what it tells us about the political realities of fifth and fourth century Athens, but its posing for us the perennial problems of philosophy and politics, truth and opinion, justice and injustice? To focus on the practical impact of utopias is to give Lycurgus almost as much space as Plato, Blossius as much as Aristotle, and Alexander as much as Cicero.

Ferguson has tried to do too much in this book and has failed to clarify his purpose. As a result, although there are some fascinating tidbits of utopian thought from such obscure authors as Euhemerus and Scytobrachion who are unlikely to be known outside a small group of classical scholars as well as interesting descriptions of the occasional relationship between political philosophers and political actors, the author has not confronted the problem of utopianism directly, of its meaning beyond the immediate societal influences, and of its philosophic significance for the ancients and for us.

ARLENE W. SAXONHOUSE

The University of Michigan

Against Method: Outline of an Anarchistic Theory of Knowledge. By Paul Feyerabend. (Atlantic Highlands, N.J. and London: Humanities Press, 1975. Pp. 339. $16.50.)

Paul Feyerabend is a prominent exponent of one variety of what has come to be called "the new philosophy of science." In this book, the first he has published in English, he brings together in revised and augmented form the substance of some of his challenging articles (one with the same title and subtitle as the present work) which originally appeared in various collections of not readily accessible monographic studies, such as the *Minnesota Studies in the Philosophy of Science* and the *Pittsburgh Series in the Philosophy of Science.* But as he explains, the present essay was intended to be only the first half of a volume, the other part of which was to have been a rejoinder by the late Imre Lakatos to Feyerabend's attack on, among other things, the version of "rationalism" Lakatos advocated. The latter's regrettably premature death made it impossible to realize this plan. The book as it stands is nevertheless welcome, if only because it makes conveniently available many of Feyerabend's characteristic ideas on the philosophy of science, the grounds for his dissent from Lakatos's views on the methodology of "research programs," and his replies to several critics of his previously published writings.

The main thesis of the "anarchism" or "antirationalism" he presents in this book is that with one lone exception, there are no methodological principles to which the procedures of any "progressive" science either do or should *invariably* conform – such as the principle that a scientific theory is refuted, and should therefore be rejected, if it conflicts with established experimental findings. A strict adherence to such rules, he maintains, can inhibit the growth of knowledge; and the only principle which will not do so is the "anarchistic" one that "anything goes." However, Feyerabend is sufficiently a "rationalist" to offer reasons and evidence (drawn from the history of science) in support of his central thesis. Indeed, more than half the book is devoted to this task.

Several other closely connected views are also presented at some length. Feyerabend maintains that the familiar distinction between facts of observation and theoretical beliefs is pointless, since all statements codifying observations involve theoretical assumptions. Accordingly, a scientific theory is "tested" by confronting it not with "bare facts" but rather with statements that are inescapably theoretical and at bottom embody some inclusive if tacit conception of the nature of things. Science is therefore said to "progress" by "proliferating" alternatives to what appear to be well-established theories: this is so because the critical observational facts which really test a theory are typically suggested and identified only by some alternative theory. Moreover, in opposition to widely held views, including Lakatos's methodology of research programs, Feyerabend believes that in general there is no "rational" way of choosing between alternative theories. For according to him, theories may be and frequently are "incommensurable," because they are committed to radically different "ontologies," as Newtonian physics and relativistic theory are alleged to be. The decision between such incommensurable theories is in consequence said to be made largely on the basis of esthetic preference, metaphysical prejudice, religious conviction, or personal wishes. He concludes that science is closer to myth than is commonly recognized, and that the much advertised skeptical rationalism of science in contrast with the dogmatism and unreliability of other modes of thought is a "fairy tale."

There is no room for a detailed critique of Feyerabend's various contentions, and I must limit myself to a few general comments, omitting evaluation of the soundness of his curious historical judgments. In the first place, he is attacking what is pretty much a straw man when he argues against a notion of method according to which there are "firm, unchanging, and absolutely binding principles for conducting the business of science." It would be difficult to find many contemporary scientists or philosophers of science who hold that the principles they may be employing in assessing the validity of cognitive claims in science cannot be modified and improved in the light of continuing inquiry, or who maintain that the adoption of such principles is incompatible with recognizing that their scope may not be unlimited or that discretion must be used in applying them. It would surely be misleading to say that because a judge in a court of law must exercise judgment when deciding a case, he is bound by no legal rules, or that because there are limits to what a given cutting tool (such as a knife or saw) can do a carpenter must foreswear the use of cutting tools. It is equally misleading to hold that because it might be undesirable in certain circumstances to adhere to some methodological precept, the one principle that is being followed is that anything goes. Indeed, in those instances Feyerabend cites in which a contrary of some widely held methodological rule is supposedly followed, it is clearly not the case that anything goes. Consider his claim that a science sometimes advances by proceeding "counterinductively" —, i.e., by proliferating new hypotheses which, like Newtonian theory, were allegedly inconsistent with what were regarded as well-established assumptions, such as the laws of Galileo and Kepler. Now, there is no shred of evidence that Newton (or any other scientist Feyerabend mentions) proposed his theory in order to develop an alternative inconsistent with accepted views. But however this may be, let us grant for the sake of the argument that such evidence is found in this manner. What is to be done with this evidence? Should we proceed counterinductively with respect to it, and in consequence perhaps retain the accepted theory? It would be silly, however, to proceed in this way, nor does Feyerabend recommend it, *if* the rationale of the initial counterinductive maneuver was to *test* that theory. Surely in such a situation it is not the case that anything goes!

On the other hand, in consonance with his dicta on proliferation, Feyerabend maintains that any idea, however ancient and seemingly without foundation (such as Voodooism) is capable of improving our knowledge. But since it is unclear from his discussion what he understands by "knowledge" and its "improvement" or whether he thinks that knowledge is ever achieved, it is difficult to say whether he is faithful to his anarchism in this pronouncement. Moreover, since he offers no explicit general formula stating when two theories are supposedly "incommensurable," and instead tries to explain the notion by examples from studies of perception, artistic styles as well as science, it is no less difficult to decide when two given theories are incommensurable, or to understand why the adoption of one such theory rather than the other contributes to the growth of knowledge. Thus, he asserts that classical and relativistic mechanics are incommensurable, because the former assumes while the latter denies that there are properties (such as shapes and volumes) which "inhere" in physical objects, so that relativity theory "cannot share a single statement with its predecessor." But if this is so, how is it possible for proponents of these theories to communicate with one another concerning the relation of the theories to experiment? Feyerabend's discussion of this point is not very helpful. He maintains that the Michelson-Morley experiment, for example, can be formulated in each of the two theories, but denies that "it is the same experiment which confirms one theory and refutes the other." The two formulations possess only an identity of *reference*, though what is thus referred to cannot be stated in a language common to both theories. Just why this is so remains obscure. In any case, he is thus led to maintain the amazing thesis that since the two theories are incommensurable, a decision between them cannot be made on rational grounds in the light of the available evidence neutral to the theories. This outcome is perhaps to be expected if one holds, as he does, that "knowledge ... is not a series of self-consistent theories ... ; it is not a gradual approach to the truth. It is rather an ever increasing *ocean of mutually incompatible ... alternatives.*" However, this makes sense only on the assumption that we have no knowledge whatsoever, neither of singular nor of general truths – an assumption which not only makes Feyerabend's cognitive claims nonsensical, but is also incompatible with just about every action in which we must engage to survive.

ERNEST NAGEL

Columbia University

The Fascist Persuasion in Radical Politics. By A. James Gregor. (Princeton, N.J.: Princeton University Press, 1974. Pp. 472. $15.00.)

Since the publication of his book *The Ideology of Fascism* in 1969, Professor Gregor has developed an interpretation of Fascism which, if accepted, would radically alter our perception of twentieth-century revolutionary movements. In *The Ideology of Fascism* Gregor argued that Fascism was no less ideologically coherent than Marxism-Leninism and that the policies implemented by Fascist governments reflected this ideological coherence. In *The Fascist Persuasion* he goes one step further by developing themes that were peripheral in the earlier book. His intent and method are clearly described in Chapter One. He wants us "to reconsider the now traditional distinctions between 'left' and 'right-wing' radicalism" according to which Fascism and Marxism-Leninism are at opposite ends of the political spectrum. Gregor proposes to bring out their "suggestive similarities" by examining their ideological roots, techniques for mass mobilization, and by disregarding their "obvious (but to my mind and for our purposes, unilluminating) differences" (p. 22).

There is a definite resemblance between Gregor's notion of the related nature of left- and right-wing movements and the views advanced a generation ago by such analysts as James Burnham, Peter Drucker, Carl Friedrich, and Zbigniew Brzezinski, whose works he cites approvingly. But whereas these earlier writers were primarily concerned with the economic and institutional arrangements of Fascist states, Gregor is only concerned with understanding how revolutionary movements mobilize the masses for political action. His message is that the prescription for successful mobilization was first devised by Mussolini's "paradigmatic fascism." The conclusion is that "perhaps Marx was not, after all, the prophet of our time. Perhaps in a curious but not totally incomprehensible sense, Mussolini was" (p. 188).

From this perspective, Gregor sees Mussolini as the true precursor of Third World leaders such as Mao Tse-tung and Castro and of the would-be revolutionaries of the American New Left and black liberation movements. All of them are said to share Mussolini's belief in the primacy of leadership over economic determinism in setting the stage for revolution. Like the Fascists, they also appeal primarily to nationalist rather than class resentments, cater to popular cravings for status and prestige rather than to economic interests, and promise economic expansion and modernization within the framework of national society rather than through international cooperation.

This kind of book often generates more heat than light. Not only is the tone polemical and even downright acrimonious, but the evidence is presented in such a one-sided way that the reader can well understand the apoplectic reactions Gregor mentions in the preface. When Mazzini is made out to be a precursor of fascism, or the contributions of Antonio Gramsci are not brought into the discussion of how Marxist intellectuals must relate to the masses, or Mao's thoughts on the question of revolutionary leadership discussed without mentioning his concept of the reciprocal influence between the political leadership and the masses, readers have a right to suspect that they are not receiving all the information to which they are entitled.

Not that Gregor's book is hastily put together. On the contrary, it is clearly the result of many years of diligent research and meditation. Furthermore, many of his insights are very valuable. He seems to be right on target when he emphasizes the cognate relationship between Marxism and Revolutionary Syndicalism, or when he draws attention to the productivist and modernizing aspirations of Fascist movements. Some of his specific assertions, however, should not go unchallenged. What evidence does he have for stating that Mussolini filled party vacancies with young people, when it is well known that by the 1940s the party was in the hands of aging cronies (p. 248)? Elsewhere Gregor leaves us with the impression that the main reason Mussolini failed to establish a solid working-class basis for fascism was the hostility of the "orthodox socialist parties who maintained a sectarian resistance to fascism" (p. 316). This explanation does not tell us why Mussolini gave only erratic support to his own labor unions. The antilabor bias that was deeply ingrained in fascism may not be immediately evident from a study of Fascist ideology, but it was certainly an integral part of Fascist policy.

Of course, we should not forget that Gregor wants to tell us mostly what the Fascists said in order to gain power and not what they did once they were in power. Nevertheless, to quote his own words from *The Ideology of Fascism*, "The implied assumption is . . . that *some* identifiable fascist precepts and convictions exercised *some* influence over overt behavior" (p. 14). That is precisely the questionable assumption, and nowhere is that more evident than in the case of Mussolini's behavior. Even if we accept Gregor's concept of ideology as being primarily a set of observations and intuitions about how to manipulate the masses, it is difficult to explain Mussolini's rise to power on the basis of ideological considerations. Gregor shows that Mussolini was more knowledgeable about the political literature of his time than his detractors have given him credit for, but in the balance this may not mean very much. The question is whether this knowledgeability guided his political actions or merely enabled him to rationalize decisions made on other grounds.

The evidence indicates that when it came to making and implementing policy decisions, ideology served essentially as a smoke screen. Ultimately, people accepted or rejected fascism depending on whether they expected to benefit or lose from its victory in tangible and sometimes carefully specified terms. Thus, members of vested interest groups such as big business, landowners, the army, and the bureaucracy accepted fascism because they correctly anticipated gains, while industrial workers, sharecroppers, and most landless laborers rejected it because they anticipated setbacks.

Had Gregor approached fascism from a social as well as an ideological perspective, he might have come up with a more comprehensive and convincing explanation because it is in the realm of social policy that the real differences between Fascist and Marxist-Leninist movements stand out. But, having decided beforehand to exclude any consideration of the differences, he sees – not surprisingly – only their similarities no matter how superficial they might be. The larger perspective would also have enabled him to give a more historically accurate account of the role that organized violence plays in the rise and consolidation of Fascist regimes, a role which he denies because "mass murder, brutality, and violence do not uniquely distinguish any political system" (p. x). On the contrary, the injection of the principles and practice of paramilitary struggle into political contests is a distinguishing feature of fascism and, for better or worse, an original contribution of the Fascist squads.

When all is said and done, it seems best to conclude that fascism was brought to power by a combination of systematic violence directed against the most steadfast opponents, by the politics of compromise designed to entice powerful vested interests, and by an ideological appeal that was most persuasive among members of the lower middle-class. Fascism was brought to power by identifiable social groups moved by specific expectations and not, as Gregor would have us believe, by amorphous "masses composed of individuals who enjoy no strong community or class ties, who are 'detached' and 'anomic' as a consequence of protracted crises" (p. 414). In other words, Gregor's insistence on the overriding importance of ideology distorts the historical reality of his paradigmatic model. He has written a thought-provoking book, but also one which is only minimally related to fascism in any of its historical manifestations.

ROLAND SARTI

University of Massachusetts, Amherst

Political Science: A Bibliographical Guide to the Literature. By Robert B. Harmon. Second Supplement. (Metuchen, N.J.: Scarecrow Press, 1972. Pp. 394. $17.00.)

This *Bibliographical Guide* represents the second supplement, thus up-dating Professor Harmon's two earlier bibliographies on this topic that were published in 1965 and in 1968 respectively. Apparently trying to make his bibliography useful to the specialist in political science as well as to the layman, the editor lists highly specialized literature in the field next to more general works and text books. The entries provide only basic information on primary and secondary literature, with the exception of the section on Reference Sources, which is annotated.

In terms of basic coverage, one-fourth of the volume is dedicated to theory, methodology and reference works in political science, while three-fourths cover the remaining major fields of the discipline. In addition, the volume contains an almost two-hundred-pages long authors' and titles' index, which certainly is an asset for the users.

More than one-third of all entries compiled in this work represent items published abroad. This adds to a diversified character of the volume and helps to awaken the awareness about the contributions of other nations to political science. This broad-minded approach does not apply, however, to Professor Harmon's chapter on American Government and Politics, which does not include foreign publications. Such an exclusive selection of entries for this chapter may wrongly suggest that there are no worthwhile foreign scholarly works available in this field.

Foreign publications that are included in the other chapters require more attention to spelling; and in the case of some generally less known languages, a parallel translation of titles into English would have been highly desirable.

While the coverage of the political science literature, particularly for the years 1965–1970 is good in general, there are some obvious weaknesses and omissions. First, the economic factor in international relations and politics, e.g., foreign trade and aid, is hardly touched upon, with the exception of a short section on the underdeveloped areas. Further, we find among the elaborate subdivisions along regional, political, and ethnic lines also several blanks: there is no entry on Eastern Europe and hardly any literature is listed on most of the Eastern European states. We encounter similar gaps in the section on the Soviet Union, where works referring to the federal structure and the multinational composition of the U.S.S.R. are scarce or not represented at all. On the basis of the given compilation, a reader may get the impression that only Russians, Balts, and Turkmens live in the Soviet Union, and that such issues which are covered within a different context, as nationality problems, civil liberties issues, and the Jewish question, do not apply to this part of the world.

In spite of these shortcomings, the *Bibliographical Guide* must be recommended as a valuable, concise reference work to contemporary political science literature.

IHOR KAMENETSKY

Central Michigan University

Ideology, Reason, and the Limitation of War: Religious and Secular Concepts, 1200–1740. By James Turner Johnson. (Princeton, N.J.: Princeton University Press, 1975. Pp. 291. $12.50.)

Johnson has made a two-fold contribution to the history of ideas and practice with respect to war in the European tradition. By taking the limited period he has been able to go into more detail than I could in my *Christian Attitudes to War and Peace.* Second, he analyzes the views of a great many writers from the late Middle Ages through the Puritan revolution with a fullness not achieved elsewhere to my knowledge.

In an epilogue the author tells us that we need a fresh appraisal of "the possible relations between ideological and non-ideological restraints on war." To this theme he proposes to devote a future study. It will be welcome.

Some of Professor Johnson's assertions call for discussion. He takes the formulation of the late Middle Ages as the classic doctrine of the just war, seeing in it a fusion of ideological and nonideological strains. I would be inclined to go back further: the just war concept in its essential features was formulated in classical antiquity from Plato through Cicero, and well suited the situation of the Greek city states, which were relatively equal in power, endowed with a common culture, and not desirous of exterminating each other. The conduct of war must, therefore, be subject to restraints. Cicero elaborated the rules for restraint and insisted that the just war must be waged under the authority of the state. Hence a revolution could not be a just war. Augustine took over and Christianized the concept, adding that the motive must be love, and churchmen should not be involved.

For some eight centuries after Augustine Europe was harassed by successive invasions and the code in practice went by the board. In the eleventh and twelfth centuries came efforts at restraint and coincidently the crusade. Johnson objects to my calling this a separate category because the scholastics subsumed it under the rubrics of the just war. Granted, but they did so only by subversion. Johnson might be the more ready to regard the crusade as a separate category had he used Carl Erdmann, *Die Entstehung des Kreuzzugs Gedankens* (Stuttgart, 1955), and even more had he given attention to the just war and revolution, which by definition is excluded. The problem became acute in the sixteenth century when the Catholic states attempted to wipe out Protestantism. How could subjects in these states resist without revolution? The solution was found by making distinctions between grades in the magistracy. Lower authorities, also wielding the power of the state, could resist the higher: the princes in Germany against the emperor, the princes of the blood against the crown in France, parliament against the king in England. Before long the device was abandoned in favor of popular sovereignty, combined with the Holy War, which Johnson recognizes as a synonym for the crusade and concedes that it disrupts the theory of the just war.

He takes issue with my statement that in the prosecution of war the crusade is unsparing. I did point out that the crusading Independents committed no atrocities against the Scotch Presbyterians and I might have added against the English Anglicans. Abundant examples can, however, be given of atrocities in crusades in accord with Cromwell's dictum that the Christian should love the enemy as *his* enemy and hate him as God's enemy. War is more humane when God is left out of it.

The most cheering note in the book is the hope that the concept of humanity may induce restraint.

ROLAND A. BAINTON

New Haven, Connecticut

Discretion to Disobey: A Study of Lawful Departures from Legal Rules. By Mortimer R. Kadish and Sanford H. Kadish. (Stanford, California: Stanford University Press, 1973. Pp. x, 241. $8.95.)

The problem of political obligation is ancient, recurring, and fundamental. The historical and logically possible responses to the issues which it raises are also varied, if not contradictory. Regardless, much of the literature on the subject tends to portray similarly a basic dilemma. The individual must choose either to obey the law and to act legally or to disobey it and to act illegally. The alternative of disobedience which is lawful *by standards within the legal system* has seldom been studied.

Discretion to Disobey is important because of its well-illustrated analysis of precisely this possibility. The central thesis of the book is the lawfulness and desirability of some kinds of departures from mandatory legal rules. The authors, a philosopher and a law professor, do *not* deny that such rules impose genuine legal obligations. Rather, Mortimer and Sanford Kadish argue that these duties are "remittable" under some conditions. The discretion to disobey lawfully in this sense pervades the American legal system, the major focus of the book. The opportunity to engage in such disobedience is facilitated by "recourse" roles. They extend a "liberty in handling obligations" (p. 35), the nature of which is examined in the first chapter.

The second chapter is a valuable analysis of lawful departures from mandatory rules by officials. At times they have the liberty to substitute their own judgments of what is appropriate for the dictates of the law. To be "legitimated" these "interpositions" must be practically effective, institutionalized, and legally unreviewable. One of the several examples which the authors analyze in depth is the power of the jury to acquit in criminal cases. The existence of this kind of liberty presupposes "deviational" discretion. It is the "exercise of authority in ways . . . either unauthorized or prohibited by rules of competence" (p. 42). This authority is obviously incompatible with the classical model of the rule of law, which the authors criticize on familiar grounds.

The third chapter is an analysis of "legitimated disobedience," or lawful departures by citizens from legal rules. The authors assail the prescription by advocates of "law and order" for unqualified compliance with legal rules. Under certain conditions the citizen is, and ought to be, exempt from the usual liability to punishment for disobedience. The norms of invalidity and the lesser evil constitute, for example, possible defenses against criminal prosecutions. A defendant is entitled to argue that the rule which he or she disobeyed is unconstitutional or invalid for some other reason. A person accused of a crime may also claim that his or her "illegal" action was the lesser of the evils under the circumstances. If the judges are persuaded by these arguments, then the defendant is not legally punishable.

This analysis suggests that nonliability to punishment is one of the requirements of legitimated disobedience. Mortimer and Sanford Kadish emphasize exactly this point throughout most of their book (especially pp. 142, 147). Disobedience which satisfies the other requirements, but is punished, is not legitimated. This implication is harsh, but understandable; otherwise, the conceptual problem arises of how actions which the legal system punishes can be lawful. At times, however, the authors take a position which raises precisely this difficulty. They imply that certain kinds of departures from mandatory rules are legally justified even if punished (p. 99). The justification of this contention appears logically to entail the appeal to a "higher" law. The resort to such a standard obviously changes the nature of the central thesis of the book. The ambivalence which results obscures not only the "message" of the authors but also the concept of law which it presupposes.

The fourth chapter is an evaluation of legitimation as a social strategy. The authors conclude that its values, "despite the risks, may possibly prove compelling" (p. 177). They justify legitimated interposition as a means to achieve justice in special cases and to adapt law to social change. They also argue that deviational discretion may be preferable to the deliberate use of delegated discretion. Mortimer and Sanford Kadish defend legitimated disobedience on various grounds, among them that it breeds respect for law, expands freedom, and encourages citizen participation in government.

The fifth chapter spells out the implications of the foregoing arguments for the concepts of law and legal obligation. The authors describe "principles of acceptance" which are the ultimate criteria for the application of mandatory rules. The existence of these norms must be inferred from the operation of the legal system rather than explicit rules. Legal realists, positivists, and proponents of natural law are criticized for their neglect of these standards. Mortimer and Sanford Kadish also object to the rational-bureaucratic model of a legal system. They conclude with a defense of their preferred model of checks and balances.

The critical part of this chapter vividly illustrates the value of the advice which Ralph Waldo Emerson once gave Oliver Wendell Holmes, Jr. After reading a paper of the latter's on Plato, the great essayist commented: "When you strike at a king, you must *kill* him." Mortimer and Sanford Kadish strike at the royalty of jurisprudence, but do not slay them. The only fatalities are straw men. Very few, if any, of the diverse theorists whom the authors criticize in effect ignore "principles of acceptance." Most of them were aware of the existence and desirability under certain circumstances of deviational discretion. Their commitment to the rule of law was qualified. For example, St. Thomas Aquinas insisted that departure from the letter of the law is legally justifiable. Strict compliance with a legal rule could be harmful to the general welfare, which is contrary to the intent of the lawgiver.

Other evaluations and concepts of Mortimer and Sanford Kadish are also subject to criticism. The wisdom of their preference for deviational discretion is open to very serious doubt. Confining, structuring, and checking discretion along the lines urged by Kenneth Culp Davis may well be a better alternative. At the least his book, *Discretionary Justice,* is an excellent statement of the other side of the question. Nevertheless, the merits of *Discretion to Disobey* far outweigh these defects. It is a dispassionate, clearly written, stimulating, and relatively original study of a most important problem. As such, the book is a valuable contribution to political and legal philosophy as well as the study of the legal process.

WILFRID E. RUMBLE

Vassar College

Marxism and Imperialism. By V. G. Kiernan. (New York: St. Martin's Press, 1974. Pp. xi, 260. $18.95.)

Mr. Kiernan is British. He describes himself as an independent Marxist (p. viii). The book is a collection of six essays written at various times and for various occasions, introduced by a long introductory chapter on the Marxist theory of imperialism and its historical formation. Mr. Kiernan is well read in the historical and journalistic literature of the field. He writes with a pleasant, easy style, with occasional flashes of Scottish wit. He follows no party line. He is not quite what I would call a unitarian Marxist. (I suspect he thinks he believes in it, rather than believing in believing it.) He does think capitalism is here to stay, although he wants socialism (in some form not wholly specified) to triumph. He sees that there are important differences between American and European imperialism, and he sees imperialism as a problem of great complexity and diversity, with historical roots in sentiment, in imitative behavior, in upper-class cultures, and in religious and nationalistic feelings as well as in economic interest.

After the introduction, which is a useful exercise in the history of Marxist thought, there is a chapter on "Farewells to Empire," a review of mainly British literature reflecting the collapse of the legitimacy of empire. A chapter on "Imperialism, American and European," traces the uncertain course of imperialist sentiment in the United States, contrasting the "people's imperialism" by which a continent, empty by standards of European technology, was settled, with dubious ventures overseas. Chapter 4, "The Peasant Revolution," gives Portuguese Guinea about the same amount of space as China, and is rather critical, though not very thorough on Maoist thought. The last three chapters are on India; one, "Marx and India," which digs up some interesting material from Marx's journalist days; one, "Marx, Engels, and the Indian Mutiny." The last chapter is "India and the Labor Party," which is certainly the most entertaining and passionate in the book, and reveals that the British Labor Party was certainly more British than anything else and that its attitude toward India differed very little from that of the Conservatives. There is nothing much on the role of the Labor Party in the transition of India to independence and one would have liked to see another chapter on that, as it clearly represented an extraordinary rapid turnabout in opinion and policy.

I must confess that I found this volume, though frequently entertaining and instructive, another example of the curious sterility of Marxist thought when it comes to understanding complex social phenomena. The real trouble is that the categories of this way of thinking – struggle, dialectics, revolution, class, exploitation, capitalism, and socialism – are both too heterogeneous and too incomplete to bear the weight of analysis of really complex phenomena. It is like trying to do chemistry with the medieval four elements. Kiernan is clearly an intelligent and perceptive writer and he constantly transcends his Marxism, but he makes no attempt at any quantitative analysis, cites little real evidence, and includes an enormous amount of hearsay. One of the great tragedies of the last hundred years or so has been that the real moral insights and legitimate indignations of the Marxists should have been encapsulated in such a narrow prison of the mind as Marx's social dogma. This work is perhaps a sign that at least some of the inhabitants of the prison are breaking out of it. If so, one wishes them well.

KENNETH E. BOULDING

University of Colorado

The Ideology of Order: A Comparative Analysis of Jean Bodin and Thomas Hobbes. By Preston King. (New York: Barnes & Noble, 1974. Pp. 352. $20.00.)

Professor King's subject is absolutist ideology, characterized as "a universal justification for limitless movement towards centralization" (p. 257). The ideas of Bodin and Hobbes are examined in detail as the outstanding exemplifications of this ideology, for not merely do these authors describe absolutist politics and commend it as an appropriate response to specific circumstances, but they also "recommend ... the concentration of power in an *a priori,* which is to say illimitable, fashion" (p. 260). Although King provides some contextual information about the Wars of Religion and the Great Rebellion, his enterprise is not primarily historical: rather he is concerned with the logical and empirical validity of the absolutist ideology, both of which he disputes at length.

King finds, indeed, that the absolutist ideology is seriously defective in a number of respects. He objects to its *a priori* character, for its invariable recommendation is that power be concentrated, increasingly and illimitably, upon a sovereign authority. He does not deny that some concentration may occasionally be called for, but he regards absolutism as such as absurd.

Second, he finds improper the absolutist's subordination of the idea of justice to the idea of order: he argues that order is nothing more than the preservation of a set of rules concerning social conduct, and that these rules may legitimately be submitted to the criterion of justice:

> Every concrete order projects a hierarchy of norms. These hierarchies do not exist *prior* to what they control, but as *a part* of what they control. A political order, a political system, taken as a rank order of norms, does not precede justice; rather, it is an expression of a reasonably, but never a totally, coherent conception of justice.... Justice is not built upon an order, but is contained within it; and where an order contains an injustice, one does not build justice upon it, but merely changes the particular that is regarded as unjust; and if the injustice is monumental, change of a proportionate dimension may be required, and the existing order disordered in order to achieve a better one (pp. 284–285, emphasis in original).

Third, there is the contention that an absolute sovereign is the best (and probably the only) way of obtaining security and stability. For the absolutist "nothing is more important than order . . . there is virtually nothing which we may not be forced to sacrifice . . . to obtain it. Hence the belief that the power of government must be absolute, perpetual, unlimited, above the law – if it is to do its job properly" (p. 283). But King is unconvinced that absolute government does in fact do this "job," and he argues that most men feel (with justification) that security and stability are more likely to be the outcome of an open, pluralistic regime, in which they and their representatives are able to participate in the formulation and execution of public policy.

In scrutinizing the absolutism of Hobbes and Bodin, King thus has useful, if not strikingly original, comments to make. On one point however, he convicts Hobbes of seriously misleading his readers on a matter which other critics have neglected. While preferring monarchy, Hobbes held that a body of men could perform the functions of the sovereign as well as an individual. But King points out that a body must have certain rules of procedure and that the people concerned cannot purport to be above these rules as an individual monarch can purport to be above the law which he himself makes. It is, seemingly, possible for men to act in conformity with a set of rules in the absence of an over-awing sovereign power, a concatenation of circumstances which, in describing "the natural condition of mankind," Hobbes had rejected. Indeed, as King remarks, "Hobbes's entire argument for the absoluteness of the sovereign flows from the assumed impossibility of men being governed by *rules* (p. 249, emphasis in original).

Although King does not purport to write primarily as an historian, he cannot, one feels, entirely escape the proprieties of the historical mode of discourse, and some of his observations are so surprising as to suggest an inadequate knowledge of the early modern period. He tells us, for instance, that in the Great Rebellion "not only the royalist but also the parliamentary party held that the king could not be disobeyed" (p. 66): in fact, a number of royalist writers (e.g., Henry Ferne, Henry Hammond, John Spelman, James Ussher, even Hobbes when discussing the predicament of the Christian subject confronted by a non-Christian sovereign) made the traditional distinction between *disobedience* and *resistance,* allowing the former when the king's commands contravened those of God, while holding the latter to be strictly forbidden, *explicitly* by the Scriptures (e.g., in Romans 13 and 1 Peter 2) and *implicitly* by the order of God's Creation. For the Parliamentarians, the specific political order was a creation of "the people" whose representatives were to protect that order when it was threatened by the aspiring tyrant, while texts such as Romans 13 and 1 Peter 2 referred to legitimate rulers and gave no protection to the tyrant. Equally puzzling is the assertion on p. 20 that Locke's political thought pointed "in two contrary directions: the one towards some form of separation of powers, the other towards an unlimited concentration of power in the hands of the people." If Locke did, indeed, give any credence to such a "concentration," many a lecture will need to be rewritten.

Finally, one cannot but deplore the blurring of the impression of sharpness left by much of *The Ideology of Order* by King's occasional lapses into otiosity. While able to wound Hobbes with the thrust about rules noted above, King is also capable of telling us "that it is far more difficult to conceive of a group, collectively speaking, having a single will, than it is to conceive of a single individual having such a will. A single individual is a biological unit, a group of individuals is not. There is of course nothing startling about this reflection" (p. 275). Indeed.

JOHN SANDERSON

Strathclyde University, Scotland

The World and Ideas of Ernst Freund: The Search for General Principles of Legislation and Administrative Law. By Oscar Kraines. (University: The University of Alabama Press, 1974. Pp. 221. $8.00, cloth; $3.50, paper.)

Although the title of this book is somewhat misleading – inasmuch as the "world" of Ernst Freund does not surface very clearly in this short nonbiographical volume – the author provides a good summary of Freund's ideas and a balanced assessment of his considerable influence as a pioneer of important political and legal reforms.

To political scientists of an earlier generation Freund would need no introduction. That generation was less mesmerized by the natural sciences and their supposed methodological precision and was more aware than our own that the social sciences, including law and political science, as Felix Frankfurter once said, are merely conventional, but convenient, departmentalizations of "holistic perplexities." Lines between law and political science were then less arbitrarily drawn, and public law was a common, respected domain of both disciplines. Hence, while I share Professor Kraines's hope that this book will reawaken an interest among political scientists in Freund's work, I entertain no high expectations in that regard.

At the beginning of his professional career Freund held appointments in political science, initially at Columbia and later at the University of Chicago. In 1902 he became a charter member of the law faculty at Chicago, where he served until his death thirty years later. In 1903 Freund helped found the American Political Science Association and he served as its president in 1916. Throughout his career he was a frequent contributor to political science, as well as law journals.

Freund, says Kraines, was neither theorist nor ideologue. Like Frankfurter, his friend and associate in numerous undertakings, he was an activist scholar, pioneering or caught up in movements for social and legal reform. His published work – consisting of twenty books and pamphlets and ninety articles – is marked by extraordinary erudition, acute perceptions of sociopolitical trends, and penetrating analysis. Most of his work is explicitly prescriptive. Because some of Freund's writings are occasional or address matters in which he possessed little expertise, their relevance today is very limited. However, many of his books and articles, despite their stylistic ponderosity, are worthy of renewed study and suggest potentially fruitful lines of further investigation.

According to Kraines, legislation and administrative law are the fields in which Freund's scholarship was most sustained and his contributions most enduring. These contributions are summarized in two chapters, "The Legislative Response" and "The Administrative Response," which together comprise about two-thirds of the book. Both subjects preoccupied Freund throughout his career. Of the four books upon which his scholarly reputation largely rests, *The Police Power: Public Policy and Constitutional Rights* (1904) was the first, and is somewhat atypical for its emphasis upon constitutional law and case analysis. Noteworthy and, at the time controversial, for its expansive conception of governmental power over the economy, it became a standard citation in judicial opinions, displacing Thomas M. Cooley's *Constitutional Limitations* as laissez-faire constitutionalism gradually receded. *Standards of American Legislation* (1917), perhaps the broadest and most provocative of Freund's works, minimizes without rejecting completely, the role of the judiciary – and of constitutional and common-law-norms – in the adjustment of complex socioeconomic relations. Freund's emphasis, according to the author, was upon a science of legislation encompassing such principles as "correlation of statutory provisions . . . standardization . . . and protection of vested rights and equality." To implement these principles Freund proposed numerous reforms, most of them now taken for granted, including the introduction of statutory analysis into law school curricula, provision for professional bill drafting services in legislatures, and delegation of rule-making power to administrative commissions.

As Kraines points out, Freund, together with Frank J. Goodnow and Frankfurter, pioneered the development of administrative law as a fit subject for research and teaching. The author provides a critical account of the contributions and perspectives of each. Freund's casebook, published in 1911 and again in 1928, as well as his last two treatises, *Administrative Powers Over Persons and Property* (1928) and *Legislative Regulation* (1932), emphasize the subject of administrative discretion, with Freund, unlike Frankfurter, looking to legislative norms and internal controls, rather than to the judiciary, for solution.

Professor Kraines is to be commended for this generally perceptive account of the writings and activities of one of this century's most eminent political and legal scholars. For anyone embarking upon a reading of Freund, this study, which places the major works in a broad conceptual framework, is a valuable introduction.

CLYDE E. JACOBS

University of California, Davis

An Essay on the Theory of Enlightened Despotism. By Leonard Krieger. (Chicago, Ill.: The University of Chicago Press, 1975. Pp. xi, 115. $9.25.)

In this essay Professor Krieger attempts to demonstrate the usefulness of the concept of enlightened despotism for historical analysis by arguing for a particular understanding of the conceptual status of the term itself. Krieger's argument is of general interest for he concludes that enlightened despotism, properly understood, is a relevant political concept today, "a permanent category, a continual option, and a recurrent choice in our politics." No longer merely an "evanescent stage in the history of autocracy," enlightened despotism is now seen as "a comparatively credible recourse against confusion" which people turn to when "confused and divided about their own interests and values" and unable to resolve "exacerbated social divisions" (pp. x, ix).

Examining enlightened despotism in the context of the eighteenth century, Krieger rejects two established approaches to the concept, and offers a third. The traditional view of enlightened despotism as a simple empirical term describing the predominant political practice of the period is seen as untenable in the face of substantial contrary evidence: actual political practice was much more diverse than the term suggests; enlightened despotism was only infrequently advocated as a political doctrine; and the concept is fundamentally self-contradictory. Krieger rejects as historically ungrounded the alternative approach of adopting the created category of "enlightened absolutism" and imputing it to the eighteenth century as an interpretative tool. His solution is to reinstate "enlightened despotism" with a new status as both sign and symbol: it has a specific literal reference, but also alludes to a more general condition. Enlightened despotism which had "a basis in contemporary usage but also transcend[ed] it," Krieger labels a "schema" "in the Kantian sense of a synthesis mediatory between the empirical and the categorical and enabling the organization of the literal manifold under unifying concepts" (p. 34).

The power of the "schema" to illuminate the larger situation rests on its self-contradictory character. The illogicality of the term captures the contradictory character of eighteenth-century life, specifically the tension between liberal "enlightenment" social goals and the absence of any instrument of change other than a despotic monarch. Thus understanding the internal structure of enlightened despotism enables one to grasp the larger political and social picture.

Krieger's attempt to articulate a historically informative concept is frustrated by his methodological goal of establishing enlightened despotism as a permanent, transcendent category, applicable to all historical periods. Failing to root it in concrete historical conditions, he does not realize the informative power of the idea. If, as Krieger says, enlightened despotism was an attempt to resolve "newly exacerbated social divisions," the schema can only be used to illuminate that situation if those divisions are clearly identified. Otherwise the schema only refers to an abstraction. Krieger for the most part remains on an abstract level, referring to unspecified "material conditions" and "intersections of tradition and innovation." Yet whatever content the term has in the essay is due to its specific historical connections. Krieger admits that enlightened despotism "referred to a particular political institution – the institution of monarchy – by whose peculiar characteristics it was inevitably shaped" (p. 89). Yet he never examines the institution of monarchy or any other social or political institution.

The other side of Krieger's failure to make his concept genuinely informative is the failure to explain the appearance of the concept itself. Clearly the doctrine of enlightened despotism appeared only in certain countries, during a specific period. Krieger offers no explanation of this fact. The final, ironic, consequence of the failure to provide a concrete historical basis for the concept is its uselessness as a contemporary category of analysis. Without knowing the specific conditions that enlightened despotism referred to, one cannot know what modern conditions are analogous to those of the eighteenth century, and so appropriate for a similar analysis.

MARK GAVRE

University of California, Los Angeles

The Intellectual Origins of the Prague Spring: The Development of Reformist Ideas in Czechoslovakia. By Vladimir V. Kusin. (New York: Cambridge University Press, 1971. Pp. 153. $8.95.)

Writers Against Rulers. By Dusan Hamsik. (New York: Random House, 1971. Pp. 208. $6.95.)

Books written by émigrés or "insiders" – persons who participated in events rather than observed them from a distant vantage point – tend to have both advantages and disadvantages. On the one hand they usually benefit from a wealth of on-the-spot information as well as a comprehension of the subjective factors involved and their relative importance. The insider need not be limited by the printed word, nor need he falter in the realm of speculative guesswork, the reading between the lines so vital to the outside researcher of closed societies. On the other hand, the pitfalls of this type of writing usually include a tendency to self-justification – individual or collective – as well as the grave risks not only of subjectivism but of the failure to perceive a general coherent pattern and meaning of events.

Vladimir Kusin has almost totally succeeded in avoiding these pitfalls in that he has presented a most broad, highly coherent picture of the ideas behind the Prague spring, in a variety of fields ranging from pure philosophy, economics, culture, political thought all the way to nationalism and foreign policy. One might have hoped for a still broader discussion of these ideas, more of the contrasting views and proposals, especially in the political but also in other spheres. Yet Kusin does provide the essential views put forth and in so doing presents a loyally characteristic picture of the ideas which generated the Czechoslovak reform movement. Where Dr. Kusin is less successful is in his perhaps subjectively influenced neglect of the Slovak intellectuals and their roles as well as their ideas in the reform movement. While he does discuss them in a few pages (in a chapter on national aspirations which is almost entirely devoted to a highly intelligent discussion of Czech nationalism), it is extraordinary that a book which justifiably emphasizes the role of the intellectuals should fail to discuss the Slovak theoreticians or analyze the complex position of the Slovaks in the reform process.

Moreover, the author does not see the twelve years (1956–1968) he presents as a process but rather a period of gestation – explaining perhaps his insistence upon 1956, rather than the early 1960s, as the beginning. For the gestation did in fact begin in 1956 with the de-Stalinization in Moscow, but as Kusin himself points out, the Stalinist Czechoslovak leaders, unlike their Polish and Hungarian counterparts, were able quickly to stop the immediate reform-minded response which manifested itself among the writers, students, even some party historians and theoreticians (like Kusin himself). Creative minds were indeed striving and working thereafter, and important works were even written, if in many cases suppressed. Nevertheless, the process itself did not resume until the second round of Soviet de-Stalinization, i.e., in the early 1960s following the twenty-second CPSU congress and a number of internal Czechoslovak events – not least of which was the economic crisis of 1962–64, but most of which were connected with the pressures building for de-Stalinization.

The dynamics of the 1963–68 period are missing from Kusin's book: the push for reform, the setbacks, the growing strength of the reformers within the Party, the clumsy efforts of the Stalinist leadership to give with one hand and take with the other. These events culminated finally in the creation of a temporary coalition of sizable forces within the Party to oust the outdated leadership, and brought to power men more responsive to the by now clearly developed demand for political reform. Not that the facts are missing from Kusin's book (though in one surprising inaccuracy he claims that Dubcek was named first secretary of the Slovak CP in 1966 rather than 1963). Rather it is the dynamic of this pre-1968 period – whether begun in 1956 or 1963 – which is missing. This omission need not, however, detract from the overall value of the book, which is, as intended, a comprehensive, sophisticated, and most rewarding exposition of the basic theoretical tenets of the Czechoslovak reform movement.

By contrast, Dusan Hamsik's book, a more personal account of a participant, has much more limited objectives than Kusin's work. Focusing on the Czechoslovak writers' struggle of 1967, which was one of the factors precipitating Novotny's downfall in January 1968, Hamsik also strives to analyze the evolution of the Czech intelligentsia which caused its revolt against the regime. Like Kusin, Hamsik emphasizes the way in which this revolt cut across Party lines, uniting Communist and non-Communist intellectuals alike, but, also like Kusin, Hamsik is interested primarily in the disillusion and frustration of the one-time believer, the Communist intellectual who cherishes the ideal but seeks a total reform of the Soviet-East European version of its implementation. Thus this book is of value not only for the rare insider's view it presents of day-to-day Communist Party control over cultural and intellectual life in Eastern Europe, but also for its analysis of the motivating forces which propelled a good portion of the Czech intelligentsia in 1968. (Like Kusin he gives little attention, though more credit, to the Slovaks – and to the students.) This analysis is all the more useful because it was written in 1968, the last chapter closing with January 1968, leaving only a prologue to add the hindsight.

GALIA GOLAN

Hebrew University of Jerusalem

Social Indicator Models. Edited by Kenneth C. Land and Seymour Spilerman. (New York: Russell Sage Foundation, 1975. Pp. 424. $15.00. Distributed by Basic Books, Inc., New York.)

This is an important book. It supplies a paradigm for social indicators which relates them to the mainstream of social science. By defining social indicators as variables in models of social processes, it clarifies the conditions under which social indicators will be useful to policy makers. It points to a social accounting framework as the mature form that social indicators will take. And it sets a new high standard for the integration of theory, methods, and data in social indicators research.

Most of the fifteen chapters were written by social scientists who participated in a Conference on Social Indicator Models held at the Russell Sage Foundation in July 1972. Thirteen chapters are grouped under two headings: (1) replication models based on data from repeated cross-sectional sample surveys, and (2) longitudinal and dynamic models using repeated observations on the same individuals or structural units. They are preceded by a brief introductory chapter by both editors and a major essay by Land.

In his essay "Social Indicator Models: An Overview," Professor Land distinguishes three types of social indicators, each designed for different uses. *Output descriptive indicators* are measures of the end products of social processes and are most directly related to the appraisal of social problems and social policy.

Other descriptive indicators are more general measures of the social conditions of human existence and the changes taking place therein. *Analytic indicators* are components of explicit conceptual models of the social processes which result in the values of the output indicators.

The model of a social process would be a system of relationships connecting all the variables involved in it. Land would divide these variables into two endogenous and two exogenous categories. The endogenous categories are (1) *output* or *end-product* and (2) *side-effect* descriptive indicators; the exogenous categories are (3) *policy-instrument* and (4) *nonmanipulable* descriptive indicators. This closely parallels the structure of Jan Tinbergen's theory of economic policy, one of the most fruitful conceptualizations in modern economics.

Land also suggests a classification of social indicator content areas according to type of activity, institutional organization, and distributive consequences, and gives a schematic representation of the sociological life-cycle, "a life-cycle ordering of distributed social attributes." Finally, he suggests that the life-space of an individual consists of three measurement domains; (1) objective conditions (the external physical and social conditions of the individual's existence), (2) subjective value-context (the individual's beliefs, expectations, and aspirations), and (3) subjective well-being (the individual's feelings, satisfactions, and frustrations concerning components of the first two domains).

The section on replication models begins with a powerful conceptual chapter by Stinchcombe and Wendt on theoretical domains and measurement in social indicator analysis. There follows an exposition by James A. Davis of the log linear analysis of survey replications based on new methods developed by the statistician Leo Goodman. The remaining chapters were written by Otis Dudley Duncan and several younger scholars who have been closely associated with him. Several chapters grow out of the 1971 Detroit Area Study, which was designed under Duncan's direction as an omnibus replication of several earlier surveys made in the Detroit metropolitan area. Chapter 9 by Featherman and Hauser describes their design for a study of social mobility in the United States, replicating the 1962 survey used by Blau and Duncan in their classic work, *The American Occupational Structure.*

The section on longitudinal and dynamic models gives central importance to *transition matrices.* The members of a population existing at time t are classified into a mutually exclusive set of n categories or "states." The transition matrix transfers each member from the particular state he occupies at time t to the particular state he occupies at time $t + 1$. Demographers have used such matrices to show the effects of changes in age-specific fertility and mortality rates upon the total numbers and age distributions of human populations.

Richard Stone, in his chapter on transition and admission models in social indicator analysis, applies transition matrices to the movements of students through an educational system, of patients through a system of medical care, and of employees through an organization. Judah Matras applies them to models of organizational growth and transformation, and James S. Coleman to the analysis of occupational mobility by models of occupational flow.

Transition matrices seem to provide an ideal framework for social accounts which would describe the movements of individuals through "a life-cycle ordering of distributed social attributes," including age, educational achievement, income occupation, marital status, and any others that may be relevant to an evaluation of their well-being. Policies to correct perceived inequities would require increases in the transition coefficients which transfer members of disadvantaged groups into preferred statuses; hence, the coefficients themselves may be viewed as end-product descriptive indicators in policy-oriented models of the relevant social processes. Coleman concludes that such a social accounting framework is the mature form that social indicators will take.

The volume as a whole reflects an indigenous revolution within sociology comparable to that which transformed economics during the 1930s, 1940s, and 1950s. Apart from Stone, Duncan, and Coleman, all authors were under 45 at the conference date and at least seven were under 34. Only 17 of the references cited were published prior to 1955; 56 appeared in 1955–64 and 231 in 1965–73! Four of the authors also contributed to *Structural Equation Models in the Social Sciences,* edited by Goldberger and Duncan (New York: Seminar Press, 1973), another landmark of the sociometric revolution linking it to similar developments in economics and psychology. The Land and Spilerman volume shows us what serious empirical research will be like under the new regime. In 1967 the idea of a Council of Social Advisors was premature; it may not be so in 1977.

KARL A. FOX

Iowa State University

Jean-Jacques Rousseau, Ecrivain Politique (1712–1762). By Michel Launay. (Grenoble: A. C. E. R., 1971. Pp. 511. Price unknown.)

Rousseau's writings treat so vast a range of problems with such richness, subtlety, and paradox, that no single interpretation is likely to be fully satisfying. What Michel Launay, professor at the University of Nice, says of the *Emile* and *Social Contract* holds all the more for the whole of Rousseau's thought: "having given birth to thousands of commentaries, they have not for all that yielded up all their secrets" (p. 358). So much is buried in Rousseau's writings that the scholar can unearth ample materials for interpretation and reflection on the subjects with which he is most deeply concerned. Launay's fundamental concern is with the egalitarian and liberating possibilities of politics, and this concern constitutes the unity of his long, sometimes diffuse, important study of Rousseau's writings through 1762 (*Emile* and *Social Contract*).

Launay approaches Rousseau as a political writer in a twofold sense: Rousseau is not only a writer about politics; he is molded by real political experiences, beginning with his youthful Genevan political education, and in his writing devotes increasingly knowledgeable and artful efforts to producing political effects on different audiences. In interpreting Rousseau, Launay combines biographical and historical inquiry with analysis of Rousseau's texts.

Launay's approach produces its best insights through investigating Rousseau's understanding of the difficulties of the political writer. Reflecting on the Abbé de Saint-Pierre's shortcomings, Rousseau was able "to diagnose the causes of philosophers' and philanthropists' political impotence, and to try to develop a remedy for it, by a wholly different, more efficacious combination of reason and sensibility. . . . Ruse or skillfulness, no longer reason, virtue or force, revealed itself as the essential weapon [of the political writer]" (p. 240). Rousseau therefore meditated on the character and situation of his addressees with sensitive subtlety and realistic political analysis, fashioning his writing so as to achieve the greatest impact on his readers. This kind of political-literary analysis enables Launay to uncover an underlying unity in Rousseau's apparently diverse political messages. The *Nouvelle Héloise,* for example, does not reflect a change in Rousseau's principles, but rather it reflects his way of influencing his French readers; it would be his "reformist tactic adapted to a time and a place, to a certain *conjoncture,* and would not exclude a more daring policy in other times and places" (p. 306). Launay's account of Rousseau's relation to political forces in Geneva is similarly helpful for grasping how his coherent political thought is adapted to another specific political audience in the *Letter to d'Alembert* (pp. 341–349).

When dealing with political influences on Rousseau's thought, Launay is treading on rather treacherous ground. His account of Genevan political ideas is interesting in itself, but his claims for its utility in understanding Rousseau's thinking are overdrawn: "many aspects of Rousseau's messages, and not only the details, but the very meaning of his works, would have disappeared for us, if we had not discovered the depth of his attachments to a historically defined social *milieu,* Geneva's class of artisans" (p. 7). Launay shows that certain political conceptions current among Genevan artisans appear in detail in the *Social Contract* (pp. 43, 66); but since in Rousseau's writing they are grounded in complicated reasoning, I find that the Genevan popular antecedents shed little light on the grounds, consequences, or problems of Rousseau's teaching. The greatest danger of biographical-historical interpretation, to which Launay occasionally succumbs, is to explain away an argument that seems bizarre or uncongenial to one's own perspective instead of trying to fathom the author's thinking. To give just one example, Launay suggests that Rousseau's theoretical rigor is bent by the prejudices of the Genevan *petite bourgeoisie,* when he argues that a woman's adultery dissolves the bond of the family. Why, Launay asks, should not the true father simply be he who fulfills the duty of raising the child? If, instead of dismissing Rousseau's position lightly, one gives further reflection to the reasons that underlie it, one is led to a deeper understanding of the problematic character of human attachments – their natural roots and their conventional developments.

But Launay has too great a respect for the integrity, profundity, and abiding relevance of Rousseau's texts to do this kind of injustice to Rousseau very often. On the whole, he makes intelligent and interesting efforts to grasp the vital coherence of Rousseau's work as a political writer. Because he discusses so many writings, no one is treated exhaustively; but he makes at least some noteworthy points about each. Of particular value is his discussion of the *Emile* and *Social Contract.* He argues convincingly that these two works together constitute a complete whole. Much of Emile's education is a formation of his sentiments and passions such that he will take seriously the contracts – of friendship and marriage as well as the social contract – through which free men can sustain honest and decent relations with each other. What we learn from the *Emile* "on the affective conditons for the good functioning of every contract made between men, sensitive and active beings" (p. 421), helps us to make coherent sense of the *Social Contract,* and in particular to avoid a simply mechanistic misunderstanding. Launay argues forcefully for the realism of Rousseau's politics, and in this connection shows how the neglected (not to say despised) chapters on Rome in Book IV of the *Social Contract* play a vital role. Rousseau is not abstract or utopian, but concerned with the possibilities of concrete history, past and future; the image of Rome offers to our reflection "a middle term between the rules of right (wooden rules, which are rigid but able to judge the real) and this real, which is never completely made, which is to be made" (p. 444).

In his conclusion, Launay makes clear that the social contract is not the best model of human relationships for Rousseau, but that, in our world, the contractual attitude, although not the most generous, is the most useful for free men. This judgment points to Rousseau's view that political life or citizenship is not the simply highest human attainment. The problem of the ultimate status of politics in Rousseau's understanding is a theme of his autobiographical writings. Launay speaks of working for another ten years on these and the other remaining writings of Rousseau. Launay's method might be especially useful for their interpretation, and we can look forward in a few years to the publication of further results of his researches on Rousseau.

JAMES H. NICHOLS, JR.

The New School for Social Research

Marxism and Epistemology: Bachelard, Canguilhem, Foucault. By Dominique Lecourt. (Atlantic Highlands, N.J.: Humanities Press, 1975. Pp. 223. $15.00.)

The book is a collection of essays by M. Dominique Lecourt, one of the founders of the *Union de Jeunesse Communiste Marxiste-Leniniste* and a professor at the University of Paris. The essays deal with the philosophy of science of Gaston Bachelard, Georges Canguilhem and Michael Foucault. Nearly three-quarters of the book is devoted to Bachelard, the most important of the three thinkers, while the other two get under thirty pages each.

Bachelard's philosophy of science represents a strong reaction against the influence philosophy has traditionally exercised over science. In his view philosophy's influence over science is necessarily bad, and constitutes an "epistemological obstacle" to the latter's growth. Science is concerned to understand and explain the world, whereas philosophy is motivated by religious, moral, political,and other "ideological" interests. Philosophy therefore has an inherent tendency to corrupt science by chartering it in the service of extrascientific interests. Further, science is experimental, innovative,and dynamic. It is inherently revolutionary and develops by "dialectical leaps." Its history is a story not of continuous development but "envelopment," not of organic growth but periodic "relinquishment." Philosophy, on the other hand, is concerned with eternal and absolute truths. It aims to comprehend reality in terms of static and timeless concepts and has a "tendency to immobilism." In its approach to history it therefore looks for similarities and continuities, and endeavors to demonstrate that no system of thought is ever completely new. Bachelard thinks that such an evolutionary view of the history of thought necessarily misrepresents the growth of scientific knowledge, and offers the scientist a totally false image of his discipline.

In Bachelard's view the scientific is an independent and autonomous activity, governed by its own criteria of truth and values with which philosophy has nothing whatever to do. It creates its own subject matter and develops its own methods of investigation and procedures of verification. Like Louis Althusser, his intellectual grandson, Bachelard argues that the objects of scientific study are not naturally given, but created by the scientist with the help of the instruments and procedures developed by the scientific community. For example, a chemist is concerned to study pure chemical substances. Pure substances, however, are never to be found in nature. The chemist must therefore purify natural substances with the help of available techniques and in accordance with generally accepted procedures. The object of his study thus rests on "a kind of social guarantee." He has no other means of knowing that it is his proper subject matter than the consensus and agreement of the scientific community.

Bachelard's philosophy of science assigns considerable importance to scientific instruments. He calls them "materialized theories." A scientific instrument is not merely a piece of machinery, but an objective embodiment of scientific intelligence. It defines and creates the scientist's world. Science "instructs itself by what it constructs." This is true "scientific phenomenology." For Bachelard a concept is scientific only "insofar as it has become technical, is accompanied by a realization technique." Bachelard rightly calls his philosophy of science "technical materialism." It is an epistemological counterpart of the familiar Marxist theory of historical materialism.

Bachelard's theory of science leads him to take a very different view of the role of mathematics in science from the one common in the traditional philosophies of science. For him mathematics is not merely a language of expression but a mode of thought: "The physicist thinks the experiment with his mathematical thought." Mathematics does not therefore merely *express* but also *stimulates* scientific creativity. Bachelard argues that the hypothesis of the meson, for example, was "initially an essentially mathematical hypothesis" suggested by mathematical equations, and not a result of new experimental data (p. 56).

After expounding Bachelard's basic ideas at some length, Lecourt outlines those of Canguilhem and Foucault. Unlike his teacher Bachelard, Canguilhem is interested in the biological not the physical sciences, and leans towards a vitalist philosophy of nature. However, like Bachelard, he too aims to develop a "science of science," and to combat philosophy's necessarily ideological influence on science. Like him he insists that science develops by revolutionary and dialectical "leaps," and inhabits a very different world from that of ordinary observation.

M. Lecourt's discussion of Foucault is very brief, and centers around the latter's *Archaeology of Knowledge.* He briefly outlines Foucault's dialectical and evolutionary view of the growth of science. He refers to but does not discuss Foucault's fascinating analysis of the nature of the ordinary and the scientific discourse, or his well-known distinction between practical and theoretical ideologies on the one hand and science on the other.

M. Lecourt has written an excellent book. He makes available to the English-speaking world some of the fascinating ideas of the French philosophers of science. He also points to interesting similarities of approach between them and Kuhn. One consequence of this is that Kuhn appears less original than he is generally taken to be. Lecourt's book has the further advantage of showing that there are other ways of understanding the scientific activity than Popper's and Kuhn's, the two most popular but ultimately unsatisfactory philosophies of science.

Without wishing to detract from this lucid and well-argued book, I would like to end by drawing attention to its two minor defects. First, M. Lecourt does not fully explain why Bachelard and others are led to revolt against philosophy. As a result their preoccupations appear somewhat eccentric and pointless. It would have been helpful if Lecourt had sketched the background of the revolt, and traced its origin to the historic confrontation between Marx and Hegel. Second, M. Lecourt does not subject the three philosophers to a searching critique. He does not, for example, fault Bachelard's theory of truth, his conventionalism, dualism, and ambiguity about the relationship between the scientific and the ordinary world, but remains content to reiterate the familiar Marxist thesis about the inadequacy of a philosophy of science not grounded in historical materialism.

BHIKHU PAREKH

University of Hull, England

The Unravelling of America: Wherein the Author Analyzes the Inadequacy of Current Political Options and Responds with a Christian Approach to Government. By Stephen V. Monsma. (Downers Grove, Ill.: InterVarsity Press, 1974. Pp. 228. $4.95, paper.)

Professor Monsma, author of *American Politics: A Systems Approach,* assays here a critique of leading political philosophies, in both domestic and foreign policy spheres, and offers his own alternative of "Progressive Realism."

The book deserves a wider audience than it is likely to get. Many will be turned off at once by the framework of "Evangelical Christianity" which he explicitly espouses and applies. If they read on, however, they will find a kind of humane utilitarianism, coupled with an unromantic view of human nature, which is thoroughly compatible with a variety of theological (or humanistic) perspectives; and the specific policy applications would win applause from such theologically diverse a crowd as Adlai Stevenson, Reinhold Niebuhr, and Pope John.

Monsma finds in the Bible two principles relevant to the realm of politics. The first is the doctrine of original sin, which he interprets to mean that man, while not totally depraved, is a mixture of noble impulses and "inordinate self-love" (the affinity with Niebuhr is striking). Second, the doctrine of man's creation in the image of God means that it is the duty of government to maximize opportunities for each person to exercise and develop his creative capacities. Working from these postulates, he finds elements of validity and invalidity in each of the major political philosophies. Burkean conservatism properly emphasized organicity, but provided no independent norm for judging injustices which inevitably corrupt any society, and "the past becomes a prison" (p. 79). Classical liberalism was commendable for its emphasis on the dignity of the individual, but naive about human nature. American liberalism properly emphasizes the use of government to help the disadvantaged – but overestimates the unselfishness and rationality of government officials. American conservatism avoids that trap, only to fall into its opposite, ignoring the injustice that exists under weak governments. The New Left properly points to dehumanization and materialism, but naively locates the source of the problem in the environment rather than in corrupt human nature. The result of such unrealistic diagnoses is, in every case, unrealistic prescription.

What, then, has Progressive Realism to offer? Not a blueprint but a set of middle axioms designed to maximize "true freedom" and minimize the effects of self-love. They entail an "activist, involved, humane politics" characterized by such principles as "rolling incrementalism" and "built-in administrative checks," and a calculus which balances "the relative number of persons who would gain rights or advantages and obligations or disadvantages, and the relative importance and value of the rights. . ." (p. 124). By way of brief illustration, the author applies the principles to health insurance and penology.

Turning to the international sphere, Monsma rather nicely examines foreign policy attitudes along two dimensions: idealism-realism, and liberalism-conservatism. Following the earlier pattern, he judges each of the four positions thus created (liberal idealism, etc.), develops his own alternative, and applies it briefly to a problem (intervention).

Evangelical Christians who are concerned about social justice will find this book of great interest indeed – for that branch of Christendom, which typically perceives conversion as the necessary and sufficient solution to all social problems, has produced few such treatises. Others will find it interesting, to the extent that they are concerned with the political philosophies of evangelical Christians – but also to the extent that they resist the temptation to stereotype a colleague on the basis of his theology.

GORDON L. SHULL

The College of Wooster (Ohio)

Hegel on Reason and History: A Contemporary Interpretation. By George Dennis O'Brien. (Chicago and London: The University of Chicago Press, 1975. Pp. 188. $8.50.)

If one accepts as reasonable the intentions which have guided Professor O'Brien in writing this book, then it can be said that he comes close to success. "My hope is to translate the insights of Hegel (and of Kojève) into terms more current in English philosophical work" (p. 8). More specifically, O'Brien has provided the student with an interpretation of Hegel's philosophy of history, based upon the material published under the title *Die Vernunft in der Geschichte.* This interpretation is decisively influenced by the Hegel commentary of Alexandre Kojève on the one hand, and by a limited but crucial reliance upon Aristotle on the other. It is externally influenced by the author's desire to make Hegel accessible to analytical philosophers of history, or to students of analytical philosophers. One senses beneath the self-imposed rhetoric of simplicity a solid understanding of Hegel's doctrine (with one or two qualifications to which I shall later turn). O'Brien has read widely in both history and philosophy, and he is able to connect his reading to an exposition of the central points in Hegel's study of history. The book is very well written, and contains many illuminating discussions of individual points. I mention some of these which I find especially helpful: the clear exposition of the status of the texts, in both the German and the English translation, upon which O'Brien is commenting (Chap. 1); the rejection of the distinction between method and content in history as it ostensibly applies to Hegel (Chap. 2); a brief but necessary reminder of the difference between Hegel and Darwin (Chap. 3); a persuasive account of how Hegel's philosophy of history, contrary to criticism leveled against him by Kierkegaard, Schelling, and many a subsequent thinker, is a doctrine of individuality (Chap. 4); a fine discussion of the role of passion in history (Chap. 5); and, in the best single section of the book, an admirable account of the sense of Hegel's doctrine of progress in history, and how he reconciles human freedom with necessity (Chap. 6).

If we consider the work as a whole, O'Brien wishes to translate Hegel into contemporary language by explaining the meaning of the famous claim that reason is the "law of the world" (p. 36 *et passim*). In order to do this, O'Brien both connects Hegel's procedure with that of contemporary philosophers of science, and (more fundamentally) distinguishes it from philosophy of science in order to relate it with connoisseurship in art (pp. 44, 132, 165ff). Hegel emphasizes objectivity and *a priori* elements in the study of history, but he is fundamentally concerned to show the unique value and intelligibility of individual style. This strikes me as on the right track, but it runs the risk of transforming Hegel into an "aesthete." It is, however, true that O'Brien counterbalances this particular danger by underlining the political character of Hegel's teaching. The state provides the matrix for the fulfillment of the individual as the concrete universal (cf. pp. 123, 132, 145 and 146–158). Nevertheless, it is not clear from O'Brien's account exactly how the concrete universal, the product of the state, is for Hegel other and higher than a great work of art.

There is a second difficulty with the analogy of art. O'Brien emphasizes the similarity between Hegel's categories and the key terms of Aristotle's doctrine: four causes, potentiality-actuality, etc. (pp. 45, 62, 145 *et passim*). O'Brien certainly knows that for Aristotle, the appreciation of art is superior to the study of history because it enables us to perceive the universal. But he does not explain the radically non-Aristotelian nature of Hegel's doctrine of individuality. I believe the trouble lies in O'Brien's debt to Kojève. Kojève combined elements of Marx, Feuerbach, and the early Heidegger in his attempt to read the *Phenomenology* as the penultimate version of philosophical anthropology (the ultimate version being Kojève's own reading). This entailed the virtual suppression of Absolute Spirit, and an exaggerated emphasis upon the "man-made" nature, not just of history, but of truth or wisdom. One result was that Kojève was unable to explain the difference between animal and human desire, or the emergence of self-consciousness; and he also exaggerated the difference between nature and history in Hegel. O'Brien repeats these errors. He accepts the Kojèvian version of the master-slave dialectic, which fails to explain how one "human" consciousness can recognize itself in another, prior to the possession of consciousness of what it recognizes (pp. 92, 135, 162). And he does not bring out the fact that Hegel overcomes the split between nature and history by virtue of his extremely difficult doctrine of Absolute Spirit as Absolute Process.

As much as I admire the brilliance of Kojève, it has to be said that his interpretation is already a vulgarization of the *Phenomenology*. Kojève and O'Brien emphasize Hegel's doctrine of individuality, but leave out the key element which renders this doctrine intelligible (if that is the right term): the Absolute. In O'Brien's case, this omission is no doubt due to his desire to make Hegel acceptable to analytical philosophy. But I think he takes the wrong tack. Perhaps the worst aspect of analytical philosophy is precisely its approach to such human phenomena as history. Kojève did not take seriously those who believe that the model for historical explanation "is the account one might offer for why an automobile radiator cracked on a certain cold night" (p. 38). We are clearly in for a spate of books attempting to assimilate Hegel to the language of contemporary English and American philosophy. This will be accomplished only at the price of trivializing Hegel, as has already happened with Nietzsche. And this result will benefit no one, Hegelians or analysts. Progress in philosophy depends upon facing up to, and comprehending, the most difficult formulation of the most profound teachings. Much as I appreciate O'Brien's book, I cannot bring myself to believe that it will contribute to this goal. On the other hand, it contains valuable pages, and deserves to be read for these alone.

STANLEY ROSEN

Pennsylvania State University

The Measurement of Children's Civic Attitudes in Different Nations. By A. N. Oppenheim and Judith Torney. (New York: A Halsted Press Book, John Wiley & Sons, 1974. Pp. 84. $5.95.)

The International Association for the Evaluation of Educational Achievement resolved in the mid-1960s to evaluate the success of civic education in a variety of different nations. This ultimately resulted in the collection of questionnaire data in 1971 from some 35,000 children, aged 10–18, attending school in ten different countries.

This particular monograph is the first from the project and describes unusually carefully the instrument development phase, through item selection and pilot studies (at least development of the attitude, though not the cognitive or perceptual, measures). This seems well worth doing; obviously the forthcoming report of findings will be of great interest to students of political socialization, if for no other reason than because of the unprecedented size and comparability of the cross-national samples.

The basic attitude dimensions measured here are familiar ones in the political socialization literature: citizenship values (participation, efficacy, patriotism, etc.), democratic values (racial and religous tolerance, egalitarianism, civil liberties, etc.), and to a much more limited degree, perception of the political process and of government. A variety of different kinds of measures were tried, including projective tests, open-ended questions, and semantic differentials as well as the more standard fixed-response questionnaire items. In the final testing, however, the projective tests and the open-ended questions fell victim to economy.

Two aspects of the measurement are particularly innovative. Factor analyses are reported within each nation, and are used as evidence of the comparability of items across languages and political systems; these also give a rare look at the coherence of children's political attitudes. Also, the initial pilot testing used projective and open-ended questions to try to detect cognitive styles of response, along abstract-concrete, personalizing, egocentric or sociocentric dimensions. Children's free responses were then used to develop a series of fixed-response items to measure these dimensions. The results of this effort will no doubt be of great interest in the final report.

DAVID O. SEARS

University of California, Los Angeles

From Hope to Liberation: Towards a New Marxist-Christian Dialogue. Edited by Nicholas Piediscalzi and Robert G. Thobaben. (Philadelphia: Fortress Press, 1974. Pp. xiv, 114. $3.25.)

Marxism and Christianity are powerful historical movements that have had a mighty influence throughout the world. Both are movements of total orientation in the sense that they constitute for their members a *Weltanschauung,* a way of thinking about reality, and a *Praxis,* a way of transforming reality. Each movement, however, is split into diverse subgroups, which means that the character of its presence in the world varies radically in time and place. Consequently since the emergence of Marxism as a dominant historical force, these two movements of total orientation have been, at different times and places, suspicious of each other, engaged in struggle against each other, but also attracted to and even allied with each other.

The slim collection of essays under review presents two forms of alliance between the movements, but forms that are sharply different – perhaps irreconcilably so – because of their different locations. In the early 1960s, some Christians and Marxists in Western and Eastern Europe entered into "dialogue" with each other. The dialogue, focusing dominantly on matters of theory, has continued throughout subsequent years in conferences and publications with varying degrees of vitality. During the same period, however, other Christians and Marxists in the Third World, especially Latin America, joined forces in a common and direct revolutionary struggle against privileged classes and reactionary governments. This volume focuses on the tensions between these two forms of alliance – dialogue and revolutionary action.

All of the contributors are Americans, i.e., United States citizens; most of them are Christians; all of them are sympathetic to the Marxist cause; yet in their precise loyalties and particular interests they disagree.

Robert G. Thobaben introduces the issues at stake with a schematic history of the two forms of alliance. His summaries are compact and elementary, yet suggestive. He clearly favors the more humanist-activist orientation he finds in Third World Marxism.

Virtually half the book is devoted to a Marxist-Christian dialogue held at Wright State University between Herbert Aptheker, director of the American Institute for Marxist Studies, and Thomas W. Ogletree, a theological ethicist. The book contains their initial statements on "What May Man Really Hope For?" and subsequent responses and discussion. The heart of Aptheker's statement is that Marxists and Christians are both utopian and share basic ethical assumptions. Therefore in the United States, as a practical basis for "hope," they should form a united front supporting a twenty-five point (but strikingly modest!) political platform. Ogletree, accepting a Marxist interpretation of immediate historical struggles and concrete human hopes, would supplement that with a Christian interpretation of ultimate religious hope as a means of sustenance in a world of insecurity and failure and a means of acknowledging the limits of all historical accomplishment.

The most vivid contribution is by Shepherd Bliss, Protestant clergyman, who out of deep commitment to the Latin American revolutionary struggle, is impatient, even angry, at academic confabs among comfortable Marxists and Christians. He would replace dialogue with praxis and talk about hope with strategies for liberation. To Bliss, the religious symbols of Exodus and Resurrection deal not with abstract verities and ultimate hope, but with revolutionary experiences of New Life.

Nicholas Piediscalzi's concluding essay appeals for reconciliation and proposes that even additional traditions of total orientation – Jews, Buddhists, etc. – be solicited for a concerted drive toward universal justice.

While the book suffers from significant deficiencies in historical and theoretical depth, it may prove useful given its explicitly narrow aim to introduce nonspecialists to the tensions between the theoreticians and the activists engaged in Marxist-Christian dialogue. On the other hand, perhaps the nonspecialist particularly needs more of a grounding in Marxist and Christian principles than these essays provide to comprehend the full import of the struggle between these two approaches to Marxist-Christian alliance.

DOUGLAS STURM

Bucknell University

The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition. By J. G. A. Pocock. (Princeton, N.J.: Princeton University Press, 1975. Pp. 602. $22.50, cloth; $11.50, paper.)

The "Machiavellian Moment" may strike us as somewhat drawn out in this long and impressive book, but we are informed right away that "moment" means aspect as well as instant. The title, we are told, was suggested by Q. Skinner of England's Cambridge University. An alternative, unsolicited subtitle might be "From Baron to Bailyn," for Pocock presents Machiavelli as the expounder of a tradition that began with Baron's civic humanism of the fifteenth century and culminated with Bailyn's opposition radicalism of the eighteenth.

Pocock's contribution, however, is nothing less than magisterial, even to these illustrious names. While they worked in compartments, he has connected them; and he has done so with a running start from Aristotle, a welcome study of post-Machiavellian Florentines, and a determined march through the thicket of seventeenth-century England. His acute analysis is usually based on an original examination of the texts, each book considered as a whole. When Pocock relies on other scholars, his purpose is to advance the argument, not to confirm it or to answer objections. This is a long book with a long thesis. The writing is "richly textured," to borrow the author's phrase, with complicated sentences on both hands, nonconditional if's (not if x, then y, but if x, then besides that y) and relentlessly expressive adverbs. There is time for prudent concessions and defensive qualifications – there has to be – but not for polemics.

Civic humanism is the republican virtue of citizens participating in rule. In discovering its importance, Pocock and the scholars whose work he integrates call attention to a modern or premodern body of thought that is quite different from liberalism. Liberalism favors rights over community, liberty over duty, representation over participation, and interest over virtue. Pocock touches on these issues, but with so much to say in exposition, he does not arrange a confrontation between civic humanism and liberalism, or between Machiavelli and Hobbes and Locke. Hobbes, indeed, is drafted into the citizens' army of civic humanism and made to serve with men he would have considered riff-raff. Although Pocock's method achieves an impressive synthesis, it deprives the "Machiavellian moment" of the clarity it would receive from distinction.

The book begins with three chiefly theoretical chapters on "time," in which a general identity of historicism and secularism is asserted. Clearly historicism is secular, since the primacy of history in this world entails the primacy of this world, but is secularism necessarily historicist? It would seem that human reason might offer an acceptable, nonhistoricist basis for secularism; but while Pocock says little about liberalism, he says nothing about modern science. The fact that Machiavelli does not, and Hobbes and Locke do, rely on modern science is worthy of comment.

To make Aristotle a precursor of Machiavelli, some adjustments are necessary to both, particularly in regard to the status of moral virtue. Pocock concentrates on Aristotle's discussion of "polity" in Book IV of the *Politics,* but he does not directly raise the question so important to Aristotle of whether moral virtue is for the sake of the polis (and thus is "republican virtue") or the polis is for the sake of moral virtue (and when it is, should be called "aristocracy"). Correspondingly, with an attitude of determined innocence, he ignores or deprecates the criminality which Machiavelli thought necessary to virtue, including republican virtue. He conducts himself like a man who has heard of brothels but cannot believe he is in one, although the evidence of "nonmoral" doings comes even to his averted eye even while he is sitting in the parlor.

With the aid of his discussion of Aristotle's "polity" (not to mention shorter treatments of Polybius and Fortescue), of Baron's researches on Bruni, and of extended analyses of Cavalcanti, Savonarola, Guicciardini and (a pretty phrase) "the lesser *Ottimati,*" Pocock places Machiavelli in a context (p. 183). He allows that "his mind was liberated" and that in *The Prince* he achieved "a breakthrough into new fields of theoretic relevance" (p. 155). but since the context stated the issues for Machiavelli and limited the extent of his possible discoveries, he is in effect cut down to size with the other cornstalks in the field. Pocock salutes Machiavelli's discovery of the innovative prince, but claims that Machiavelli did not, or could not, solve the problem of making the prince's innovations durable until he discovered republican virtue in the *Discourses*. Apart from the fact that this does not accord with what Machiavelli says at the beginning of *Discourses* III 35, Pocock seems to have too quickly dismissed his own insight that *The Prince* "does not present a single rounded portrait, but a gallery of specimen types of innovator" (p. 180). For perhaps the teacher of princes can offer something valuable to each specimen type of prince, thus round them up, and thereby avoid the instability of relying on one of them.

In considering the *Discourses*, Pocock again fails to press a promising point. Although his topic is Machiavelli's way of maintaining the republic, and although the theme of his book is the relation of politics and time, he does not mention the "perpetual republic" that Machiavelli flashes before our eyes (Machiavelli denies it in III 17, asserts it in III 22, and implies it in I 20, II 5, and III 3). He concludes too soon that Machiavelli did not accomplish what he had set out to do, that he was unable to revive ancient virtue because he could not correct the mistakes of the ancients (p. 216). In the end Pocock gives him credit only for reviving the citizen-soldier (p. 218); and in consequence he develops the "Machiavellian moment" in general and inconsiderately as an improvement on Machiavelli. He is quite aware of the difference between the "premodern" (i.e., pre-Hegelian) view of history as movement away from "moral stability" and the modern view as movement toward it, or rather, toward "incessant qualitative transformations of human life" (p. 551); and clearly he prefers the latter. But he does not allow Machiavelli to state or to explain his preference, and so to give his own, far broader definition of the "Machiavellian moment." Pocock's chapters on Machiavelli occupy only an eighth of the book, but they are the heart of it; and at the heart there is a fundamental unclarity. Most students agree that a great turn in the world's affairs occurred at about the time of Machiavelli, and they agree that it brought a change toward secularism and realism roughly of the kind now popularly associated with his name. But because of the power of modern historicism, there is great reluctance to attribute this change *to* Machiavelli, or even to consider that possibility. Pocock is more learned and more penetrating than most, but he shares this reluctance. For him, Machiavelli is not a prince or a founder but a creature in a context supplied by Pocock's own "model."

There remain eight chapters of this work to be summarized. Pocock continues with Florentine republican thought after Machiavelli, discussing Gucciardini's later writing, the *Dialogo del Reggimento di Firenze*, and works of Giannotti and Contarini in which Venice is presented to Florentines as a "paradigm." Then follow five chapters on Machiavellism in England, where we see it pass from courtiers to republicans as in the course of the Civil War the notion of the "balanced constitution" took over from the "thesis of descending authority." It falls next to the eighteenth-century "country party" opposing the Court Whigs, and here Pocock's treatment of Bolingbroke and of Montesquieu is outstanding. The last chapter is on the appropriation of the "country" ideology by Americans and its abandonment in the Constitution, as argued by Bailyn and Wood. Pocock believes in addition that the messianism which recurs in American history is the work of the Machiavellian moment. At the end of his book he discloses a "conservative" (p. 551) or liberal (p. 552) distaste for a tradition (the Machiavellian!) which rests on moral absolutes. When a man comes to a reasonable conclusion it is sometimes best to leave his reasons in peace.

HARVEY C. MANSFIELD, JR.

Harvard University

Political Science and Area Studies: Rivals or Partners? Edited by Lucian W. Pye. (Bloomington: Indiana University Press, 1975. Pp. 245. $10.95.)

This will not be the last book on the topic, and that is a pity. Political scientists are forever gazing into the intellectual pool to see how many heads are revealed; each gazes with the intention of identifying which of them most nearly conforms to his or her own conception of truth and beauty. Too many heads are considered monstrous; even two heads evidently are impossible to live with. This is particularly so when one head, "area studies," seems clearly defined and the other head ("political science"? "functional or process analysis"?) seems very murky and elusive indeed.

Of the ten gazers included in this symposium, only Harry Eckstein is skeptical of the area focus as a guide to the discipline's self-realization. After making a very strong case that Western Europe is a better geographic area than most for the conduct of comparative analysis, he nevertheless opts for "problem communities" as a much better basis for testing theoretical propositions cast at the macrolevel of the polity. His view seems to be echoed by Leon Lindberg, whose case for Western Europe seems to be that, if one wishes to elucidate problems and processes of that group of countries that are in that vague category denominated "advanced industrial societies," then Europe as an area focus makes a lot of sense. To which Eckstein would reply that European countries are less distinctive than we sometimes think, that some "European" countries (e.g., Australia, Canada, New Zealand, Israel and the United States) are not in Europe at all, and that to speak of Europe as a naturally defined analytical category involves gross oversimplification.

Eckstein is essentially correct, although I am not sanguine about macrolevel generalizations that do not easily guide us to the kinds of empirical investigations that can disconfirm them. I think he is dubious too, but this inference cannot be clearly derived from what he writes.

Some of the other authors express skepticism about theories of political science that are unencumbered by the rich and profound diversity of area-centered phenomena. These skeptical observations, especially those of Myron Weiner, Samuel, Huntington, Kalman Silvert, and Alfred Meyer, deserve most careful attention. Huntington, for example, is typically arresting in a brief essay depicting why the American polity is remarkably unique and why, therefore, concepts, research approaches, and theories that derive from the American experience cannot readily be applied elsewhere, not even when carefully "adapted" to alien lands. Weiner, too, is somewhat rueful in his persuasive assessment of how little mileage we have got out of the dominant general theories of political science when these have been paraded as instruments to help us understand the polities of South Asia. He reminds us that Karl Marx, whose conceptual scheme has been used by some to "explain" everything about every country, was not so simpleminded as to argue that his formulations applied to countries like India and China that had not experienced the preconditions of his historical dialectic. In any event, Weiner's enumeration of how many theoretical propositions don't fit that area of the world is entirely sobering.

One difference between Marx and most of the contemporary general theorists, of course, is that Marx did turn Hegel around and produced a large number of empirical propositions that could be disproved. The difficulty, the frustration, and the agony of more recent so-called general theories is that they often *do* purport to explain everything but wind up explaining nothing at all, or only the trivial. Some of us have said this before but with little apparent effect. The horror of these "theories" is that they are still uncritically adopted and "applied" to countries as diverse as Albania and North Korea, China and the Soviet Union, Chile and Uganda, and you name the country.

The statements by Meyer and Silvert are the most trenchant. Meyer disposes of the underlying question of this volume with the laconic "why, both, of course!" Area knowledge undisciplined by orienting concepts becomes an intellectual Babel. Concepts unleavened by empirical information gleaned from *areas* is likely to remain unverified speculation. Worse than that, we are led to believe we have more useful information than is manifestly the case.

Meyer wants to provoke so that we will reassess. He suggests that, given how little the discipline offers by way of theoretical aids, area specialists with no information about the latter have probably done better work because of this ignorance (p. 101). Furthermore, he argues that a narrow focus on data that can be quantified and hypotheses that can be made "operational" will impoverish and has impoverished the discipline. In line with his recent criticisms of those who write about Communist systems, Meyer stresses that their wholesale incorporation (in their work) of the recent theoretical models of the discipline has been a near disaster. I join him in asserting that these "models" have turned into Procrustean beds, and I add that recent revisionist descriptions designed to compensate for earlier uses of the "totalitarian" model have produced near travesties of political science.

Meyer's attempt to describe the nature and evolution of political science by analogy to the field of art (pp. 117–128) is thought-provoking but overworked. Both fields do indeed deal with the human condition, with reactions to situations of overwhelming power and disorder, anarchy and the fear of death. Both fields, as he says, use the methods of comparison, and these areas are most effective when comparisons drawn are bold and apt. But he is wrong, I believe, when he claims that the artist and the political scientist consider some human conditions "too obscene" to depict; when he suggests that few political scientists are concerned with the aesthetic (e.g., *elegance*) of scientific work; when he describes writers like Dante, Shakespeare, Swift, and Stendahl as "great political scientists"; and when he fails to acknowledge that, in political science, a major impulse to development is the self-same conflict of generations he associates with the evolution of art.

Silvert is no less provocative in his claim that the dominant major conceptual approaches of the discipline are "antithetical to area studies whose prime task is to establish the cultural parameters of social activity . . . [D]ata from foreign fields could not be absorbed by a discipline so fixed on a search for the common that it had no idea-frame for holding the different" (p. 157). In his analysis of works in political science (including one of mine) published in the 1960s, Silvert makes a strong case for his lament that few of the central concepts there used would go very far to help us understand the dynamics of Latin American politics.

Chalmers Johnson's essay is an attempt to show that the countries of East Asia may lend themselves to the kinds of case-study analyses suggested by Arend Lijphart. What Johnson has to say about the utility of this approach has been said before, and more succinctly, by my colleague, James Fesler. Clearly, case studies that help us to confirm or disconfirm theories, that deliberately use general knowledge to help us better to interpret a given case, that may serve to generate new hypotheses are useful. Even more useful, it might be added, is Eckstein's notion (elsewhere elucidated) of the "critical case" – that singular instance where a theory either *must* be verified or the theory's author must return to square one and start over again. But case studies can and should be executed anywhere; nothing about them intuitively argues in favor of a geographically circumscribed area encompassing many *diverse* nations.

The basic underlying argument of this symposium is not "area" versus something else; nor is it, surely, whether research in political science should be "behavioral" or something else. The argument is whether our so-called general theories, our concepts and typologies, our choice of research problems and our styles of research – tied as they have been to very limited experiences in time and space – will serve as well in developing better comparative and, eventually, universal knowledge about politics. The answer is no.

Therefore (thankfully) more work must be done. The nice thing about several of these essays is that they tell us where, how, and why we have erred, and where we might now turn. Taken all together they should convince us once for all that what we are after is a better political science that produces additive knowledge based on biting propositions about the empirical political world, and the institutions and processes that characterize it. In the meantime, let us give the straw man the title of the book suggests the overdue burial he deserves. The intellectual pool is full of other interesting images too. We have only to gaze into it less self-consciously.

JOSEPH LAPALOMBARA

Yale University

Between Existentialism and Marxism. By Jean-Paul Sartre. Translated by John Mathews. (New York: Pantheon Books, 1974. Pp. 302. $10.00.)

Contrary to what the catchy title may promise, this book is an anthology collecting some of Sartre's essays and interviews dating from 1959 to 1970, and having little to do with either existentialism or Marxism. It is meant neither for neophytes unable to follow the tortuous logic of one of today's most profound thinkers, nor for scholars already familiar with these recurrent themes, but for those who fall somewhere in between: the educated consumers who have read or heard about Sartre and who would like to find out more without having to wade through his technical works. Thus, it will probably sell well, but add little. While it may have made sense to publish these essays in this form in France where Sartre is standard conceptual fare, their publication in English without even a summary introduction to contextualize and relate the various pieces will simply intensify the present confusion about contemporary French thought. Only Sartre's usual brilliance keeps this haphazardly assembled anthology from being a total intellectual disaster.

Structured as a kind of introduction to Sartre in 300 pages or less, the book is divided into five sections: Self-description, Politics, Philosophy/Poetry/Painting, Psychoanalysis, and Intellectuals. None of the five comes close to providing even a rudimentary account of Sartre's position. The one on psychoanalysis, for instance, merely reproduces Sartre's short introduction to a taped confrontation between a psychoanalyst and a patient who, tired and outraged by the paucity of tangible results after three years of psychoanalytic therapy, challenges the whole enterprise as a fraud. The other three parts in this section consist of the transcribed confrontation and two dissenting opinions by former editorial associates of *Les Temps Modernes* (where the whole section originally appeared). To anyone even vaguely acquainted with Sartre's views, it is immediately obvious why he would want to publish such a dubious document which, as his co-editors point out, could only have the heuristic function of *initiating* a debate concerning problematic aspects of psychoanalysis. Although Sartre mentions "the irruption of the *subject*" (p.

200) and the violent character of the doctor-patient relationship, no one not already familiar with the sado-masochistic dialectic of *Being and Nothingness* could possibly recognize this as anything but an amusing but trivial piece. And those familiar with Sartre's earlier works will be at a loss to find anything new in all this.

The same could be said of the section on Politics. It closes with a puzzling interview concerning organization where Sartre paints himself in a corner while being lectured by the interviewer. Conducted by Rossana Rossanda – a member of the *Manifesto* group which had just split from the Italian Communist Party – the interview searches in vain for the theoretical determinations of a new type of political organization that would avoid bureaucratization while at the same time performing the function of traditional Communist Parties. Again, this comes as no surprise to anyone who eight years earlier had read Sartre's elaborate explanation of the logic of group dynamics in his *Critique of Dialectical Reason*. But, on the other hand, no one can even begin to grasp this logic merely by reading the interview (which, contrary to what the book claims, was originally published in the U.S. in the fall of 1969 – a full year before it appeared in England).

The most inappropriate section, however, is the concluding one on "Intellectuals" which opens with a series of three lectures originally delivered in Japan in 1965. Well before the full development of Maoism and ultra-left alternatives to the French Communist Party, Sartre updates the classical Gramscian analysis which, after the XXth Congress, can only end up leaving intellectuals in a state of "unhappy consciousness" as the only viable political posture. This essay could not have been more foreign if it had been left in the original French. Not only is it historically obsolete within the French context – as Sartre himself acknowledges in the short introductory paragraph written for the French republication – but, given the rather different predicament of American intellectuals, it is also completely irrelevant in this country.

The presence of his by now classic essay on "Czechoslovakia: the Socialism that Came in from the Cold" ultimately saves the book. Here we have Sartre at his best. The philosopher, the writer, and the politician converge to produce probably the best essay written on the Czech situation after the Russian invasion. Originally written as an introduction to a volume of interviews with Czechoslovak intellectuals, this essay painfully traces the tragedy of a "socialist" degeneration which has profoundly affected three generations of intellectuals caught within an "imposed" socialism as oppressive as any fascism in recent times. His analysis forces a profound rethinking of all accepted notions and schemes in order "to produce new structures which will ensure that the next revolution does not give birth to *that sort of* socialism" (p. 117). It is regrettable that such a brilliant essay had to be buried within so much unrelated and irrelevant material which will confuse more than enlighten its intended audience.

PAUL PICCONE

Washington University

The Politics of Rights: Lawyers, Public Policy, and Political Change. By Stuart A. Scheingold. (New Haven, Conn.: Yale University Press, 1974. Pp. xiv, 224. $10.00.)

Scheingold's book analyzes the usefulness of judicial decisions as a tool for achieving social change. By social change, Professor Scheingold means the redistribution of power. While he approaches the topic from the bias that law is valuable to the extent that it facilitates redistribution, the book is not a polemic. The author's analysis draws on the findings of others, augmented by hiw own insights, rather than being the product of new fieldwork.

The most useful insight of the analysis is the distinction made between what Scheingold calls the myth of rights and the politics of rights. He applies the former to the notion that once a court declares the existence of a right – for example, school desegregation – then realization of the right will naturally result. The compliance literature is used to show the flaws in this type of expectation. Scheingold goes on to criticize the heavy reliance on litigation, which the myth of rights emphasizes as the technique for producing change, on the grounds that it is costly, slow, and at times counterproductive.

Having rejected the utility of the myth of rights, the author suggests that court victories, rather than signaling the attainment of a right, can better be viewed as catalysts for political mobilization. Thus judicial recognition of a new right will alter how some people view a situation. Some "have-nots" may be emboldened by the decision to try to use the political process to translate the terms of the court order into reality. With the law on their side, a new self-confidence and sense of purpose may promote group activity either in the guise of mass protests or of lobbying efforts. "Insofar as court decisions can legitimate claims and cue expectations, litigation can contribute to both activation and organization; to the building of new coalitions; and in the long run, to a realignment of forces within the political arena" (p. 132). Judicial recognition of a new right may also prompt some of the "haves" and their allies in policy-making positions to re-evaluate programs.

Having indicated how court decisions can trigger the development of political pressures, Scheingold evaluates the likelihood that law will serve this function. He does this by exploring the socialization process of lawyers (in law school) and the environment in which they practice. Scheingold correctly notes that the legal education process concentrates on imparting analytical skills and ignores or sweeps aside questions about the social consequences of decisions. The rationale that an attorney's responsibility is to represent the interests of his client as best he can and to ignore the public good is basic to the legal profession. The rule of precedent emphasized by the casebook method further conditions lawyers to accept the status quo.

Despite the features of legal education which socialize students *not* to use their talents for reform, there are some whom Scheingold refers to as "activist lawyers." These range from lawyers who adopt a fairly conventional approach, using litigation on behalf of society's disadvantaged, to the "innovators" who "construct an integrated campaign comprised of legal, electoral, and other political strategies. . ." (p. 183). Thus the innovative activists, like Ralph Nader, are ones who rather than putting their faith in the myth of rights use the law to mobilize public opinion or to orchestrate group pressures.

Scheingold's analysis leads him to a basically negative conclusion. He sees little likelihood that even as a stimulus for mobilization court-made law can produce social change, although it is useful in setting policy objectives. Unanswered is whether other avenues of lawmaking such as legislation or rule making in the executive branch hold out greater hope. The implication is that they do, but the success of litigation relative to other techniques receives little attention. Nor does the author fully consider the proposition that courts may serve a special policy function to the extent that they may legitimize demands which have little popular support and which therefore would be unattainable in other arenas.

The Politics of Rights is marred in places by organizational problems. Although the author carefully sets forth the topics to be covered in each chapter, he sometimes digresses and pursues tangential matters. Another difficulty is the failure to define some of the terms used. Nonetheless the book is useful in prompting a rethinking of the role which law does and can play in policy change.

CHARLES S. BULLOCK, III

University of Houston

Where Have All the Robots Gone: Worker Dissatisfaction in the '70s. By Harold L. Sheppard and Neal Q. Herrick. (New York: The Free Press, 1972. Pp. 222. $7.95.)

During the late 1960s and early 1970s, such disparate groups as middle-class college students, black ghetto residents and white union workers were venting acute dissatisfaction with established leaders, policies, or institutions. This book, written by two labor economists, deals with one of these groups, the white blue-collar worker, whose alleged "blues" became a favorite theme for media journalists, governmental task groups, and sociological researchers.

The point of departure of this book, as in much of the worker-alienation literature, is the premise that "more and more workers . . . are becoming disenchanted with the boring, repetitive tasks set by a merciless assembly line or by bureaucracy" (p. xi). This dissatisfaction, the authors contend, is particularly characteristic of the younger generation of blue-collar workers, who are less willing to accept the authoritarian disciplines of the work place and who vent their discontent not only in the work arena but in their voting behavior and other ways.

The major portion of the book reports the results of interview surveys of about 400 white male blue-collar union workers, conducted in a few communities in Pennsylvania and Michigan in 1970 and 1971. About 100 of these workers were identified as having the "blues," based on responses to questions concerning their job satisfaction and perceived advancement opportunities. Most of the resulting analysis consists of simple, bivariate comparisons between these disaffected workers and the remainder of the sample. The workers with the blues were found to be older than other workers; they rated their job tasks as more boring and were generally more alienated from society than the rest of the sample. Among this group, the younger workers appeared more dissatisfied with the nature of their job, and with lack of advancement opportunities, while older workers (over 30) were most unhappy about their pay. Of special interest to the political scientist is the fact that the younger discontented white blue-collar workers in this sample were also more dissatisfied with the existing political establishment and voted disproportionately for Governor Wallace in the 1968 presidential election.

This book is written in simple nontechnical style and contains some provocative insights. From a scholarly standpoint, its results are, however, a very slender peg on which to hang the generalizations and inferences made through this book. No information is provided the reader as to how the workers included in this survey were selected. Statistical tests of significance are rarely cited, and no attempt is made at any systematic use of available techniques of multivariate analysis.

More fundamentally, the authors' conclusions that worker alienation is increasing and that "restructuring of work" is the primary solution to the blue-collar blues appear inconsistent with the results of more systematic national surveys which indicate: (1) that there has been no consistent national trend in worker dissatisfaction during the past fifteen years, and (2) that blue-collar workers, unlike white-collar workers, are most interested in the financial rewards of their jobs rather than job challenge. (See, particularly, Robert P. Quinn, Graham L. Staines, and Margaret R. McCullough, *Job Satisfaction: Is There a Trend* [U.S. Department of Labor, Manpower Research Monograph No. 30, 1973].)

HAROLD WOOL

National Planning Association, Washington, D.C.

Games for Society, Business, and War: Towards a Theory of Gaming. By Martin Shubik. (New York: Elsevier Scientific Publishing Co., 1975. Pp. 371. $16.50.)

This book is a delight; its reading yields insight and much pleasure. Shubik surveys gaming, from its use as entertainment through its employment in social science research. Shubik clarifies distinctions between *simulation* which "involves the representation of a system or organism by another system or organism which purports to have a relevant behavioral similarity to the original system" (p. 12); *gaming,* which "in contrast to simulation, necessarily employs human beings in some role, actual or simulated in its operation" (p. 13); and game theory, a mathematical "language for the description of conscious decision-making processes involving more than one individual" (p. 14). Shubik lays foundations for the development of a theory of gaming, in which analytic (especially those from game-theoretic enterprises) and behavioral (including those from psychology, sociology, economics, and politics) models may be combined for purposes of teaching-and-training, operational gaming, and experimentation. An elegant, elementary exhibition of the analytics is presented in Part II of the volume, "A Game Theory Background for Gaming" (pp. 27–153), with "exercises" at the end of each of the five chapters therein.

For those political scientists interested in placing the earlier descriptions of simulations in political science given by Wayne Francis and Paul Smoker in Guetzkow, Kotler, and Schultz (eds.), *Simulation in Social and Administrative Science*(Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1972 [pp. 245–363]) in a gaming context, this book provides an enlargement of horizons, especially in two of its four chapters on *Some Specific Games and Their Uses,* namely Chapter 13 on "Military Models, Simulations, and Games" (pp. 279–294) and Chapter 14 on "Political Science, Urban Development and Other Uses" (pp. 295–308).

As one reads the book, one almost hears Martin Shubik talking – he's the very embodiment of an Oxford don. His thought flows fluently. He's given to intriguing challenge, as when he argues: "Perhaps . . . in any application of gaming from kindergarten teaching to major war games success will depend upon the amount learned by the teachers and designers as well as the students and players" (p. 24), and as he later warns: "Problems, such as the meaning and measurement of national interests or the depth of ideology, are well worth studying using a gaming context, just so long as the teacher or experimenter does not confuse his limited tools and special models with the many other levels of reality and creative fiction" (p. 301).

Over the years Shubik has made substantial contributions to the theory of games, as his forthcoming volume with his long-time colleague, L. S. Shapley – *Competition, Welfare, and the Theory of Games* – will indicate. Most of us also know their essay, "A Method for Evaluating the Distribution of Power in a Committee System," which appeared in the 1954 volume of this journal (*APSR,* 49 [1954], 787–792). Shubik's edited volume on *Game Theory and Related Approaches to Social Behavior* (New York: Wiley, 1964) was a worthy precursor of the exciting book in hand. Although Shubik is an economist, he's certainly as broad in his interests and competencies in the social sciences as Kenneth Boulding, even though he may be less well known to political scientists.

Shubik is not only an interdisciplinarian, he is a cosmopolitan, as his researches in an amazing number of places in the world testify. He has conducted experiments with colleagues of other cultures on at least five of the six continents. He is part of the creative complex at Yale which provides the intellectual foundations from which Yale's new graduate School of Organization and Management is being built. The breadth and scope of Shubik's performance in the book matches his outstanding credentials for having been its author – and enables one to forgive quite easily occasional shades of oracularity, as he shares wisdom with his reader.

Be forewarned: Shubik's subtitle, "Towards a Theory of Gaming" is accurate. As he reiterates at the end of his book: "The different disciplines of the behavioral sciences, together with game theory and institutional studies, are all required in the modeling activities of game construction. In particular, the game models of the theory of games provide the rationalistic benchmarks against which the structure of any game can be compared" (p. 331). Shubik struggles toward theoretical formulations, especially in Part III (pp. 155–199); in my judgment, however, he is far from achieving a theory of gaming. Those concerned with the interrelations of game theory and gaming will find his passage on "A Contrasting of Game Theory Assumptions and Behavioral Assumptions" and his subsequent query, "What is a Solution in Gaming?" (pp. 157–170) seminal. Many of us applauded when Bruce Russett, Marguerite Kramer, and Anatol Rapoport of the *Journal of Conflict Resolution* decided they would "no longer devote a specially identified section [of their journal] to 'Gaming' " (*Journal of Conflict Resolution*, 17 [September, 1973], 379). Perhaps a more intimate development of game theory with behavioral substance will accelerate our ability to move more rapidly "towards a theory of gaming."

It is not surprising, given the sweep of Shubik's interests, that not all references are cited or that on occasion the author has failed to update himself. But it was disappointing that the provocative work of Duke, Kotler, and Naylor was not considered worthy of more than mere mentions. Shubik produces a few excellent summaries, as in Table 18 on "Games with More than Three Players" (pp. 270–271) and as in the final section of Chapter 11, on "Some Specific Games and their Uses: Experimental Social Psychology and Game Theory" (pp. 276–278). One regrets that Shubik did not prepare similar condensations for the other parts of his complex book, so that all could see the forests for the trees, even if the book seems not intended as a text *per se*. Shubik inspires the reader by virtue of his visions for the "Yale Gaming System" (pp. 236–242), involving a computerized system for gaming experimentation. It is sad he didn't create a blueprint with some detail for collaboration to meet the needs for "Joint Usage Among Different Disciplines ... in Teaching and Research ... Among Different Institutions ... via Common Programs and Time Sharing" for work in simulations and operational gaming (pp. 312–318).

Some of us feel bewildered by these new developments, often confusing gaming with the theory of games and are having difficulty in understanding the differences many draw between gaming and simulation. Here's the chance to learn and enjoy!

HAROLD GUETZKOW

Northwestern University

Life and Liberty: A Return to First Principles. By Frederick Martin Stern. (New York: Thomas Y. Crowell, 1975. Pp. 212. $7.95.)

Because the author of this volume is apparently not an academic and perhaps not known to many readers of this journal, it is in order to say a little about him. Frederick Martin Stern was educated in German universities, is a doctor of law, was an executive in industry in pre-Hitler Germany, and is now an American citizen. He has written three other books, none of which I've read: *Capitalism in America: A Classless Society* (1951), *The Citizen Army: Key to Defence in the Atomic Age* (1957), and *The Junker Menace* (1945).

In this book, as its title indicates, Dr. Stern seeks to show that there is a natural, vital connection between democracy, the social system based on "liberty with equality," and the life process itself. We are told that the principle on which the organic world operates from the cellular level upward, competitive yet cooperative diversification, offers the model on which human society in all aspects should fashion itself.

On this theme the book's deficiencies are serious. Treatment of the biological/social thread is superficial and dogmatic. Sustained argument and illuminating analysis is lacking. Sizable leaps are made from basic yet sketchy biological conceptions to alleged implications for human social life. The claim is asserted and reasserted without apparent appreciation of its many genuine problems.

The proposition that the organic side of the universe *in some sense* entails cooperative, deliberative democratic forms may be true, although not so long ago the same grounds were claimed to lead to raw social Darwinism. Nonetheless, it may be that something basic to our nature as social organisms *is* best served by a democratic system. But little said here renders that complex proposition or the logic of its consequences clear, much less convincing. Much more would have to be presented in order to connect *that* level of the organic world coherently to human society, democratic or nondemocratic.

The complementary thesis of the book is a defense of the U.S. version of liberal pluralism which, Dr. Stern tries to argue, can with reasonable effort be turned more in the direction of a flexibly organic community. To the standard features of liberal pluralism (countervailing, separated power at all levels, individualism, civil liberties, equal opportunity, and regulated competition), Dr. Stern adds those of considerable central political control and features of the welfare state. Thus "liberty with equality."

That concoction forms an unstable but familiar mix. With fair accuracy it does describe the U.S. and, barring the gloomier possibilities, denotes the probable terms and measures on and with which the U.S. will have to hammer out at least its near future. The author's treatment of a broad range of U.S. practices and problems is generally sound. Most of his suggestions for improvement are sensible, straightforward, and welcome for themselves. They are capable of implementation with the nation's currently available spiritual and material resources.

But it all has very little to do with an organic community, for what Dr. Stern defends, even after we include his improvements, remains essentially plural, competitive, unstable, secular, and dynamic. Those qualities simply are not the central features of, or even compatible with the central features of, any description or instance of an organic community with which I am familiar. Let me be clear. What Dr. Stern sees in contemporary U.S. society is there to be seen. It has genuine virtues, attractions, and achievements. Thus what he defends and the manner of defense has its own considerable merit. But it is quite another thing to claim that mainly by extending the principles of liberty and equality within U.S. industrial, plural society we are offered even the beginnings of an organic community. Indeed, if Plato, Burke, and F. H. Bradley are cogent defenders of the organic community, then we would have to take a direction counter to that proposed by Dr. Stern, to restrict rather than to extend liberty and equality. Much more than liberty and equality are necessary to an organic society, for such a society depends on deeply unifying bonds and the shaping power of stable, embracing institutions. Role and station, hierarchy, a deeply binding sense of membership in at least one's "little platoon," habit, custom, ritual, tradition reverenced for itself – all these are standard features in the organic model, to say nothing of the anti-individualistic conception of the human soul which is integral to the organic view. Yet all those features are, to say the least, paradoxical in a pluralist nation, committed to equality and individualism, still wedded to progress, "the possible," and to rapid social change at every level. We probably can do and obviously *should* do better with what we have and what we are. Dr. Stern's recommendations on these matters are to the point. But if he or we are serious about a more organic community, then the level of analysis and understanding must be much deeper.

It should be noted that this book could profitably serve as a text in some college courses, because it has a thesis, and its ambitions are large and unfulfilled. Thus it would provide a springboard for a more incisive and illuminating analysis of the central problems it raises. It has other virtues also. It moves at a crisp pace over a broad range of social, economic, and political concerns. Its recommendations are meliorative. Its tone is modest and the author's good will is evident everywhere. It is addressed to an important audience: the mature, reasonably well-informed, reasonably open-minded citizens who take thought for the institutions and practices of the United States, and who care about its future. That audience deserves to have its mind taken seriously. Dr. Stern does so, expressing quiet confidence, even good cheer, concerning the nation and how, short of revolution, it can be improved. Those are not minor virtues these days, when we have heard so much that is shrill, impatient and pretentious, cynical or despairing. Despite its serious deficiencies and limitations, then, this is a welcome and useful book.

R. J. ROWAN

University of British Columbia

Handbook of Leadership: A Survey of Theory and Research. By Ralph M. Stogdill. (New York: The Free Press, 1974. Pp. 613. $19.95.)

Since the start of the twentieth century, the subject of leadership has occupied the attention of many social psychologists. To prepare the *Handbook of Leadership,* Ralph Stogdill, one of the major figures in this research tradition, reviewed more than 5,000 empirical and theoretical contributions. His product is awesome: a total of eight major sections and forty-one chapters summarize the results of the effort. For the person considering whether to read this book, the section headings should be of interest. They are:

(1) Leadership Theory
(2) Leader Personality and Behavior
(3) Leadership Stability and Change
(4) Emergence of the Leadership Role
(5) Leadership and Social Power
(6) Leader Follower Interactions
(7) Leadership and Group Performance
(8) Conclusions

The essential product of this work is a series of inductively derived empirical generalizations. The more a given relationship among variables was studied and replicated, the more confidence Stogdill shows in the findings. I have found no book or article that even closely approximates the comprehensiveness of this work. Indeed, anyone who wanted to determine what social psychologists had contributed to knowledge about leadership would be foolish to ignore this book.

Despite the fact that this is a "handbook," it is written to be read – even from cover to cover. The writer clearly made an effort to help the reader by preparing short chapters, structuring readable sentences, and offering frequent summaries. Because it is so well organized the book should also be extremely valuable for anyone who wished to carry out his/her own literature search in the area of leadership up to 1970.

Any effort to comprehend and order an ordinarily large body of research is to some extent an exercise in projection. No individual can retain a purely inductive stance toward the literature, even if he or she so desires. Sooner or later personal biases influence the way the apparently objective results are reported. Stogdill is no exception.

One bias that Stogdill shows is toward rejection of data and theory about the complex emotional dynamics of leadership. He explicitly omits a discussion of the charismatic leader because "this important variant of the leadership role has not been a willing or frequent subject of research that involves measurement or experimentation" (p. viii). He indicates that the numerous biographical studies of charismatic leaders add little to the understanding of leadership.

Professor Stogdill's approach to topics which have had a large number of studies is to tabulate and count the number of times a given result has been obtained. He does this several times throughout the handbook, the first one pertaining to the identification of leadership traits. Covering all empirical studies from 1904–1970, he finds the two traits most frequently associated with leadership to be sociability-interpersonal skills (49 positive findings, no negative results) and intelligence (48 positive findings, 10 negative results). Summarizing the results of the research on leadership traits, Stogdill does *not* conclude that interpersonal competence is the trait most often associated with leadership, although his own literature search method should lead him to this conclusion. His review of the literature on leadership training shows a similar bias.

To the extent that Stogdill reaches a single conclusion, leadership behavior seems to him to consist primarily in "structuring expectations in the single pattern that contributes positively to group productivity, cohesiveness and satisfaction. This pattern of behavior is perhaps the central factor in leadership since it is intimately associated with the definition of leadership . . . philosophies of leadership and training methods that undermine this factor destroy the very foundation of leadership" (p. 419).

In the growing rapprochement between political science and social psychology, the subject of leadership is definitely an area of common interest. The political scientist who wants a well-written summary, with interpretations and conclusions, of what social psychologists have done with this area until 1970 will find none better than Stogdill's *Handbook*. It has much to offer students of public administration whose concerns pertain to leadership within relatively bounded human organizations. It will be less attractive to those who have attempted to analyze leadership relations by more idiographic methods or who have been most concerned with the political behavior of leaders who have regular face-to-face interaction with a very small proportion of the individuals whose lives they influence.

CLAYTON P. ALDERFER

Yale University

The Formation of the New Left: The Early Years. By George R. Vickers. (Lexington, Mass.: Lexington Books [D.C. Heath], 1975. Pp. 166. $14.00.)

This lucid work is important, not because it narrates the early history of the American New Left (something which has already been done), but because it offers a mature social science explanation of the New Left's emergence and contributes to concept formation in empirical theory. George Vickers, a staff sociologist for the Russell Sage Foundation, avoids feckless speculation, rests his argument on public evidence (including archival documents and empirical findings), owns up to speculation where evidence is inconclusive, and recognizes that his study is but a first step toward understanding the New Left.

The author was, himself, "deeply involved" in the New Left – including Students for a Democratic Society – and the book was originally intended as a defense of the New Left which would show how the actions of the activists were "a rational response to 'reality' as they perceived it, rather than a response to some deep, unconscious psychological urges" (pp. ix–x). Vickers criticizes the existing psychological accounts of the New Left centering around the theme of "generational conflict" – in particular, the generation gap views of Lewis S. Feuer, and the nongeneration gap views of Kenneth Keniston and Richard Flacks (which stress intraclass continuity between parents and children). Although Vickers ultimately constructs a neo-Marxist, sociological explanation, I find his views to be quite compatible with those of Keniston and Flacks to the extent that they all emphasize the vital importance of the (upper-) middle-class family socialization experiences of the early activists. Even though he ends up with less than the apology he set out to write, Vickers avoids psychosociological reductionism by employing a neo-Marxist "dialectical" or "relational" approach which joins the "self-active" creation or *praxis* of the actors to the conditions that structure their action.

Focusing on class culture, Vickers emphasizes the post-World War II differential in child-rearing practices separating middle-class parents (especially those relatively well-educated and holding bureaucratic jobs) from working-class parents. Because these middle-class parents worked not with things, but with data or people in relatively unsupervised fashion, they experienced "self-direction," hence valued it, and then taught it to their children, whereas working-class parents experienced, valued, and taught compliance with external authority. Vickers then analyzes the changing class configuration of postwar America in terms of the increasing role of knowledge in the functioning and growth of the economy – especially in government and corporate bureaucracies. "Not only did 'white-collar' and 'service' occupations grow relative to 'blue-collar' and 'farm' occupations, but . . . in white-collar occupations the proportion of 'managers, officials, and proprietors and sales-workers declined, while the proportion of clerical workers and 'professional, technical, and kindred' workers sharply increased" (p. 121). Generally the dynamic sector of the middle-class practicing "permissive" child-rearing spawned the New Left activists – who, in turn, did the bidding of their parents, who were interested in promoting new cultural standards congenial to themselves.

The growth in the role of knowledge required the growth, professionalization, and bureaucratization of universities, and their increasing interpenetration with government and business. But just as future university students (coming, as usual, from middle-class homes) were learning from their parents how, so to speak, to "do their own thing," the universities were becoming highly structured and competitive training grounds for the occupational elite. But many students were psychologically unprepared for survival in those universities – which required conformity to authority and habituation to deferment of gratification. With a sharp neo-Marxist eye for contradictions, Vickers states: "The contradiction between the values and relations within the family, and those within the university, led to discontent among the children from the most advanced layers of the bureaucratic, white-collar strata, which took the form of political and cultural opposition based on self-directed values" (p. 124).

Vickers concludes that the New Left was not really an anticapitalist movement – revolutionary rhetoric notwithstanding:

> What the New Left represented, objectively, in this historical process was an attempt to *rationalize* institutional relationships and cultural organization, to bring them in line with the changes in the social relations of production. The attempt to develop a political theory and practice based on . . . "self-directed" values . . . was aimed, in this sense, at institutionalizing those values, interests, and beliefs fostered by the working conditions of educated white-collar workers (pp. 133–134; italics in original).

After some fifteen years of New Left activism, the institution of private property remains intact. We have not achieved socialism, but rather the legitimization of marijuana, pornography, abortion, homosexuality, unisex, and nonmonogamous life-styles. The New Left enabled the new managerial bourgeoisie (sometimes called the "New Class") to make a cultural revolution against the old entrepreneurial bourgeoisie, thus giving it a cultural status to match its economic standing. The political New Left may have evaporated, but the counter-culture has successfully permeated American culture. As a result, American capitalism has *advanced* by resolving one of its more recent internal contradictions.

DALE VREE

Stanford University

Social Indicators and Societal Monitoring: An Annotated Bibliography. By Leslie D. Wilcox, Ralph M. Brooks, George M. Beal, and Gerald E. Klongan. (San Francisco: Jossey-Bass Inc., 1972. Pp. 464. $12.50.)

The United States Department of Health, Education, and Welfare in its 1969 publication, *Toward a Social Report,* defined a social indicator as "a statistic of direct normative interest which facilitates concise, comprehensive and balanced judgments about the condition of major aspects of society. . . ." A concern with the development of such systematic data has been the guiding force behind the development of the contemporary social indicator movement. In the words of the authors of this volume, this movement is concerned with "generating social indicators to serve as tools to monitor social change. . ." (p. 1).

The contemporary social indicator movement arose in the late 1950s in response to concerns by both policy analysts and policy makers that insufficient systematic social data were available to provide adequate intelligence for rational policy and program development and planning. The literature which has evolved concerning various aspects of social indicators — definitions, development, theory, application, policy and planning, and concepts — has been substantial. It has been interdisciplinary in scope and has appeared in a wide range of journals and books. Given this dispersion of scholarly materials, an annotated bibliography such as *Social Indicators and Societal Monitoring* serves a very useful function.

The bibliographic compilation in this volume has been indexed in four separate categories which facilitate finding material: the Annotated Index, the Author Index, the Key-Word Subject Index, and the Address Index. In addition to these indexes the book contains an introductory essay by Ralph Brooks entitled "Social Planning and Societal Planning" in which he has outlined the stages in the development of the social indicator movement, current issues, and current activities of scholars of the movement. He has brought together in his presentation many of the most central concerns of the individuals involved in the development and the use of social indicators. The bibliographic listings also provide interested individuals a good place to begin any research on the use or development of social indicators.

MARIAN LIEF PALLEY

University of Delaware

From Radicalism to Socialism: Men and Ideas in the Formation of Fabian Socialist Doctrines, 1881–1889. By Willard Wolfe. (New Haven, Conn.: Yale University Press, 1975. Pp. 333. $17.50.)

Wolfe's book traces the intellectual origins of Fabian socialism up to the moment when that doctrine took definite shape in the *Fabian Essays* of 1889. He is particularly concerned with the relationship between Fabianism and the radical wing of English liberalism, seeking to show that Fabianism (and indeed English socialism in general) continued to bear a strong radical imprint. The book is scholarly without being dull, and Wolfe is to be complimented on the lucidity and control with which he presents the ideas of his protagonists.

Part I presents the Radical background to Fabian theory, with particular emphasis on J. S. Mill and the land reform movement. The discussion of Mill's socialist sympathies is careful and balanced, though Wolfe somewhat overrates Mill's direct influence on the Fabians by not fully appreciating the contrast between his version of socialism and theirs. Wolfe is on firmer ground when he traces indirect influences via the positivist movement, and via the land reformers, with whom Mill was associated in his last years. The land reform movement was quite crucial in the transition from radicalism to socialism, since the argument that landowners were exploiting the rest of society from their position of monopoly was quintessentially radical, and yet it could be extended by the Fabians to apply to capitalists as well.

Part II contains intellectual biographies of several leading Fabians, and attempts to fill in the intermediate stages which led them from orthodox radicalism to socialism. Shaw went through a phase of extreme libertarianism in the mid-1880s before entering the Fabian fold. With Sidney Webb the path lay through Comtean positivism; with others it was spiritualism or Christian socialism. The variety of the beliefs which eventually converged in the sober doctrines of Fabianism is striking, and Wolfe's careful and well-documented analysis of this intellectual potpourri could scarcely be bettered.

The biographies are used to support some general theses about Fabian socialism. Noting that the origins of Fabianism lay in religion or in substitute religions like positivism, Wolfe argues that the movement itself was a "quasi-religious phenomenon." The Fabians, he maintains, were basically Evangelicals who interpreted their moral duties in social rather than individual terms, and whose deepest objection to capitalism was that it encouraged the selfishness and immorality of capitalist and worker alike. Finally the Society's economic collectivism was a relatively late addition to its philosophy, which never succeeded in supplanting the central ethical and religious elements.

This produces a picture of the Fabians which contrasts sharply with the more usual image of a group dedicated to social planning based on the exhaustive compilation of empirical facts. But how far is Wolfe's picture simply a result of his selection of material? His studies are confined to the period before the group had established its collective identity, when the individual members were more inclined to indulge their private enthusiasms. The discipline of the Society produced some major changes in attitude. If Shaw was a near-anarchist in 1885, he was anything but an anarchist by the 1890s; and Webb's positivist evangelism gave way to the sober historical research which produced those monumental studies of trade unionism, cooperation, etc. that came to typify the Fabian approach to social and political questions. One has to ask how relevant Wolfe's study is to the understanding of the mature Fabianism which exerted a real influence on the labor movement in Britain.

The book's focus of interest also leads Wolfe to neglect the authoritarian and antiliberal aspects of Fabianism. "The perfect and fitting development of each individual," wrote Webb in his *Fabian Essay*, "is not necessarily the utmost and highest cultivation of his own personality, but the filling, in the best possible way, of his humble function in the great social machine." By 1889 the Fabians had come a long way from the liberalism of Mill.

Fabianism cannot be identified with socialism as such. In its emphasis on centralized planning and on the role of the expert, it differed greatly both from Mill's idea of cooperative production within a market framework and from the communism of Marx and the anarchists. It appealed to a small group of middle-class and lower-middle-class professionals, to whom it offered a worldview which accurately reflected their social position. For that reason it has to be separated both from bourgeois liberalism and from the inchoate radicalism of the working class – with which, however, it was later able to form an uneasy alliance. But if Wolfe's book fails to tell the whole truth about Fabianism, it is nonetheless an invaluable guide to the intricate tangle of conflicting beliefs which led to its formation.

DAVID MILLER

University of East Anglia, England

Justices and Presidents: A Political History of Appointments to the Supreme Court. By Henry J. Abraham. (New York: Oxford University Press, 1974. Pp. 310. $10.00.)

Who were the one hundred individuals who have served on the U.S. Supreme Court, and how did they get there? Who were the nominees that did not make it to the bench? What considerations came to play in the presidential selection and senatorial confirmation processes? To what extent did the appointees who served on the Court live up to the expectations of their nominators? These are the questions that Henry Abraham attempts to answer in his "historical political account" that he correctly notes is also "a mini-history of the Court itself" (p. viii). Professor Abraham begins with "prefatory remarks" devoted to the Nixon nominations and then has a chapter that in general spans the different methods of judicial selection and the selection process for Supreme Court justices. This is followed by a chapter on the qualifications or backgrounds of Supreme Court appointees which takes us to the heart of the book, the six chapters that survey all Court nominations beginning with those of George Washington and concluding with Lyndon Johnson's. A brief epilogue completes the book.

To observe that *Justices and Presidents* is an ambitious undertaking is to state the obvious. It would be unfair to expect so relatively short a book to be able to do for each Court appointment what took David Danelski an entire book (and a splendid one at that) for *one* such appointment (Danelski's *A Supreme Court Justice is Appointed* [New York: Random House, 1964]). Rather Professor Abraham presents a broad historical review of the Supreme Court with capsule biographies of the justices, a general audience-oriented version of the politics of their appointments, and brief subjective evaluations of their performance on the bench based in part on the Mersky-Blaustein survey of legal scholars' ratings of the justices. Students should find the book enlightening particularly concerning the less well-known nineteenth- and early twentieth-century Court and justices. Specialists should find Abraham's work highly suggestive, at least implicitly, of hypotheses about the individual Court appointments.

Essentially the book tries to tell us about the nature of judicial recruitment by providing a perspective drawn from the broad sweep of history. I would have preferred, however, a more explicitly theoretical analysis of recruitment. It would also have been helpful had the book addressed itself more systematically and empirically to questions of biases in judicial recruitment with more of a focus on those who did not make it to the nomination stage, the nature and extent of transactions surrounding appointments, and the impact of the individual appointments on the course of Supreme Court policy making (which would require a more precise and objective accounting of judicial behavior than that given in the book).

Taken on its own terms, however, one can note that the more famous and contemporary justices receive more substantial coverage than do the more obscure nineteenth-century justices who are treated in review-book style. For example, the background and politics of appointment of Justice John McKinley is disposed of in little more than a hundred words followed by the observation: "He [McKinley] would not disappoint his nominator [Van Buren] during his fifteen years on the high bench" (p. 95). The only other mention of Justice McKinley occurs seven pages later: "In the summer of 1852 Mr. Justice John McKinley died after fifteen colorless years on the bench – during which time, because of illness, he was as much absent as he was present" (p. 102).

These quotes highlight another distinguishing characteristic of the book, its highly subjective nature. In many ways the book can be considered a personal interpretation of American history and the Supreme Court. For example, in addition to the speculation about how presidents must have viewed their appointees' performance on the bench, we are told without the presentation of supporting evidence that Earl Warren "was an influential participant in President Kennedy's decision to nominate Secretary of Labor Arthur J. Goldberg" (p. 22); that had Nixon nominated Senator Byrd, "there is little doubt that his colleagues would have given their approval" (p. 40); that "Daniels' record on the Court amply justified Van Buren's faith in him: it proved to be as nondeviationist as McLean's was the opposite" (p. 96); that before Justice McKenna went to the Court he was a Ninth Circuit appeals judge where "he served five undistinguished or even incompetent years" (p. 143); that Senate rejection of Judge Parker "is now all but universally regarded not only as regrettable but as a blunder" (p. 189); that when Justice Tom Clark resigned to enable son Ramsey to become Attorney General "the country deeply regretted his self-imposed resignation" (p. 230). (Was I the lone oddball who cheered?)

On balance, I should stress that I enjoyed reading Professor Abraham's work and that even if the book tries to do too much and is perhaps too subjective, it after all does present the considered views of an eminent political scientist and leading scholar of constitutional law. That in itself should commend *Justices and Presidents* to scholars and students.

SHELDON GOLDMAN

University of Massachusetts-Amherst

Political Money: A Strategy for Campaign Financing in America. By David W. Adamany and George E. Agree. (Baltimore and London: The Johns Hopkins University Press, 1975. Pp. 221. $11.95.)

The Twentieth Century Fund sponsored the research project that led David W. Adamany and George E. Agree to their examination of campaign finance in *Political Money*. The project brought together an academic and a political practitioner – the former an academic with considerable "real-life" political experience (Adamany is formerly State Secretary of Revenue in Wisconsin), the latter a fund raiser who has been active in national politics for two decades. While the idea of such a mix is exciting, the results in this book are something less so.

The authors brought to their task a disillusionment with existing methods of financing political campaigns and a belief that some sort of public finance was a necessary response. From this common ground, Adamany and Agree have produced a well-written contribution to the literature on political finance. Their book is a useful compendium of past campaign spending practices and patterns and a helpful background to understanding the development of current legislation in the field. It is also a pioneering attempt to design a program of government funding in the context of America's candidate-centered culture, a difficult task if candidate independence is not to be further enhanced and the party system is to be protected.

The authors' preferred method of financing U.S. political campaigns – and the major argument of the book – is public financing through a voucher system in general elections, with matching grants in the primaries, and stringent limits on campaign contributions. Vouchers have long been espoused by Agree; he worked with Senator Lee Metcalf on the 1967 proposal for the voucher system of financing presidential campaigns. The plan outlined in *Politicial Money* would involve as a first step a universal voter registration for federal elections, followed by distribution of presidential, Senate and House election vouchers to each registered person eligible to vote in such elections. Vouchers would be imprinted with the voter's name and address, and accompanied by explanatory literature, candidates' statements, and postage-paid envelopes for mailing the signed vouchers to chosen candidates. Vouchered funds would then be disbursed by the Federal Election Commission as payment for certified candidate expenditures.

While the goal of the voucher system, to permit citizen designation of government funds, is worthy, the system is cumbersome because of the different types of vouchers to be accounted for – the vouchers become a kind of "funny money," produced, distributed, and tracked at considerable expense. To encourage direct citizen participation, one questions why it would not be more feasible to take the process a step further – such as to a matching grant system as proposed for the prenomination period, or to a 100 per cent tax credit program within certain limits which would achieve the same goal at far less administrative expense than the voucher system (whose cost Adamany and Agree estimate at $50 million). It is not enough to legislate tax or matching incentives, or vouchers; action to improve solicitation and collection systems is essential to make them work, and the book's discussion of this is limited.

There are several areas in which the authors are unfair, or perhaps "too quick" in their negative judgments. One relates to disclosure laws. While laws requiring reporting of receipts and disbursements have been on the books for decades, disclosure at the federal level only became comprehensive and effective on April 7, 1972 – a date that will live in political infamy – with the implementation of the Federal Election Campaign Act of 1971. Coming midway through the 1972 campaigns (the Finance Committee to Re-Elect the President had already raised $20 million before the cutoff date), the law provided only partial opportunity to judge the effectiveness of disclosure with strict reporting requirements. While disclosure would seem not to have changed many presidential votes by November, it brought evidence of financial malpractice (the laundered Mexican checks of a predisclosure contribution) that helped begin the unraveling of Watergate.

Nor are we able to fully test the effectiveness of disclosure as a deterrent to political abuse at the federal level by experience in the 1976 elections. The limitations on contributions imposed by the 1974 Amendments to the election finance statutes make disclosure of contributors less interesting. The amount that an individual may now legally contribute may not be large enough to make him worry about hiding the fact he gave it, although disclosure of amounts given is still crucial for receiving matching incentive payments in government funding programs. Until there has been greater opportunity than we have had to date to measure the effectiveness of disclosure, it seems premature to dismiss it as readily as the authors do.

Adamany and Agree are excessively harsh also in their judgment on the effectiveness of tax incentives – particularly the system of tax credits whereby a contributor may directly reduce the amount of tax he owes by one-half of his campaign gift up to $50 ($100 on joint returns). Again, the quarrel is with the amount of data the authors have on which to base a judgment. Tax credits as an incentive to political gifts have been available at the federal level only since 1972, and were initially at half the $50/$100 figure which Congress set in 1974. Tax credits were available in some states before 1972, but the main form of tax relief for the political contributor offered by the states has been a tax deduction based on relatively low income tax rates. The deduction is less equitable in that it is governed by an individual's tax bracket. For the authors to claim that tax incentives have "failed entirely" is a judgment passed before these incentives have really been given an opportunity.

Despite these misgivings, the book is well worth reading and pondering. More blueprints of programs for government funding are likely to be necessary before an acceptable and constitutional system of government assistance can be developed for American politics.

HERBERT E. ALEXANDER

Citizens' Research Foundation,
Princeton, N.J.

The Unequal Elites. By Robert P. Althauser and Sydney S. Spivack, in collaboration with Beverly M. Amsel. (New York: John Wiley and Sons, 1975. Pp. 204. $12.50.)

This concise, well-organized, and carefully researched and written volume should be of particular interest to quantitatively oriented scholars of social and economic inequality between the races; and generally, its findings should be of interest to all social scientists. The authors' (Sidney S. Spivack directed the study but died before the writing phase) main objective is to determine whether black and white college graduates of equal resources will receive an equal share of rewards in society's marketplace, thus the title, *The Unequal Elites.*

The authors compare closely matched (on family social class, quality of education and grade-point average) samples of both black and white male college graduates. In total more than 800 blacks and whites who graduated between 1932 and 1964 were interviewed in 1967 and 1968.

The research indicates that generally the parents of black college graduates had attained higher education levels than had the parents of identically matched white graduates, though the parents of these black graduates held occupations with less, or only equal status. Similarly, the black graduates themselves obtained more postgraduate education than their white counterparts, although their current occupational status is only equal, and their median incomes are lower than the white median incomes by two to three thousand dollars. Thus the inequality experienced by these black elites is multifaceted. Not only have these black graduates achieved more educationally than their white counterparts, but in terms of their occupational status they have not received more for their efforts, and in terms of their income they have been rewarded less. Finally, these black elites report less satisfaction with their occupational positions and general living situations than do the white elites.

The weight of evidence clearly points toward racial discrimination in society's marketplace. As the authors themselves acknowledge (p. 168), however, possible need for achievement differences between these male black and white graduates have not been controlled for. It could be that evidence of less need for achievement among the blacks than whites could partially account for the status and income disparities (this could be considered discrimination in socialization). Conversely, evidence of higher achievement needs among blacks would point toward greater discrimination than that based on the authors' tacit assumption that the need for achievement is equal for both races. Indeed, the authors are remiss for not attempting to control for this variable in their research design, or at least for not reporting on the literature (or lack of it) on comparing need for achievement among blacks and whites.

Except for this problem, the work is methodologically sound and scientifically objective. Moreover, this book makes an original and valuable contribution to the empirical study of social and economic inequality between the races.

CLIFFORD J. WIRTH

University of Rhode Island

Foreign Policy and the Bureaucratic Process: The State Department's Country Director System. By William I. Bacchus. (Princeton, N.J.: Princeton University Press, 1974. Pp. 350. $14.50.)

Since World War II, the study of the making of American foreign policy has become a favored subject of social scientists, journalists, and practitioners, and much reportorial, monographic, and essay literature has been produced – in the nature of theoretical and empirical analyses, descriptive and historical recountings, memoirs, and case studies of particular episodes. This volume, according to its author, is intended not to supersede or replace other approaches, but to complement them. In terms of intent, content, and focus, it concerns foreign policy formulation in general; more specifically it deals with bureaucratic policy making – and even more precisely, with the role of middle-management bureaucracy.

While this volume tangentially treats the roles of the White House and other components of the governmental mechanism, as its subtitle indicates it concentrates on the function of the country directors within the Department of State, and it probes the permeative problem of policy coordination. It contributes to the overall understanding of the conduct of foreign relations and, like other recent in-depth studies of American chiefs of diplomatic missions, the presidential special representative, the professional diplomat, the ambassador-at-large and the country team, it helps to fill one of the glaring gaps in the operation of the United States foreign relations process.

Professor Bacchus's stated purpose is "to discover the actual place of the country director in the foreign affairs community, and . . . to gain insights into the policy-making process as a whole" (p. 9). He admits that his treatment is a "drastically simplified account" which slights many complexities. It is designed to explain "some of the difficulties underlying the continuing attempts at reform," especially that commencing in 1966 which, it was contended, "represented State's last real chance to assume control of foreign policy," and had it worked as intended, the overall influence of the Department of State "would have been substantially increased" (pp. 7–8). In short, this is a survey of the purposes, functioning, and successes and failures of the country director system during the past decade.

The basic data of this volume are derived from three related sets of interviews, embracing forty-six incumbent country directors (1968–1969), who provided information about their perceptions and attitudes; fifty-four other government officials (primarily "opposite numbers" who worked directly with the country directors), to ascertain their assessments of the system; and approximately forty others, who gave details concerning specific events, irrespective of the official positions held or their involvement in the system. The interviewing process used, including the author's questionnaires, is described in an appendix.

Relatively short, descriptive and analytical in approach, well organized and documented, and lucidly written, this volume is less concerned with the theoretical or scientific analysis of decision making than with the broader institutional and personal qualification aspects of policy making. Although encompassing the intellectually distinguishable political, bureaucratic, and rational approaches to the subject, it emphasizes the institutional component. It consists of an introduction, which explains the author's parameters and design, nine chapters, and three appendices.

The first chapter presents a general overview of the policy process and the specific bureaucratic role of the country director. Chapters 2 to 4 cope with the nature and functioning of the country director system, reviewing the need for and the mechanics of administrative innovation, the organizational posture of the country director, and his interaction patterns within and outside the Department of State, including relationships with field operations. The next chapter presents a more analytic view of selected aspects of the larger policy process, addressing such matters as the participants, the information factor, the consideration of alternatives, and the nature of constraints.

Review of the individuals serving as country directors is the subject of chapter 6, which touches on the interplay between personnel problems and resources on the one hand, and organizational ability to validate and fulfill established objectives on the other. After analyzing questions of effecting bureaucratic change and innovation in chapter 7, the author turns to assessing the country director system from the perspective of those most intimately involved, and discusses its future prospects. The final chapter summarizes the author's views on the functional value of the system and the possibilities of, and limitations on, working-level policy leadership and coordination.

Fourteen useful tables and three diagramatic charts supplement the analysis. The tables deal with such quantifiable matters as the level of country director contact with American embassy staff abroad, country director-foreign embassy relations, religions of birth and career progression of country directors, and comparison of age by grade of Foreign Service Officers and country directors. The appendices present a comprehensive profile of 153 country directors who served from 1966 to 1973 (included are such matters as birthplace, race, sex, education, age, rank, career status, promotion, and career progression); a breakdown of country directors by geographic areas of responsibility; and the author's research methodology and interviewing program.

As with many ventures in foreign relations organizational and procedural reform, the author concludes that the country director system "did not live up to the expectations of its architects," in that country directors could hardly be said to have assumed "full responsibility . . . for all activities in the country or countries assigned to them" (p. 288). Nevertheless, ending on an optimistic note, he adds: "In the absence of progress in establishing a clean-cut role for State in the new era, the modest success of the country director system at the margins of the policy process represents a not inconsiderable achievement" (p. 301).

In summary, this review of an aspect of Department of State bureaucratic functioning possesses advantages of limited and manageable scope, probing analysis, relative brevity, and scholarly treatment. Its principal contribution lies in its enhancement of understanding of the intricacies and interactions that characterize the foreign policy system, as well as the problems of aligning expectation and pragmatic operation of institutional processes. It should be of special interest, therefore, to both practitioners and students of the conduct of foreign relations.

ELMER PLISCHKE

University of Maryland, College Park

The Vision and the Dream of Justice Hugo L. Black. By Howard Ball. (University, Alabama: University of Alabama Press, 1975. Pp. 232. $8.50.)

The late Justice Hugo Black, who died in 1971, got another round of recognition when his long-time colleague William O. Douglas resigned from the Supreme Court in 1975. For example, on the latter occasion Philip Kurland, the distinguished lawyer-scholar of the University of Chicago, remarked that Douglas and Black had had more profound influence in shaping the Constitution of the United States than had any other Justice since Chief Justice John Marshall.

We cannot know what Black would have thought about Kurland's assessment. A comparatively modest man, Black always declined to make public comment on similar if less sweeping judgments about his contributions to the country through the Court. If, in private, a presumptuous acquaintance asked what he thought about statements concerning the great extent of his influence, he was inclined to respond that the main influence he had tried to exert was toward making judges apply Constitutional provisions in their original meanings.

That Black tried consistently to be a constitutional literalist is amply illustrated in this book. Professor Ball here has compiled Black's views, primarily as expressed in his Court opinions during thirty-four years, on a number of major constitutional issues, including substantive due process and government's prerogatives in regulating the economy; procedural due process; and First Amendment guarantees of individual liberty. Another section is devoted to Black's opinions and views on what is entitled "Substantive Due Process: Regulating Social Relations." In describing Black's constitutional literalism or the pre-eminence he accorded to the First Amendment, Professor Ball is not telling us anything new. Nevertheless, he does usefully show that Black's approach to issues in the several emphasized areas remained constant, and his influence on the Court remained strong, to the last. (Unfortunately, a miserable job of copy editing – revealed in misspelled words, incomplete sentences, and other typographical errors – detracts further from this limited substantive contribution. The publisher owes the author an apology.)

Black's constitutional concepts are clear and have been influential, but they are not quite so self-explanatory, so completely and automatically persuasive as Mr. Ball apparently believes. Neither are those concepts, despite admirable internal consistency, totally free of inconsistency as Black applied them.

Anthony Lewis, a perceptive writer and an admirer of Justice Black, recently wrote in *The New York Times* that "almost nothing is explicit in the Constitution – judges have woven the crucial decisions from generalities." I believe Mr. Lewis would agree that the Bill of Rights is more explicit about boundaries of action, for governments and individuals, than is much of the rest of the Constitution. In any event, even if the First Amendment's commandments are as absolute as Black thought them to be, they are not always unambiguous as guides in concrete cases. The first part of the First Amendment is: "Congress shall make no law regarding an establishment of religion. . . ." Justice Black repeatedly stressed that "no law means no law." But in a variety of cases, Black and his colleagues had to exercise judgment about whether statutes or programs in question involved "an establishment of religion."

As Ball recognizes, Black hated the "balancing" concept espoused and applied by a number of his brethren – earlier by McReynolds and Cardozo and later by Frankfurter and Harlan. But it is hard to see what other intellectual and judicial process was involved in the *Korematsu* case in 1944 where the Court upheld, in a Black opinion, the "quarantining" of U.S. citizens of Japanese descent in wartime. Chief Justice Warren said, after his retirement, that *Korematsu* was a decision which, in retrospect, he thought constitutionally erroneous and one which he personally regretted, since the Court had supported the position he had argued as Attorney General of the State of California. Black, by contrast, in an interview with Anthony Lewis published shortly after the Chief Justice's death, said that he thought the decision was a correct one, because people were rightly fearful of the Japanese, and, had Japanese forces attacked the Pacific Coast, a lot of innocent Japanese-Americans "would have been shot in the panic" (p. 206). That position more reflects a practical balancing of possible results than an absolute view of constitutional commands.

I join in the belief that individual liberties and the democratic process in America are enhanced because of the long presence of Hugo Black on the Supreme Court. Yet at least some of the "clear, explicit and lasting Constitutional boundaries" which Black recognized so precisely and believed in so passionately were partially the product of his own personal convictions, informed by a thorough reading of history as well as by other influences in his life. Professor Ball, beyond reciting some of Black's more important opinions, nicely summarizes the Justice's beliefs about the efficacy and fragility of American democracy, and about the rule of law. Other authors have illuminated the context in which his views became law, or factors of influence on the law. But many questions remain, some of them relating to the origins of particular judicial positions, others to the motivating impulse that made Black such a fierce competitor in political, legal, and philosophical arenas. Not even in the aggregate collection of books about him has the whole story of Hugo Black, as a man, lawyer, politician, philosopher, and jurist, yet been told.

STEPHEN P. STRICKLAND

University of California, San Francisco

Coastal Resource Use: Decisions on Puget Sound. By Robert L. Bish, Robert Warren, Louis F. Weschler, James A. Crutchfield, and Peter Harrison. (Seattle: University of Washington Press, 1975. Pp. 206. $10.95.)

Saving the Coast: California's Experiment in Intergovernmental Land Use Control. By Melvin B. Moguloff. (Lexington, Mass.: D.C. Heath and Company, 1975. Pp. 136. $12.00.)

These are two quite different books dealing with a single but complex issue: the appropriate political institutions for governing shorelines in the United States. The first is an analysis of the economic, demographic, political, and natural resource setting of Puget Sound in the State of Washington and the events leading to the adoption of the Shoreline Management Act in 1971. The second is a critical evaluation of California's brief experience under the Coastal Initiative passed in 1972. The first is written from the multiple perspectives of the economist and a political scientist consciously utilizing political economy and public choice frameworks for analysis. The second is less concerned for theory than for practical judgments concerning the promise of the Coastal Initiative to deal with issues of coastline use and management.

Despite the differences in approach and perspective, the stories they tell are similar. In both states, environmental groups were frustrated in repeated efforts to obtain legislative action on proposals to impose controls on coastline development. The result was in each case a resort to the initiative process and approval by the voters of measures establishing a broad role for the states in shoreline management. In neither case did the initiatives go as far as the environmentalists wanted, particularly in limiting local involvement in shoreline use and management decisions. The statutes call for continued responsibility for local government in shoreline decisions, with state political units having authority to review local decisions with respect to their conformity to the standards set down in the law.

The study of Puget Sound is a comprehensive examination of the complex ecology – both natural and human – of the total region. It depicts clearly the diversity of the region and the natural division and interdependencies of interests with respect to the development and use of the shoreline. This diversity of values is clearly displayed in the vote on the Shoreline Management Act, with the voters of the more sparsely settled areas opposing the Act, while the voters of the Seattle metropolitan region strongly supported it. The authors demonstrate the continuing struggle between local interests in sparsely settled, undeveloped, and economically less well-off areas who want the perceived benefits of development and the urban interests who look upon these less densely occupied regions as their playgrounds.

The authors provide a learned discussion of the principal elements of analysis of the political economists: externalities, public goods, common pool, flow resources. But the application of these principles in terms of the actual situation in Puget Sound is disappointing. The authors obviously believe in some form of regional government, although they recognize the difficulty of sorting out local, regional, statewide, and national interests even in as distinctive a region as Puget Sound. They believe in regional government because it reduces costs for interests that participate in environmental decision making only at high cost to themselves and with difficulty of sustained efforts. But just how does one apply the principles of political economy – i.e., which externalities should be dealt with by which level of government? Agreeing that some resources are public goods, what system of public management will provide for optimization of the public welfare? Who should have what kind of voice in what kinds of decisions? If one accepts the idea that the locality is too narrow a framework to encompass all interests concerned with the locality's resources, what is an appropriate broader framework? And how does one tell what it is? What is the appropriate weight to give local interests concerned for jobs and development as against broader regional or national interests concerned for environmental protection? And what would they conclude if dominant local interests were for environmental protection and the broader regional and national interests preferred development? There are no answers in this study despite the sophisticated tools of analysis.

The Moguloff book offers no such comprehensive discussion of the physical and social environment of the California coastline or of the events leading to the Coastal Initiative. It does offer an assessment of what the regional and state commissions did during their first year, particularly in coming to terms with the political environment in which they operated. This involved two major tasks: granting or denying permits for development and developing a coastal plan. The plan is only now taking form and therefore is not included in this assessment, but the permit-granting function is evaluated. The essential argument is that the regional and state commissions have not stopped development along the coastline, that they could not without substantial risks to the viability of the coastline control effort itself, but that they have made a difference. In effect, they bargained with developmental interests, making them pay a substantial price in terms of accommodating other interests in bringing their projects to fruition. The author emphasizes the dangers inherent in the control effort: that denials of permits for development would lead to bitter and prolonged battles over alleged "taking" of property without due compensation. He concludes they have done an effective, if necessarily imperfect, job of keeping coastline protection in an important place on the political agenda.

These are both useful books by authors who are thoughtful and well informed. They recognize complexity, multiple sets of values and the realities of the political systems in which decisions must be made. It is not clear from their analysis, however, why shorelines should be considered apart from other natural environments that are threatened by development. What are the consequences, for example, of imposing controls on shoreline development – controls that have differential effects on various groups wishing to use shoreline resources – while failing to impose similar controls on adjacent resources that are not defined to be within the "natural" shoreline boundaries? Any definition is bound to be arbitrary, but it is not clear why the shoreline or any other natural phenomenon should define a resource management area. A metropolitan region, while not "natural," does have boundaries capable of definition in demographic or economic terms which may be as meaningful as the boundaries of "natural" areas such as shorelines or river basins. Perhaps the area west of the Cascades in Washington and the part of California south of the Tehachapis constitute appropriate regions, not because of physical geography but because people interact intensively in those regions, depend on each other, and compete with each other for resource use.

Whatever one's conclusions may be, these studies have provided useful analyses and arguments regarding the environmental management problems of our era. They give us insights into resource management design questions and sensitize us to the significant issues.

DEAN E. MANN

University of California, Santa Barbara

In Command of Tomorrow: Resource and Environmental Strategies for Americans. By Sterling Brubaker. (Baltimore, Md.: The Johns Hopkins University Press, 1975. Pp. 177. $7.95.)

Like Brubaker's *To Live on Earth: Man and his Environment in Perspective* (1972), this new book, also a Resources for the Future Study, is intended for the general reader.

The two books furnish a quite comprehensive view of many of the physical, social, and economic problems faced by man in attempting to ensure his long-run future on earth. In the new book, as he did in the first, the author declines to join the doomsayers. He says he is optimistic, in the Western tradition, that man can control *(In Command of Tomorrow)* his fate in the long run in spite of materials depletion and environmental degradation.

Brubaker's main concern in the new book is with the United States. For many political scientists, as well as other readers, one basis of his focus on the United States will be debatable. He feels that only the United States is sufficiently well organized to achieve the necessary consensus to adopt and maintain over the long run the strategies to counter depletion of basic resources and degradation of the environment. He does not ignore global implications, but his emphasis is on what the United States can do in its own interests, as well as in serving as an example to other nations.

For students of political feasibility, Brubaker's discussions of the possible major strategies for man, and especially for the United States, are perhaps the most interesting parts of the new book. He contends that there are only two paths for man to follow in securing permanent occupancy of the earth, and that the choice between paths cannot be too long postponed. One path is to try to find a "modest but sustainable place in the natural system by changing to a technology that is nondepleting in character" (p. 4). Brubaker refers to this path as a "low-technology" course. He notes that this approach, while promising to extend the time of man's tenure on earth, presents some inevitable difficulties. The reasons are many: many materials inevitably dissipate with use, and recapture of some materials would require large amounts of energy. Further, the loss of metals over time would make it difficult to capture and utilize energy. The first path presents the dangers of inevitable economic decline.

Mr. Brubaker's second path, which he thinks is the path we should choose, is in part based upon his belief that a high production society is necessary to sustain a worthwhile culture, and in part on the belief that presently Americans, and eventually others, will settle for no less. He thinks we should make

> a calculated effort to escape the constraints of resource exhaustion through new technologies. It would not simply rely on enlarging the conventional supply of exhaustibles but rather would seek to replace them with inexhaustible resources. The difference is between a temporary reprieve and a permanent solution. The second path means finding an inexhaustible source of energy as a prerequisite to other technology that would make use of more common materials. Given ample energy, we might make a direct attack on entropy, using common rock and sea water as resources, or perhaps we could reach for the alchemist's dream of elemental transmutation (p. 5).

Clearly, the "alchemist's dream" is suggested with tongue in cheek!

The book is excellent in its explanations of issues, and the strategies prescribed seem sensible. But the book will arouse disbelief among a number of readers. First, many will not agree that technology can be counted upon to achieve such heights. Second, although many of the strategies suggested will find approval by many observers of the economic, social, and political scene, there will be disagreement on what seems to be Brubaker's belief that these strategies can be followed without much more positive, even authoritarian, government than we now have.

ROBERT H. PEALY

University of Washington

TV Violence and the Child: The Evolution and Fate of the Surgeon General's Report. By Douglass Cater and Stephen Strickland. (New York: Russell Sage Foundation, 1975. Pp. 167. $5.95.)

Since its introduction in the United States, television has been the object of great hopes and fears. Proponents of television have pointed to its potential for expanding our experiential horizons through almost instantaneous exposure to people and events around the world. Critics have warned, however, of its potential for constricting our intellectual and social horizons by converting active humans into passive vidiots. In addition, many have expressed deep concerns about the possibly harmful consequences of exposing millions of children and adults to a television diet rich in violence.

This fear led eventually to the establishment in 1969 of the Surgeon General's Scientific Advisory Committee on Television and Social Behavior. Three years and 1.8 million dollars later, the committee reported "... a preliminary and tentative indication of a causal relation between viewing violence on television and aggressive behavior; an indication that any such causal relation operates only on some children (who are predisposed to be aggressive); and an indication that it operates only in some environmental contexts. . ." (p. 76).

In *TV Violence and the Child,* which was commissioned by the Russell Sage Foundation, Douglass Cater and Stephen Strickland provide a highly readable history of the events leading to and immediately following the publication of this conclusion. Cater, as director of the Aspen Institute Program on Communications and Society, and Strickland, as director of the Washington, D.C. office of the Health Policy Program of the University of California, San Francisco, bring to their discussion first-hand experience with media and public policy issues. Chapters are devoted to describing the controversial selection procedure for committee members and the internal conflicts over the development of a *modus operandi;* to summarizing the findings of various laboratory and field research projects on the effects of televised violence on children, of institutional analyses of the television industry, and of content analyses of television programming commissioned by the committee; and to chronicling the responses of the public, the government, and the television industry to the report. On the basis of their interviews with participants and their investigation of public records in the controversy surrounding the report, Cater and Strickland conclude that the nation's scientific, public, and governmental institutions are ill-equipped for recruiting expert talent to provide quick and clear findings to guide public policy decisions, especially when the problem is complex and politicized.

The book's strength lies in the authors' ability to synthesize information from a variety of sources to provide a comprehensive history. Students of public policy decisions will no doubt welcome this, but they should not turn to *TV Violence and the Child* in order to find a social scientific analysis of a governmental commission. Indeed, there is little social science in this discussion of the Surgeon General's committee. No references are made to analyses of previous commissions, and no social scientific concepts or models are employed as explanatory tools. Despite the book's value as a chronology of events, one cannot help wondering whether the material presented is sufficiently detailed to be of value to students of public policy. Those who wish to develop general models of governmental decision making and who hope to use *TV Violence and the Child* as a source of data are likely to be frustrated. The detailed data necessary for developing generalizable conclusions is probably lacking.

RONALD P. ABELES

Social Science Research Council,
New York

Thomas McKean: Forgotten Leader of the Revolution. By John M. Coleman. (Rockaway, N.J.: American Faculty Press, 1975. Pp. 352. $16.95.)

As a biography this book has shortcomings, the most serious of which is the decision to end the narrative in 1780, before McKean's service as governor, 1799–1809. The omission is particularly puzzling in view of the inclusion of seemingly tangential material on McKean's early life. In chapters II ("Education under the Rev. Francis Alison") and VII ("Robert McKean and the New Jersey Medical Society") Thomas McKean is not even the central figure. This is not to say that these chapters have no value. Indeed, they are instructive pieces on two aspects of colonial social history that students of colonial education and medicine should welcome. But, they belong in journals. In this book they are distracting. The remaining parts of McKean's early life are well done, depicting a remarkable colonial success story. From decidedly middle-class beginnings McKean made his way, through politics, marriage, and social contacts, into the elite circle of the middle colonies. His use of a legal career as a stepping-stone to political office is particularly revealing of the inextricable mixture of the lawyer's art and politics in the colonial era.

Professor Coleman's understanding of the intricacies of Pennsylvania-Delaware politics is acute, and his explanation of the coming of the Revolution to those two colonies is expert. The account of the manner in which the radicals maneuvered Pennsylvania into support for independence in 1775, with propaganda, class appeal, and not a little military intimidation is a side of our revolution not often seen. Professor Coleman's picture of the American Revolution is much more recognizable to students of the modern phenomenon than many Americans would have it.

The handling of the internal workings of the revolutionary governments, both state and national, is on a par with the recounting of the coming of the rebellion, and it is here that the book makes its most significant contribution. Professor Coleman does not succeed in elevating Thomas McKean to the stature of a John Adams. McKean is always present, however, often in the inner councils, sometimes on the periphery – but consistently a participant. This is certainly to be expected, as McKean's service on the national level dated from the Stamp Act Congress and his was the longest tenure of any of the members of the Continental Congress. What Professor Coleman has provided is a close look at the revolutionary governments, from local level to national level, through the eyes of one participant. This, rather than McKean's career itself, is the book's significance.

JAMES P. WHITTENBURG

College of William and Mary

The Politics of Revenue Sharing. By Paul R. Dommel. (Bloomington: Indiana University Press, 1974. Pp. 211. $8.50.)

When several prominent mayors labeled the revenue sharing program a "gigantic double-cross" and a "cruel hoax" it may have come as a surprise to some who had heard the same mayors strongly supporting the passage of revenue sharing several years before. The explanation of this paradox is the main focus of *The Politics of Revenue Sharing.* Professor Dommel examines the various modifications that initial proposals underwent before passage of the State and Local Fiscal Assistance Act of 1972. The coalition necessary for passage of the Act included Republicans and conservatives who wanted to decentralize decision-making power as well as liberal Democrats who were under pressure from urban interests who badly needed funds. Dommel's analysis traces the development of the issue of revenue sharing and makes understandable the change in attitude of the mayors.

The 1950s and 1960s saw increasing pressure on state and local governments for greater services while at the same time tax revenues were not rising proportionally. Growing dissatisfaction with the proliferation of federal aid programs and the sophistication of grantsmanship necessary to obtain federal funds generated disenchantment leading to several proposals to replace the existing federal grant system with direct aid to the states.

The forerunner of the 1972 Act was a proposal for tax sharing by Walter Heller which was further developed by a Johnson task force headed by Joseph Pechman. It was spurred by the expected economic boom in 1964 and the wish to avert fiscal drag by distributing surplus revenues to the states. The plan was subsequently modified so as not to depend on a surplus but to provide a fixed percentage of federal income taxes to the states. The money would have no strings attached and would supplement existing federal grant programs.

Dommel traces the capture of the initiative for revenue sharing by the Republicans after Johnson's abandonment of the Heller-Pechman proposal subsequent to the 1964 elections. The many revenue sharing bills introduced by Republicans covered a large range of proposals but the dominant thrust was to use tax sharing as a means of turning much federal policy making over to the states by abolishing many federal grant programs.

In the presidential campaign of 1968 both candidates favored some form of revenue sharing and the focus of the various protagonists shifted from Congress to the White House. The cities, however, were still suspicious of proposals that would funnel federal aid through the states. They felt that such a program would give too much discretion to state officials who would not see urban problems as major policy priorities. This would be particularly dangerous to the cities if categorical grants were replaced by revenue sharing.

In August of 1969 President Nixon announced his plans for revenue sharing as part of his "new federalism" program. His primary concern was the decentralization of governmental power, but he did not propose that the existing grant structure be replaced by revenue sharing. According to Dommel this was the key to liberal congressional support for the plan. The cities were promised a share of the funds and were assured that the categorical grant programs would not be eliminated. Thus liberal members of Congress were put under much pressure from urban interests to support revenue sharing.

After explaining the changes in the revenue sharing package necessary to gain the support of the diverse constituencies which finally came together to make passage possible, Dommel spells out the final compromises reconciling the president's plan with House and Senate concerns. Liberals and urban interests had to be assured that revenue sharing would be "add-on" and not "substitute" money. One of Dommel's conclusions is that mayors and liberals were not entirely wrong when they cried "double-cross" after seeing Nixon's 1974 budget. It recommended many cuts in grants-in-aid to state and local governments, some of them being explicitly justified by the availability of revenue sharing funds.

Dommel's book is particularly good in putting the revenue sharing issue in the context of the overall policy-making process. A major strength is the clarity with which he traces the development of the issue. From the early proposals to replace the categorical grant system to the final compromises to attract urban interests, he pinpoints the shifts in the strategy of proponents and their bases of support. His analysis makes us better able to evaluate statements about what revenue sharing was intended to be.

In the final chapter he presents some data on the initial reports on the use of revenue sharing funds. By now these data have been supplemented in subsequent reports and analyses. The impact data, however, are not the main point of Dommel's book and can be found updated in several books oriented toward the economic and distributional effects of revenue sharing. Dommel's purpose is to analyze the development of the revenue-sharing concept and its journey through the policy-making process, a task which he executes admirably.

JAMES P. PFIFFNER

University of California, Riverside

Shoreline for the Public: A Handbook of Social, Economic, and Legal Considerations Regarding Public Recreational Use of the Nation's Coastal Shoreline. By Dennis W. Ducsik. (Cambridge, Mass.: The MIT Press, 1974. Pp. 257. $12.50.)

Fifty-five per cent of the American people live within the 50-mile coastal belt bordering the Atlantic and Pacific oceans, the Gulf of Mexico, and the Great Lakes. Only one-third of these 60,000 miles of shoreline are suitable for recreation; only 5½ per cent of that one-third is publicly owned. Yet public demand for recreation is increasing at an annual rate of 12 per cent, and the recreation industry is the fourth largest and fastest-growing industry today.

The paradox of shrinking land supply and increasing demand for water-oriented leisure activity has been created by a policy-making process which has historically and systematically underrepresented the value of public open spaces. To encourage the development of environmental and ecological priorities in shore line policy making, Congress passed the Coastal Zone Management Act of 1972, which provides matching funds for coastal states that create state coastal zone management programs. The

Act mandates expansion of public recreational opportunities by "giving full consideration to ecological, cultural, historic and aesthetic values as well as to the needs for economic development" (p. 7).

Since the tendency has been to promote private economic interests at the expense of the more diffuse, ill-defined social welfare concept, Dennis Ducsik, Instructor in the Department of Civil Engineering at MIT, has written this book to provide a "groundwork for the development of effective policies to combat the continuing encroachment on the public interest in the shore line, and to bring about a more effective representation of that interest in coastal decisionmaking" (p. xiv). Ducsik sees the conflict as between "the need for expanded recreational opportunities for the public (especially in urban areas) . . . and the desire for intensive private development. . ." (p. 4).

Part I of the book develops the historical, philosophical, and institutional reasons for the problem and arguments supporting the environmental/ecological perspective. He sees the land-area interface as a resource rather than a commodity which must be enhanced to protect its opportunities for human recreational uses and to preserve its vitality as an ecological system. Supply of suitable coastline is limited by site problems and natural factors such as pollution and erosion, but more importantly by the economic and political relationships which tend to safeguard and enhance the rights of private property owners. "The private market is ill-suited for the allocation of recreational resources for public use. . . . Public recreation . . . ranks low on the capital intensive scale, its value to the public is diffuse, costly to collect and possibly unquantifiable" (pp. 67, 69).

Having established the need for government intervention to promote public needs, Ducsik focuses in Part II on legal strategies for change because "it is the law that will shape and coordinate policy alternatives. . ." (p. 84). Two chapters examine the law of the seashore, and common law approaches to create availability and accessibility of littoral open spaces for the public. Chapters 8, 9, and 10 each deal with possible state policy tools: purchase of land through state powers of acquisition or easement or both; the use of state police power, expanded to include open-space objectives within its scope; and land-use controls such as zoning, setbacks, official mapping, and other regulatory strategies. The final chapter ably summarizes the rationale for collective action, the need for administrative flexibility in building a case, and the necessity to balance social and economic objectives in setting priorities for shoreland allocation.

While the language of the book tends to be technical, dry, and a bit repetitive, and the rationale traditionally pro-environment, the technique chapters present the relevant case law clearly and concisely, emphasizing both the advantages and disadvantages of each option from the policy maker's perspective. Indeed, one of the most useful aspects of the book is its focus on institutional interactions between state and municipal governments, and between legislatures and courts, which should provide policy makers with a clear map of past relationships as a guide to future activity.

CAROL S. GREENWALD

Radcliffe Institute

Pork Barrel Politics: Rivers and Harbors Legislation, 1947–68. By John A. Ferejohn. (Stanford, Calif.: Stanford University Press, 1974. Pp. 288. $12.50.)

Professor Ferejohn tests two hypotheses about congressional behavior in this study of the Corps of Engineers' appropriations: (1) the institutional distribution of influence in Congress will affect legislative outcomes, and (2) programs which can be distributed among constituencies will receive favored treatment by Congress. Ferejohn is ingenious and effective in presenting many tables, graphs, and regression analyses to support his arguments in defense of the hypotheses. Most of his empirical conclusions are consistent with commonly held views, and some of the data suggest new hypotheses about relationships among congressional influence of alternative models of congressional behavior. Unfortunately, the Corps of Engineers is a very special agency, and therefore it would be hazardous to use this one sample to verify any general statements. And even within the restricted discussion of the Corps, the conclusions drawn by this sophisticated analysis are not always persuasive because of the limited data and tests.

The three chapters of Part I present a summary of the economic and political benefits-costs literature, and informal arguments for the hypotheses. A more careful defense of the hypotheses is contained in the book's concluding chapter. The second part, four chapters, reviews the budget cycle of the Corps, and the House, Senate and Conference Committee treatments of the aggregate Corps budget. Part III, the most ambitious section of the volume, analyzes the distribution of corps projects among states by evaluating marginal changes associated with membership in key House or Senate committee posts.

The conclusions that can be drawn from the analysis of the aggregate appropriations to the Corps are flawed by limiting the study to one agency, and when others are considered, the comparison is to an aggregate of other bureaus. As the author points out, the Corps "correctly considers itself to be a Congressional agency," and therefore it is not surprising that Congress and its committees have a different role in regard to the Corps budget. To test his hypothesis that a divisible budget will be favored by a constituency-serving Congress he would have had to deal with a number of agencies whose budgets satisfy his condition of divisibility among states, and there are a great many of them. In fact, most of the budget is divisible when you consider inputs as well as outputs.

The heart of the volume is the empirical evidence about the distribution of projects by the influence of congressional members. Ferejohn concludes that the states of members with influence over the appropriation process will gain by their membership. Influence will be exercised through well-recognized attributes such as committee membership, seniority, chairmanships, and so on. Generalized influence in the legislature, however, has no constituency payoffs; to be effective influence must be specific to an agency. These hypotheses are tested through a set of regressions of memberships, seniority, etc., on the number of new starts of construction in a state initiated by the House or the Senate or proposed in the president's budget.

If a state receives relatively more projects than it "needs" because it has relatively more members of key committees, then influence has a payoff. Reasonable, but one might argue that the distribution of representation is a function of willingness to invest political resources in response to constituency demands and that "influence" is no more than a mechanism to allow a state to pay a "higher price" for "more valued" goods. If there is a political marketplace then the analyst has to separate out the forces of demand and supply to discover whether there is any independent weight to be given to congressional organization. Unfortunately, Ferejohn's efforts are marred by the variables he has analyzed, but he has made a beginning.

A questionable part of the empirical analysis is the use of the number of Congress-initiated construction starts per state as the dependent variable – the measure of political payoffs to influence. The number of project starts and the state as a unit of aggregation should have some positive value to a legislator, but they may not be the project's most significant characteristics for the state or for the legislator.

The number of projects need not be closely related to the size of current or future appropriations within a state. Since projects come in many sizes, a state that has proportionately more starts need *not* have proportionately more appropriations or constituency benefits. Certainly, the features of a project and its scale are of importance, and one should expect that the legislator is forced to make tradeoffs among a project's many dimensions. The legislator may appear to be very effective by sponsoring a project, but if it is small and single-purpose, his influence for constituency payoffs may be illusory. New construction starts are necessary for a state or district to be favored, but they are not sufficient. In fact, Ferejohn properly stresses that majority rule will bias the budget toward small projects distributed over many states. Therefore, the number of projects necessary to get political consensus may enter as a constraint, but if the leadership varies the distribution of sizes, the budget not be "grossly" inefficient. A better variable would have been the anticipated scale of future appropriations.

The second part of this variable, the total per state, seems even more questionable. The anecdotal and theoretical literature on congressional behavior stresses the satisfaction of district, rather than state, constituency interests as a legislator's objective. Even Ferejohn, when he elaborates his arguments in the final theoretical chapter, deals only with district constituency representation. He chose not to use construction starts in a district as the dependent variable because he found a negative relationship between influence and starts – a surprising conclusion that calls for more analysis than the few sentences Ferejohn presents. He defends the state rather than district variable on the basis of the cohesiveness of state delegations. But not all states have cohesive delegations. It would have been interesting to use cohesiveness of delegations as an independent variable to see how powerful it proves to be. In passing, the author mentions that California and Texas are cohesive delegations and that these states have several members on key committees. Possibly, these two facts are not independent; cohesive delegations overstaff key constituency committees and are rewarded by a larger number of starts. Whatever the pattern of causation for a state, the exclusive attention to the state variable may have led Ferejohn to make incorrect judgment about the payoff for generalized congressional influence. Leadership roles outside of the public works committees may have significant payoffs where the leader needs it, in his district. Senior legislative leaders have careers in Washington, not in state offices, and therefore they may not be as active in state coalitions and objectives as members who choose more parochial roles. A total neglect of district payoffs may have seriously weakened any conclusions that could be drawn about the influence of nonpublic works leadership.

I do not offer the above criticisms to discourage readership and serious study. On the contrary, the dissertation is a useful addition to the growing body of quantitative Congressional studies. There are sections, e.g., the relations between the Senate and House, which have substantial conceptual improvements on earlier works and present impressive empirical evidence. The criticisms are meant to help in the cumulative process of research. If others deepen Ferejohn's work on the Corps and extend it to other agencies we will have a much firmer understanding of the relationship between legislative institutions and policy outcomes.

JULIUS MARGOLIS

University of California, Irvine

Blacks and the Military in American History: A New Perspective. By Jack D. Foner. (New York: Praeger Publishers, 1974. Pp. x, 278. $10.00, cloth; $3.95, paper.)

Jack Foner's *Blacks and the Military in American History*, demonstrates that Afro-Americans played significant roles in every American war beginning with the Revolution in spite of the resistance to their participation by generations of politicians and military officials. The Revolutionary War set a pattern in which blacks were accepted into the military during hostilities, when white manpower was in short supply, the response to mobilization was poor, or there was danger of black defection to the enemy. Admission was delayed to avoid alienating slaveowners, and when peace came blacks were discharged as quickly as possible. With lower manpower demands the military returned to its conventional practice of favoring whites.

This pattern continued well into the twentieth century, but Professor Foner limits himself to a presentation of historical detail, and leaves institutional patterns and changes undiscussed, although they are implicit in the material.

For example, Foner shows that successive generations of blacks believed their performance in war would prove their worth as human beings and citizens. Veterans of the Civil and the Spanish-American wars, and W. E. B. DuBois in 1918 felt that war would bring " 'an American Negro with the right to vote . . . work and . . . live without insult' " (p. 109). By World War II and the Korean war, blacks struggled to gain entry but with less hope about the results, although in 1967 Lt. Col. George Shaffer said of the high casualty rate of blacks: " 'I feel good about it. Not that I like bloodshed, but the performance of the Negro in Vietnam tends to offset the fact that the Negro wasn't considered worthy of being a front line soldier in other wars' " (p. 205).

Second, Foner demonstrates how institutions such as the American military have acted as carriers of their society's values by consciously teaching their allies the theory and practice of racism.

Third, racism has assumed radically different forms in the years between 1900 and 1970, according to Foner. Blacks were admitted into the military by 1945, but were concentrated in labor battalions, and disciplined by discriminatory procedures. Army "justice" for black soldiers was so swift that Thurgood Marshall remarked in 1951: "even in Mississippi a Negro will get a trial longer than 45 minutes. . ." (p. 190). Official desegregation of the military had begun in 1954, but supportive facilities were still being desegregated in 1963.

Finally, the military has been viewed as a channel for black upward mobility, but Foner's study suggests that the military's absorption of large numbers of unemployable blacks mutes economic and social discontent. For example, 64 per cent of eligible blacks were drafted for Vietnam in 1967, but only 31 per cent of eligible whites; 27 per cent of black soldiers were assigned to the infantry, but only 18 per cent of whites, and 20 per cent of army fatalities were accounted for by blacks. To complete the cycle, higher proportions of blacks end up in military courts and receive dishonorable discharges, which officially certify them as unemployable. Foner's work suggests that desegregation has integrated racism into the military rather than eliminated it.

In spite of its limitations, Foner's study will be a valuable sourcebook on the military's contributions to the status of blacks in the United States.

DIANNE M. PINDERHUGHES

Dartmouth College

Who Shall Live? Health, Economics, and Social Choice. By Victor R. Fuchs. (New York: Basic Books, 1974. Pp. 168. $8.95.)

In the midst of the uncertainty and anxiety that pervades the American health care establishment – especially concerning the future role of government – a calm, articulate analysis of health policy and health care is a particularly welcome and useful contribution. And it is precisely this that economist Victor Fuchs has given us in his lean, eminently readable monograph.

Who Shall Live? deals with the ends and means of American health policy. Joining a growing number of leading observers of the American health scene, Dr. Fuchs (Vice President and senior research staff member, National Bureau of Economic Research) encourages us to view the pursuit of health (evidenced by reduction in mortality and morbidity) as the objective of national health policy and to view health care itself as one of the factors, together with heredity, individual behavior, and environmental influences, that affect morbidity and mortality. Among the data used to buttress his argument, Fuchs points to selected mortality data by age, race, and sex that is worth pondering. Together with what is known about etiology of various causes of death, these data highlight the broader fundamental theme of his book, that the connection between health and medical care is much more tenuous than we are inclined to believe. The importance of genetic and environmental factors and personal behavior and "lifestyle" appear to be much more important than most would suspect, common sense notwithstanding. This is the theme of a fascinating section analyzing mortality data for the contiguous states of Utah and Nevada.

The bulk of Fuchs's book is devoted to the medical care system (with separate chapters on the physician, the hospital, drugs, and health care financing) reflecting the fact that the health crisis in America centers on financial and geographic access to medical care services. Moreover, the principal health issue in the United States remains National Health Insurance – how to redistribute equitably the growing per capita cost of medical care among the population consistent with the efficient production and appropriate utilization of health care services and without exacerbating the already high rates of inflation in this sector. In focusing on physicians, hospitals, and drugs, Fuchs deals with the lion's share of medical care costs.

His treatment of these subjects goes far in helping the reader understand the behavioral relationships and incentives at work in the American health care system. Such understanding is fundamental if we are to be successful in controlling growth in per capita costs of care in the years ahead. But, no matter how well we do on future cost containment, we are faced with the question of who is to pay the cost of medical care services. If there is a single theme in his chapter "Paying for Medical Care" it is that such costs cannot be shifted from consumers to some other party like government, or business. Costs always come back to the individual, if not directly, then indirectly through increased insurance premiums, reduced wages, higher prices of the products of American business, or higher taxes. To be sure, alternative payment schemes can mitigate the devastating costs to specific families or individuals from prolonged or catastrophic illness, and mechanisms can be devised that shift the burden of paying for health care from the lower income to higher income families and individuals, but in the last analysis the average per capita cost of care will be the same for a given level of services utilization.

Written by a specialist, in language everyone can appreciate, *Who Shall Live?* is a clear contribution to better public policy in health.

MICHAEL S. KOLEDA

National Planning Association,
Washington, D.C.

Crime and Criminal Justice: Issues in Public Policy Analysis. Edited by John A. Gardiner and Michael A. Mulkey. (Lexington, Mass.: D.C. Heath & Co., 1975. Pp. 211. $14.50.)

Research in the arena of crime has grown geometrically in the last decade. Once almost exclusively the province of sociologists, crime has begun to attract the sustained attention of political scientists, particularly as government agencies such as the Law Enforcement Assistance Administration look for hard-nosed analysts of public policy. The purposes of this volume are to provide an analysis of the program evaluation literature in criminal justice and to speculate about where future research should proceed.

On balance, John Gardiner and Michael Mulkey have done a less than skillful job of selecting and editing contributions (fourteen in all). With few exceptions, the length of selections is too short (median – eight pages), the ideological array is depressingly narrow (neo/pseudo-liberal) and the substantive focus is highly skewed (four essays on police). This substantive imbalance reflects, in part, how LEAA and other organizations have allocated research dollars, but it results also in highly repetitive discussions of the same literature. In contrast, scant attention is paid to the (trial) court system. There is only one selection on prosecutors, one critique of the NAC-CJSG's

Courts, and nothing on grand juries, the administration of bail, sentencing or public defender systems, to name only a few areas where research and field programs have been undertaken. In sum, the framework of criminal justice has been poorly conceptualized.

As with many edited volumes, the quality and originality of these contributions vary widely. By my somewhat crude count, I would rate the proportion of "worthwhile" selections to be slightly less than fifty per cent, and some of these present substantial problems. Two essays are well done – James Q. Wilson ("Crime and the Criminologists," reprinted from *Commentary*), and Wesley Skogan ("Public Policy and Public Evaluations of Criminal Justice System Performance"). Wilson makes a useful distinction between causal and policy analysis of crime by observing that policy planners are constrained politically and logistically from designing programs which go to the roots of crime. Notwithstanding definitional problems of crime which Wilson characteristically evades (crime = street crime), this cautionary warning about the limits of government and its institutions, especially post-Vietnam, seems well founded. It is, nevertheless, subtly ignored in some subsequent chapters. Robert Martinson, for example, after a painstakingly long secondary analysis of attempted correctional reforms, concludes that few, if any, types of programs consistently produce lower recidivism. Yet this result should surprise few (or need twenty-five pages of reiteration), if Wilson's warning is even partially heeded. Rather than dwelling upon inept research designs, Martinson might well have reflected on *why* prison "reforms" apparently do not affect postprison, crime-related behavior and on resultant policy implications. In contrast, Skogan undertakes a concise review of consumer evaluations of police performance with appropriate concern for the varying methodological integrity of the several studies; more importantly, he offers some specific, well-thought-out suggestions for future research [e.g., controlling for citizens' initial *expectations* about performance quality, and differentiating the various tasks which police perform]. With similar opportunities at hand, others in this volume merely toss out at the reader a slew of general, unrelated research questions which someone, one day, might (often ill-advisedly) pursue.

Three other chapters are problematical, primarily in their lack of resolution or full development of ideas. Lief Carter raises the important dilemma (drawn from his excellent empirical study of a prosecutor's office) of the limits both of flexibility (discretion) and its obverse, uniformity (lack of discretion), in criminal justice systems, but goes no further; his policy implications are abstruse. Michael Mulkey provides a concise review of the law and social science literature on plea bargaining, but also fails to go further. One possible advance in plea bargaining research would be to break down the phenomenon by broad offense type, particularly as localized data bases on *felony* plea bargaining are now being developed. The City Bar Association of New York has undertaken one such study related to the impact of legislative restrictions on charge reduction for drug offenses. Finally, Jameson Doig provides a creative interpretation of the historical forces which led to the juvenile justice system, but his policy conclusions [to spend more money on community facilities and/or reduce juvenile justice "overreach"] miss a significant part of the problem. The policy irony of juvenile justice seems to be that while many youths are detained, labeled and possibly incarcerated, without due process, for "offenses" which are not punishable under adult criminal codes, many other juveniles disproportionately contribute to the serious crime rate without having to face the relatively more severe penological consequences which confront adults.

What, then, does this volume add up to? The book is much too expensive to be used in undergraduate teaching and not very well done for graduate seminars. It could have served as a useful reference work for criminal justice researchers and policy planners, but the level at which contributors write and the understandings assumed of their readers perhaps vary too widely. Some assume in-depth familiarity with the literature, while others treat readers as novices to the phenomenon of, say, plea bargaining or white collar crime. In sum, I question whether literature reviewing and (idle) speculation can produce good books in the absence of the presentation of original research findings, innovative theorizing,or solid text writing. The functions and uses of books are not infinite; such things should be better mapped out in advance.

JOHN PAUL RYAN

American Judicature Society, Chicago

Making the Papers: The Access of Resource-Poor Groups to the Metropolitan Press. By Edie N. Goldenberg. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 164. $13.00.)

Lacking the wherewithal to exert direct influence upon public policy decisions, resource-poor groups try to communicate their demands to officials indirectly through the news media. This case study, by a political scientist at the University of Michigan, explores the conditions surrounding group bids for access to the metropolitan press. Specifically the focus is upon four resource-poor groups in Boston – a mass-based organization lobbying on behalf of the elderly, a community improvement organization, a group of the poor on Cape Cod promoting the cause of low-cost housing, and a welfare rights organization trying to improve the lot of mothers with dependent children. The principal targets of these groups are the *Boston Globe* (and *Evening Globe*), the *Herald Traveler,* and the *Record American.* The data were derived in 1971 and 1972 when the author served as a reporter intern and also interviewed 142 persons representing the news media, resource-poor groups, policy makers, and social welfare professionals.

In the growing literature examining the relationship between press and politics there is certainly a place for a book describing the transactions of press, interest groups, and public officials. Moreover, there is justification for narrowing that scope, as does Goldenberg, to the interplay of resource-poor groups and the press. The problem with this particular effort, however, is twofold: (1) the depth of coverage, specific findings, and general conclusions probably do not warrant presentation in book form; and (2) a few key questions hinted at in Goldenberg's discussion scarcely get the attention they deserve.

The author divides the discussion into five parts. The first is a brief but still too detailed airing of the nature of access and the strengths and weaknesses in David Truman's formulation. Person A has access to Person B when the former has a hearing with the latter who has not as yet foreclosed the possibility of granting A's requests. Access is relational, direct or indirect, not always transitive, measured in a variety of ways; it is only a hearing that may or may not lead to a successful outcome and, hence, distinct from influence. Part II lists the group goals and resources that affect decisions to seek access to the press. Part III, the most rewarding, discusses the subtleties and nuances of newspapers as institutions and reporters as individuals that play upon how group leaders perceive and approach the press. Part IV makes explicit a point easily inferred from earlier chapters, i.e., the competition between the media, the plurality of political interests, and prevailing social norms figure in the success that resource-poor groups have in obtaining access. Part V adds such generalizations as "the initial interactions between resource-poor groups and metropolitan newspapers occur as a result of group initiative or newspaper initiative or both" (p. 133), and "the likelihood that a pattern of interaction between a resource-poor group and a metropolitan newspaper will become regularized depends somewhat on how, when and with whom the initial contact between group and institution occurred" (p. 141). Since the author provides detailed summaries at the close of each chapter, the concluding chapter repeating the content of those summaries appears superfluous. The monograph leaves the impression that the raw material for an excellent journal article has been stretched beyond its limits; the didactic and redundant style combined with the publisher's overkill in openness of format, headings, and subheadings makes for an unfortunate outline-like quality.

The principal question raised in this study is what variables affect the relationships between resource-poor groups and newspapers? Credit Goldenberg with a meticulous and seemingly exhaustive listing – individual and group goals, public and private; nine specific group resources; four characteristics of nonmedia targets; a dozen or so identifiable properties of the newspapers; and the perspectives, role conceptions, sensitivities, capabilities, goals, attitudes, resources, and strategies of reporters. Almost obscured by this impressive inventory of variables is a concern that has occupied students of the press and politics at least since Walter Lippmann, i.e., how do political interests and journalists mutually take part in the collective enterprise of construing when a happening is an event, an event is newsworthy, and a newsworthy event is a story? Valuable though Goldenberg's exercise, it is typical of efforts that Herbert Blumer two decades ago labeled and criticized as "variable analysis." It does not answer the vexing questions raised by Lippmann and others. To be sure, an accounting of variables advances understanding so long as we regard the media only as Goldenberg does – i.e., "as an intermediary mechanism between the governors and the governed" (p. 1). But as indicated in the works of Leon Sigal, Gaye Tuchman, Tamotsu Shibutani, Harvey Molotch, and Marilyn Lester, the social construction of political news is an inclusive, adjustive process and not simply mechanistic. *Making the Papers* contributes only marginally to clarifying that total aspect of relations between interest group and the press.

DAN NIMMO

University of Tennessee, Knoxville

Barriers to Establishing Urban Ombudsmen: The Case of Newark. By William B. Gwyn. (Berkeley: Institute of Governmental Studies, University of California, 1974. Pp. 93. $4.50, paper.)

This monograph carefully chronicles the success in authorizing the first municipal ombudsman in New Jersey and explains Newark's failure to fill the position. Originally assigned to assist and assess the operations of that city's prospective ombudsman, Tulane's Professor Gwyn has revised his report to be another study in the Institute's Ombudsman Activities Project submitted to the Office of Economic Opportunity. He examines the movement's development and the reasons for the particular type of office established by ordinance (text included as Appendix II). For complete coverage, the views of principal participants are offered.

Although the earliest proposal is traced to the wake of the 1967 riots, "race was not an important factor in the defeat of the Ombudsman" (p. 24). The detailed account begins with Mayor Hugh Addonizio's rejection of a state commission's recommendation for a Newark civilian review board and his endorsement of a local study urging an ordinance based heavily on Professor Walter Gellhorn's Model Ombudsman Statute. But Addonizio's plan was doomed when Newark could not secure the state legislation necessary to provide the subpoena power that was apparently required for OEO funding.

The bulk of the volume analyzes the efforts of Kenneth Gibson, Newark's first black mayor and a "technocratic moralist" (pp. 28–30), to overcome political attacks on the ombudsman idea resulting from the municipal council's hostility toward his administration. Because Gibson's commitment to the change was initially "tepid" (p. 11), OEO took the initiative in February 1971, proposing a two-year demonstration project. But the federal agency's objection at first that Newark's draft ordinance did not adhere closely enough to the Gellhorn model caused "a delay that may have contributed to the eventual failure of the whole scheme" (p. 13). Obligated by official legal advice to seek council approval, Gibson had to abandon his timetable and appointed the black selected by his search committee to another position.

Even passage of the ordinance (May, 1972) did not signify victory. Five months later, after several postponements of consideration and last-minute attacks by two municipal police organizations, Gibson's candidate – a black lawyer who had not resided in Newark since 1968 – was rejected as a "carpetbagger," 6–2 (one abstention). "That most of the council on this occasion were voting against the office of Ombudsman and not the person nominated to fill it is suggested by arguments recorded in the minutes" (p. 49), the author argues convincingly. Moreover, Gibson "appears to have made a mistake" in refusing to offer another qualified nominee (p. 67).

If establishment of a classical ombudsman and his appointment should depend upon a reforming mayor and a city council of traditional politicians, Gwyn recommends an executive ombudsman, over whom the council has no control.

A reading of Gwyn's well-documented case-study helps one understand the lack of demand in New Jersey for ombudsmen with any real authority. "Action Now," a program established (1971) with Model Cities funds, has been handling citizen complaints in Newark (Appendix I) and elsewhere. Several municipalities now assign such duties to administrators or even to so-called "ombudsmen." But the only significant change has been creation of a state Department of the Public Advocate (1974), including a Division to deal with executive agencies.

HARRIS I. EFFROSS

Rutgers University

Bureaucratic Politics and Foreign Policy. By Morton Halperin, with Priscilla Clapp and Arnold Kanter. (Washington, D.C.: Brookings Institution, 1974. Pp. 340. $8.95, cloth; $3.50, paper.)

Wisdom is the hallmark of this book. Its principal author is a man of impeccable credentials. An in-and-outer, Morton Halperin left Harvard to serve as Deputy Assistant Secretary of Defense responsible for political-military planning in the Johnson administration and, briefly, as a senior National Security Council staff member in the Nixon administration. Clearly he is an experienced and thoughtful participant-observer of bureaucratic politics and this book reflects his background. Using the antiballistic missile (ABM) controversy as a case study, Halperin explores foreign-policy making, emphasizing that the arena is largely closed to those not in the executive branch. This White House-Pentagon twilight world behaves according to the rules of "bureaucratic politics." The book aims at illuminating the policy process of this dark arena for the scrutiny of political scientists and the instruction of potential deputy assistant secretaries.

It is for this exposure of the foreign-policy process that Halperin deserves the most praise.

This topic is little understood because it is little known. While in the past five years Allison, Campbell, Destler, Hoopes, Janis, Rourke, myself, and others have joined Halperin in a growing movement to examine how the bureaucracy makes foreign policy, the quest is still merely exploratory. In comparison to the more accessible domestic-policy arenas, empirical data are sparse. Hence Halperin offers his reader unique insight into the process.

Among his most original insights, the author notes the strong parallels between Presidents Eisenhower and Johnson (pp. 108–109), the well-developed art of leaking information to news reporters (pp. 173–189), and the contrasts between military and civilian officials (pp. 227, 256–258). Contrary to popular myth, generals and admirals are less likely to be "good soldiers" than are their civilian counterparts.

As long as the reader seeks sagacity, the book proves excellent. The problems occur if he seeks more, for the methodology lacks structure, precision, and ties to the relevant literature. Indeed, no systematic overall theory is apparent. While specific generalizations abound, these rules of bureaucratic behavior do not add up to a unified whole – even an arbitrary, heuristic framework. Neither can these generalizations be called hypotheses for they are not falsifiable. The study also lacks a clear definition of its level of analysis. Sometimes the unit of analysis is a major organization such as the army, navy, or air force, sometimes a small Pentagon staff office, sometimes an individual man. Although the book contains frequent and extremely lengthy quotations of memoirs and syntheses of memoirs to provide factual examples, it is not well tied into the theoretical literature of political science. For instance, Chapter 14 considers field-headquarters relations. Although this is one of the classic issues of public administration, not one of the many relevant theoretical works is among the twenty-seven books cited.

If wisdom is sought, Halperin provides it. His book is chock full of perceptive insights tempered by first-hand experience. If systematic analysis is the goal, however, the book is less satisfactory. The study is uneven and disjointed. Indeed the argument presented yields little explanation of the ABM controversy that is supposedly the focus of the work. The concluding chapter unwittingly suggests that Richard Nixon's 1968 election and changing Soviet defense strategy had a greater impact than bureaucratic politics. Perhaps the book is best compared to the many memoirs it cites; in this category it excels for it is an analytical memoir.

David Howard Davis

Cornell University

Policymaking by Plebiscite: School Referenda. By Howard D. Hamilton and Sylvan H. Cohen. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 294. $20.00.)

As the authors point out in their preface, school referenda are the most numerous examples of direct legislation in America, and perhaps the world. The approximately 1,000 school-bond, 2,000 school-budget, and 5,000 tax-levy elections annually exceed the combined number of referenda in city, county, and state governments.

This book, in attempting to inventory and analyze these referenda for both the professional educator and the political scientist, is clearly an ambitious undertaking. It makes a useful contribution in several ways. First, it is an excellent source book on state rules and regulations on school referenda, state referenda outcomes, and local case studies. Second, it presents an extensive review of the literature with an attempt to use the authors' original research to draw conclusions from the disparate findings of referendum studies. Finally, it offers an interesting methodological note in explaining the use of Q-factor analysis to uncover voter profiles in actual election situations.

The review of the literature and the campaign histories include some novel and intelligent conclusions regarding referenda campaigns and outcomes. The authors, both political science professors, rightly conclude that, contrary to popular academic belief, a large turnout will be associated with passage of a referendum and a low turnout with defeat in most urban areas. This happens because a low turnout electorate is skewed by an inordinate proportion of retirees and other persons opposed to taxation, while a high turnout electorate will include more members of minority groups that typically vote yes. One of the interesting findings from their own research in Austintown and Bowling Green (Ohio) is that there is a substantial amount of economic rationality in voting. They found that the less favorable an individual's tax/income ratio, the most likely he or she was to vote no.

The problem with the campaign histories and the review of the literature, however, is that it is somewhat artificially divided into seven chapters: The Decisions of the People: Voting Rates, Approval Rates, and Determinants; School Levy Elections: Strategies and

Campaigns; Bond Elections: Issues and Strategies; Communications, Knowledge, and Influence; School Referenda and Community Conflict; Voting Patterns – The Correlates; and The Voter's Decision-Making Process. As might be expected from this listing, there is both a good deal of overlap between the chapters and a lack of integration. Indeed, no real attempt is made to determine the dynamics of the voter's decision-making process. The chapter of that name is simply another review of the literature on the correlates of voting. Nor is much attempt made to integrate these chapters with the following chapter on voter profiles (obtained from Q-factor analysis) in order to come to some sort of conclusion about the decision-making process.

The chapter on Q-factor analysis is perhaps one of the more interesting in the book. Q-factor analysis involves having a set of subjects rank statements (the Q sample) about an event, issue, person, etc. The rankings by each subject are correlated with the rankings of each other subject and the resulting matrix of correlation coefficients is factor analyzed to obtain clusters of persons who ranked the statements similarly. The authors obtained four attitudinal factors in their analysis of the Youngstown tax levy controversy. These included (1) the "good citizens" who identified with the school establishment; (2) the "alienates" who were parochial, suspicious of authority, and populist in their orientation (also the only group opposed to integration of the schools); (3) an ambiguous or cautious group; and (4) the "caretakers" who favored maintenance of only traditional services because of their intense concern for economizing.

The authors proceeded from the Q-factor analysis of "how" the attitudes were sorted to a survey research concern for the frequency distribution of these factors in a given electorate. They analyzed an election in Austintown, Ohio, and found a strong relationship between the scaled attitudes and the direction of the vote. In Austintown, knowing whether a respondent was a "good citizen," "caretaker," or an alienated individual was more valuable information for determining the direction of the vote than were the typical demographic variables employed by most voting research. Because this book is also directed at professional educators, the authors recommended Q-factor analysis as an "intelligence" tool to guide campaign strategy in a specific election situation.

Despite problems in writing style and organization, this book represents a useful and needed addition to the rather sparse field of referendum research. While it is flawed in the coherence of the theory presented, the authors must be given credit for bringing together in one book, the vast body of state rules and local campaign histories, and the academic voting behavior research.

CHRISTINE H. ROSSELL

Boston University

The Presidential Campaign: The Leadership Selection Process after Watergate. An Essay. by Stephen Hess. (Washington, D.C.: The Brookings Institution, 1974. Pp. 121. $2.50, paper.)

Stephen Hess, a political scientist and a veteran of presidential campaigns, has undertaken a critical reflection on our presidential selection system. His essay focuses primarily on two questions: how well does this system work, and how may political parties reestablish their control over it? To this task, he brings not only first-rate credentials, but a cool, dispassionate perspective, an incisive, analytical approach, and a willingness to stick his neck out in making judgments and offering reform proposals.

Hess concludes that on the whole the selection system works well. He is hardheaded enough to characterize presidential campaigns as vulgar, costly, wasteful, chaotic, lacking in creativity and intellectual vigor, and failing to entertain. Nevertheless, he adds, they do expose the candidates adequately to the people and the people, in turn, to the candidates. Do the campaigns test sufficiently the key qualities – personal, political, and executive – that he finds to be essential for a president? He concludes that the first two are adequately tested while the latter is not. Since, however, Mr. Hess regards character and "political instinct" as much more important for the voter to ascertain and for a president to possess, the failure to test for executive qualities is no great indictment of the selection system.

Unlike those who criticize campaigns as excessively long and too burdensome for the candidates and the electorate, Hess asserts the functionality of the campaign as an ordeal. Campaigns, he contends, serve as both testing and training grounds, enabling the electorate to learn about the candidates and affording the latter useful lessons about the country and its people; the length of campaigning only enhances such opportunities. At the same time, campaigns bring forth new policy initiatives and open up to greater scrutiny policy issues already in the public sector.

Hess is critical, however, of the system's ability to produce enough potential leaders. He would widen the pool of talent by expanding the number and types of professional politicians – and nonpoliticians – under consideration. Thus he offers an imaginative proposal: having special groups of politicians – mayors, governors, and Congressmen – as well as associations of businessmen, educators, and labor leaders jointly serve to nominate candidates to be considered by the national convention. Unfortunately, Hess fails to explore the nature and problems inherent in such innovations.

Hess's second theme is the value of more disciplined political parties and the need for their regaining control over the presidential campaign. Watergate, in Hess's eyes, was partly attributable to the divorce of the presidential nominating-election process from both political party and professional politician. Hess proposes to magnify the roles and importance of parties and professional politicians at the expense of the individual candidates, the mass media, the money-givers, and the public relations firms. Parties do have it within their power, he asserts, to regain control of the presidential campaign process.

To attain these ends, Hess calls for strengthening the parties' national committees and conventions. He favors a mixed public-private financing system in which the national committees receive and control the funds in presidential campaigns. He recommends that the national committees also propose a code of ethics and conduct, binding on the candidates for the nomination and election process and backed by the ultimate sanction – denial of the presidential campaigns. He recommends that the national committees also propose a code of ethics and conduct, binding on the candidates for the nomination and election process and backed by the ultimate sanction – denial of the presidential nomination! Bi-annual party conferences should examine the state of health of their parties and should prepare platforms for the upcoming presidential campaigns. The choice of the vice-presidential candidate should reside primarily in the national committees which should also be empowered by law to respond in an election year to television-radio addresses by an incumbent president of the opposite party.

Readers of Hess's essay may find some difficulty in reconciling the author's conclusions that the present selection process works rather well with his strong insistence on the need to reestablish party control. Moreover, some of his proposals seem to raise more questions than they answer. On the whole, however, Hess's provocative, well-written essay on the presidential selection system deserves serious consideration.

ABRAHAM HOLTZMAN

North Carolina State University

Aspects of Early New York Society and Politics. Edited by Jacob Judd and Irwin H. Polishook. (Tarrytown, New York: Sleepy Hollow Restoration, 1974. Pp. 150. $7.50.)

This brief volume is the consequence of a symposium held in October 1971 under the sponsorship of Sleepy Hollow Restorations. Aside from dealing with New York politics from 1689 to 1789, no common theme or methodological approach ties the different essays of this book together. The book is also unfocused in form as well as content, consisting of two perfunctory comments by the editors, four formal papers and two critiques, and an address by Richard B. Morris entitled "The American Revolution Comes to John Jay."

In a highly derivative but well-written essay, "New York in the American Colonies: A New Look," Milton H. Klein argues that colonial scholars have too exclusively emphasized developments in New England and the South and have not looked carefully enough at the Middle Colonies, especially New York, which, because of its heterogeneous character, presaged the kind of country America was to become. The point is well taken. Yet in pursuing it Klein tends to belabor the obvious, to superficially treat complex issues, and to never confront the meaningful and challenging historical problem of how a society whose intellectual heritage was community and consensus adjusted to and channeled its enormous diversity.

Patricia Bonomi's "Local Government in Colonial New York: A Base for Republicanism," is a knowledgeable survey of local institutions with particular emphasis on developments in Kingston before the Revolution. Briefly comparing the way local government operated in New York with that of England, Professor Bonomi finds the former to have been more vigorous and broadly based and takes issue with those historians who have stressed too exclusively the extreme aristocratic nature of politics in colonial New York. While there is much of value in what she says, I cannot help but feel she has pushed a useful point too far by downplaying or ignoring the role of the judges of the county courts who held their positions by appointment, the dominance of the Whig theory of government and the role of deference, as well as the uneven distribution of wealth, status, and education – all of which in important ways affected how power was actually accumulated and used. This essay will undoubtedly add additional fuel to the already heated debate among historians over how much and what kind of democracy existed in colonial America.

In "The Age of Leisler – New York City, 1689–1710: A Social and Demographic Interpretation," Thomas J. Archdeacon uses a computer to analyze tax assessment roles and an early census of Manhattan. He argues that the Leislerian movement is to be explained mainly in terms of the resentments of the city's Dutch population over rapidly increasing English influence. Although Archdeacon's evidence clearly does seem to indicate "an unmistakable Dutch aura" among Leisler's followers, the significance of this for the movement is asserted rather than proved, while the economic sources of the conflict tend not to be sufficiently explored. Still, to those scholars who see enough conflict in early American history to reject consensus synthesis, but who do not find the class-conflict orientation of the Progressive historians acceptable, and who have therefore stressed the importance of religious and ethnic differences, this essay will prove a useful reference point.

A brief study by Edwin G. Burrows on "Military Experience and the Origins of Federalism and Antifederalism," uses commonsense arithmetic rather than sophisticated statistical analysis to examine the military careers of the fifty-four Federalists and forty-nine Antifederalists who were candidates for the New York Ratifying Convention. Burrows finds a strong correlation between Federalism and service in the Continental Army on the one hand and Antifederalism and the state militia on the other, and argues that the militia experience bred a localist political outlook while service in the Continental Army bred a more cosmopolitan outlook, and in the context of the late eighteenth-century America, a more nationalist spirit. This interpretation has already been stated by a number of scholars. Moreover, Burrows never confronts the sticky question of what effect a localist and cosmopolitan attitude had in predetermining the military choices of individuals, thus reversing cause and effect. Nonetheless this is a useful essay for it strongly supports what seems to rapidly be becoming the dominant way of interpreting the struggle over the ratification of the United States Constitution.

Bonomi and Archdeacon's essays are particularly valuable. One can only feel they would have been better served as journal articles where they would have attracted the immediate attention they deserve, rather than being buried in a book where they will have to wait to be discovered.

RICHARD E. ELLIS

State University of New York at Buffalo

Bureaucratic Encounters: A Pilot Study in the Evaluation of Government Services. By Daniel Katz, Barbara A. Gutek, Robert L. Kahn and Eugenia Barton. (Ann Arbor: Survey Research Center, Institute for Social Research, The University of Michigan, 1975. Pp. 264. $12.50, cloth; $7.00, paper.)

As a government expands its activities of service and regulation, it expands the number of occasions for contact with its citizens about practical matters. Such activities as job training, medical aid, and driver licensing, important enough to justify government involvement, are important to citizens as well. From a citizen's standpoint, contact with a government bureaucracy is likely to be an important part of his experience of government overall. The authors of *Bureaucratic Encounters* believe that a study of citizens' dealings with public bureaucracies is important in determining: (1) the utilization of government services, (2) the quality of experiences with government agencies, and (3) public support for the political system.

The study focuses on Americans' experiences in seven areas of government service and constraint. The service areas are job finding, job training, medical aid, retirement aid, unemployment compensation, workmen's compensation, and public assistance; the constraint areas are taxation, driver licensing, traffic violations, and the police. Public experience with these government activities was gathered in spring 1973 as part of the Institute for Social Research's semiannual national Omnibus survey.

The study's questionnaire (reproduced in the Appendix) was extensive, and enumeration of the study's findings on utilization and experience alone takes the reader serially and unremittingly through ninety-seven pages. This long enumeration, however, contains a number of findings which challenge familiar stereotypes. Among the most interesting are data about the characteristics of users of the seven government services. Contrary, for example, to the "popular notion" (p. 26) that needy blacks prefer not to work is the finding that "twice the proportion of blacks as whites utilize public employment agencies, and twice the proportion seek job training. . ." (p. 26). Nor did the study find support for the notion that minority groups and the poor are generally the heaviest utilizers of the services surveyed. Such characteristics as age, race, level of education, income, and occupation relate to differences in utilization of particular services, but service utilization overall is surprisingly generalized across these categories. These sorts of findings led the authors to assert that governmental services in the seven areas ". . . do not comprise a welfare state for the deprived and needy; they are the way of life for the whole nation" (p. 38).

The findings on experiences with government agencies contradict other popular notions. The respondents, for example, did not find service agencies to be slow and muddling in handling their cases, nor did their experiences reflect the stereotyped image of faceless, alien bureaucracies. Other noteworthy findings relate to perceived fairness of treatment and awareness of appeal procedures; interesting as well are the relatively unfavorable differences in findings for constraint agencies.

While, for example, almost 60 per cent of the service agency users "felt that their situations had been improved or their problems resolved by the agency they visited" (p. 115), nearly half of the respondents with experience with constraint agencies believed that their problems were worsened by the agencies' handling of them. There was a similar difference between service and constraint agencies in perceived fairness of treatment.

Most engaging, however, is the study's discussion of the nature of public support for the political system. In reference to David Easton's three levels of system support, the authors assert that it is necessary, in gauging the actual degree of support, to define indices which embrace the "actual cognitive structures" of system members (p. 140). The terms of theory must be defined in reference to measurable reality. Accordingly, the authors through factor analysis of survey responses arrived at nine attitude dimensions of system support. Based on both experience and general beliefs, the attitude dimensions represent sentiments of attachment to the political system, and include interpersonal trust, confidence in national leadership, symbolic nationalism, and evaluation of personal experience with government bureaucracy. The survey respondents were then related to the factors by age, race, income, occupation, sex, education, relative deprivation, and working status. There is not strong support in all dimensions from all categories of respondents, but lack of support is discontinuous. The political system appears by and large to be supported by various groups for various reasons. To be truly disaffected, one must be alienated multidimensionally.

Bureaucratic Encounters provides an outside-in view of governmental functioning in areas affecting the lives of ordinary people, and illuminates a common but important area of political experience. The quality of that experience, in contrast to all that has been written recently about the alienating effects of modern bureaucratic society, is surprisingly positive. Most important, however, *Bureaucratic Encounters* adds needed clarity and attitudinal substance to the study of system support. It is now possible to consider this fundamental political attribute in concrete, human terms.

DANIEL MADAR

Brock University, Ontario

The Domestic Presidency: Decision-Making in the White House. By John H. Kessel. (North Scituate, Mass.: Duxbury Press, 1975. Pp. 149. $7.95.)

This research monograph seeks to describe how domestic policy is formulated in the White House. The author observes quite correctly that more is known about congressional decision making than about White House decision making and sets out to provide some remedy. Wisely, he restricts the focus of investigation in several ways. Time: Only the Nixon White House. Policy area: Only domestic policy. Focus: Only the workings of the Domestic Council. What we have, then, is a case study, rather than a comprehensive and systematic inquiry. Yet this is a good way to proceed when not much prior work has been done.

The great danger in this approach is that the monograph easily degenerates into story-telling. Neustadt's *Presidential Power* is a case in point. Anecdote is piled upon anecdote, completely smothering whatever systematic theoretical formulations the work once contained. Fortunately, in Kessel, theoretical considerations and interests predominate over anecdotal components. Kessel can hold in check the anecdotal element because he approaches the subject matter not as a historian-journalist, but as a social scientist, with a theoretical orientation, explicit definitions, and specified hypotheses. He has an explicit methodology, and an awareness of the basic rules of evidence and inference. On these grounds alone, the book is a most welcome change from the more customary literature.

The author's conceptual apparatus is derived from group theory. The units of analysis are the staff members of the Domestic Council, but the level of the analysis is the group whose members share attitudes and have "a reasonably stabilized pattern of interaction. . . ." Kessel goes on (p. 28) to state that ". . . our interest in more inclusive organizational units . . . lies in how the larger units constrained and delimited the activities of the Domestic Council staff."

The author's technical tools include depth interviews, scale construction, and correlational analysis, all of which appear to have been competently used. The interviews were conducted in 1972 with twenty members of the Domestic Council, up to and including Kenneth Cole, then Deputy Director under John Erlichman.

This book opens with a discussion of the history of executive staff organizations generally, and of the Domestic Council specifically. This discussion is placed into the context of the various factors (e.g., presidential character, power situation, calendar, etc.) which affect the president's effectiveness and the behavior of the President's administration.

Next the author examines the recruitment process (unsystematic) and background (highly educated, but otherwise fairly diverse) of the Domestic Council staff, the policy attitudes of the staff members, the communications patterns, and the power patterns. He finds (1) that fairly high levels of policy consensus existed on most policy issues (particularly economic issues); (2) that the leaders occupied the important communication positons; and (3) that the power patterns in the main followed the formal hierarchical channels (legitimacy) but that expertise also served as a power base.

A chapter entitled "The Political View from the White House" deals with the perceptions of the Domestic Council staff members of their tasks, and with the staff's perception of and relationships with such significant political entities as the president, voters, cabinet members, congressmen, party leaders, and lobbyists. Most interesting among the many findings are these two:

(1) The staff quite uniformly divided the political environment into three broad classes according to their relative importance: foremost, the president; second, voters, cabinet members, and staff colleagues; and only third, party leaders and interest group spokesmen.

(2) The staff members' perceptions of the president's policy position were fairly uniform and were similar in location to their own policy positions; and the staff members' perceptions of the public's policy preferences were generally accurate, except on the issue of "governmental activism," where they underestimated the public's willingness to use government authority and resources to achieve desired ends.

The next chapter deals with the "Activities of the Council." It contains detailed discussions of the Council's major functions: clarifying options, determining priorities, backstopping the president, collecting information, and monitoring the agencies. The example of the development of revenue sharing is used to illustrate these functions. At its creation the Domestic Council was given an exceedingly broad mandate: "to serve every policy need from the first glimmer of a social problem through an exhaustive evaluation of the ultimate government response" (p. 81). This mandate, of course, was well beyond the capacity of a staff of twenty-eight persons; the activities just listed had emerged as the major functions by the end of 1972.

In the last chapter, Kessel assesses the effectiveness of the Domestic Council in the period from its inception in July 1970 to the end of President Nixon's first term. This rather limited time frame was dictated, in the first instance, by the author's research calendar. But it was also determined by the Watergate-related resignations and shifts of personnel. The activities and effectiveness of the Domestic Council of President Nixon in his second term and of President Ford will require new investigations.

The effectiveness of the Domestic Council in its first two and one-half years is assessed from three perspectives: the needs of Richard Nixon, the needs of the voters, and the needs of the presidency. Kessel concludes that the Council served very well the needs Nixon had for a vast, but orderly array of facts, presented in written form, with decisions made in physical isolation. As a result, the Council "became one of the most important elements in the Nixon White House" (p. 114). Kessel also finds that the Council staff was fairly well attuned to voters' needs to have their policy positions understood and taken into account. The needs of the presidency seem to have been served better by the Council's staff than by other White House agencies. The Domestic Council staff seems to have been least affected by the generally prevailing paranoia of the Nixon White House, and was less ready than others to regard political opponents as enemies.

The Domestic Presidency is a small and unpretentious yet insightful book of considerable importance. It can be recommended not only to students of the American presidency but also to everyone who has an interest in political organization.

PETER W. SPERLICH
University of California, Berkeley

The Washington Lobbyists for Higher Education. By Lauriston R. King. (Lexington, Mass.: D.C. Heath & Co., 1975. Pp. ix, 127. $11.00.)

This is the most useful book yet to appear on Washington representation by higher education interest groups. Its ostensible purpose is not to analyze conclusively the pattern of relationships between higher education representatives and political decision makers, nor to contribute toward a theory of policy making or policy systems. Rather, the author intends simply to discuss the evolution of the Washington representatives from a barely visible presence in the late 1950s to a self-consciously political presence today. In the course of developing his thesis, Professor King deals incidentally with contextual forces, and refers briefly to the more salient aspects of the changing interrelationships between Dupont Circle and Capitol Hill.

The conceptual or theoretical approach of the book is to treat higher education as a "developing interest group," although there is insufficient attention to that notion to make a significant contribution to systematic analysis in political science. The historical context for the analytical portion of the book is thoughtfully and succinctly developed, with a demonstrated acquaintance with the important literature in the field.

The greatest virtue of the work, however, and the major intellectual stimulus to the reader, is the Section (Chapters 4, 5 and 6) which addresses important political changes on the part of the representative groups. King discusses what he names the "traditional," "pragmatic," and "activist" perspectives of the Washington representatives, with a focus on evolution from the first to the last.

The traditional perspective "holds that advanced learning has an intrinsic value for the cultural and intellectual life of society, and as such should compel the support of the major institutions, including government, of the society in which advanced education takes place" (p. 65). This perspective, says King, places higher education apart from other social and political interests, and manifests a pattern of "snobbish elitism." Higher education's occasional involvement in national politics, therefore, must eschew the "tedious work" of mobilizing support and engineering compromise in order to maintain professional dignity and transcend the "narrow concerns of self-interest." According to the author, the key historical event which weakened this perspective was the mobilization of the higher education community behind enactment of the Higher Education Facilities Act of 1963.

The pragmatic perspective stresses the tactical need for forming and maintaining coalitions of higher education groups in order to speak in one voice to policy makers, but retains the view that when higher education becomes politically involved "it should do so in a rational rather than an adversary manner" (p. 71).

Finally, the activist perspective sees higher education as standing "in competition with other interests equally worthy of support" (p. 79) and therefore in need of more forceful and conventional lobby representation. This perspective is held by a small minority within the higher education community.

Dr. King focuses on the critical presidential transition, from Logan Wilson to Roger Heyns, at the American Council on Education, as a turning point in the triumph of the pragmatic over the traditional perspective at One Dupont Circle. I believe that he is too harsh, however, in his assessment of the Council's political leadership under Wilson. His discussion of the Council and its relationships with the more narrowly based groups is essentially accurate, but lends itself to much more theoretical treatment in terms of policy making and authority patterns between an interest-aggregating organization and a variety of interest-articulating groups within a developing policy system.

This book, in summary, is well researched, well written, and cogently argued. It should have pedagogical value that far transcends its focus on higher education.

LAWRENCE K. PETTIT

Montana University System

Pollution, Prices and Public Policy. By Allen V. Kneese and Charles L. Schultze. (Washington, D.C.: The Brookings Institution, 1975. Pp. 125. $6.95, cloth; $2.95, paper.)

Pollution, Prices and Public Policy is another, but less technical endeavor in a growing list of works concerned with the merits of using the "market or pricing" approach as an alternative to the "centralized bureaucratic" approach in controlling environmental pollution. In part, this book by two economists, Professors Kneese and Schultze, is a summary and synthesis of previous work in the field. More significantly, it analyzes new material on the limitations of past and current federal legislation in water and air pollution control, legislation that relies on the bureaucratic approach, and then sets out to show in a systematic and logical fashion the advantages of the pricing approach over the bureaucratic technique.

Starting with the point that the current legislation in pollution control is not cost effective (and that this is particularly defeating in troubled economic times), the authors state the theme of the book – national pollution control programs can be designed to reduce substantially the cost we are now paying to clean the environment. According to the authors, this is not being achieved. The current bureaucratic approach which relies on national ambient and emission standards and which is administered and enforced by elaborate implementation plans, extensive monitoring and testing, fines, court proceedings and large subsidies is simply not working effectively. The main reason for the failure of this federal legislation is that the pollution problem, as embodied in thousands of individual pollution sources, is too technical, too diversified, and too diffuse to be handled by a centralized bureaucratic method. Additionally, since only part of the cost of control is ever recorded in governmental budgets, with the burden of the cost being borne by the polluter and the consumer, control goals are not tempered by full cost and are likely to be overzealous.

What the authors recommend instead is regulation by the pricing system, or more specifically, effluent charges. They argue that if we put a price on air and water, just as prices are placed on other valuable resources, consumers will use them more carefully. This approach, by their logic, places the onus of the technological decisions where these decisions can best and most efficiently be handled, with the firm or the operator, leaving the responsibility for policy decisions, that is, how much control we want, with our representative institutions.

A number of previous studies on the cost effectiveness of effluent charges are cited as evidence for the utility of the market approach (although it is not clear whether these studies are based on actual institution of effluent charges or if they are premised on simulated data). Also, several specific suggestions for administering the effluent charge approach and for achieving its political acceptance are offered. For example, to administer it, the authors suggest a minimum national charge, plus – in the case of water control – regional systems for coordination of water resources within watersheds. To reduce the political and cultural barriers that stand in the way of adopting a market solution in the environmental area, the authors recommend that congressional staffs include people with the technical background that will enable them to analyze better these pricing approaches. Furthermore, they see the need for educating political representatives on alternatives to our timeworn bureaucratic solutions. Finally, they call for consideration of the pricing alternative in other policy areas, such as health insurance and energy policy, contending that the complexities of these areas are equal to those of the environmental area and thus not suited to the bureaucratic technique.

The arguments for the pricing alternative are worth close scrutiny if only to force more critical thought about government's role as problem solver. The arguments are also worthy of examination because the logic and evidence offered by Kneese and Schultze show that current approaches are inadequate to solve complex, diffused problems such as environmental pollution. There appears, nevertheless, to be an underappraisal of the bureaucratic approach and overappraisal of the pricing approach. First, the federal government's regulation of both water and air pollution has no doubt been flawed, but in many locales where water and air quality have been measured, improvement is evident. While Kneese and Schultze concede this improvement, they contend that it was achieved at too great a dollar cost and that fundamental changes in industrial processes or transportation modes were never encouraged. However, it must be remembered that these improvements are actual reductions accomplished in many regions of the country, not the simulated or localized achievements broached as evidence for the pricing system. Moreover, in actual nationwide programs, the slack and uncertainty that accompany program implementation are very likely to inflate cost.

A second point that needs emphasizing is that the pricing approach shares many of the same problems the current system has. There is still the political problem of setting charges or standards and the administrative problems of developing, testing, and implementing effluent measuring devices. In fact, it may be politically more difficult to accomplish this under an effluent system since polluters are actually assessed a dollar charge; whereas polluters may not have to pay under the standards approach unless convicted or put under court order.

A third important consideration in the assessment of the pricing method is that there is no necessary one-to-one relationship between effluent charges and pollution reduction. Firms could pay the charge and reduce costs elsewhere to make up for the loss; and given the problems of designing and maintaining control equipment, firms might readily look elsewhere in their cost structure. Furthermore, it is possible to transfer the effluent charges to the consumer, a strong possibility in oligopolistic markets.

In short, some of the problems associated with applying the pricing method do not receive adequate attention. Nonetheless, the market approach is one that deserves more study and in fact is already receiving attention through experiments in housing allowances and income maintenance. The book's rather critical treatment of current methods of massive government intervention coupled with its cogent presentation of the market alternative should stimulate greater discussion among scholars, policy makers, administrators, and citizens; and it is hoped that it will stimulate a more critical analysis of these alternative problem-solving approaches.

JOHN F. SACCO

University of South Carolina

Harry Hopkins and the New Deal. By Paul A. Kurzman. (Fairlawn, N.J.: R. E. Burdick, Inc., Publishers, 1974. Pp. 219. $9.75.)

Professor Kurzman states quite accurately that his "study makes no pretense at being an exhaustive study of Harry Hopkins in the 1930s." He passes lightly over Hopkins's early life and then in a series of sketches traces his development from a case worker in a private welfare agency to the head of the primary relief agencies of the New Deal. This description includes references to his administration of a division of the Red Cross and his directorship of the New York Tuberculosis and a Health Association as well as his directorship of the New York Temporary Emergency Relief Administration when Franklin D. Roosevelt was governor.

The significance of these activities in his conduct of the New Deal relief agencies is detailed. The first agency, the Federal Emergency Relief Administration functioned well because he assumed that the quickest way to the millions of needy people was streamlined federal administration using existing governmental machinery. In the succeeding agency, the Civil Works Administration and the Works Progress Administration, direct federal administration was used. The policy shifted from direct relief to work relief to work adjusted to the individual skills of people on relief.

Professor Kurzman shows special interest in the impact on social work of Hopkins's handling of these varied activities. He used many social workers along with others. Historically social workers had been case workers and administrators raising money for private welfare agencies. In the relief agencies they were called upon to take heavy responsibilities and learned the administrative skills associated with the federal civil service. From the New Deal on social workers in their many varieties of case work and many facets of public welfare administration have grown in numbers. Hopkins is given credit for having established the high degree of respectability which the profession now has. As a result of his vigorous administration of relief Hopkins is also given credit for breaking down the barriers among the federal, state and local governments.

The study notes that Hopkins's influence with Roosevelt went far beyond the relief field. In arguing for relief funds Hopkins is given credit for pursuading Roosevelt to adopt a policy of "deficit financing." He is given equally broad credit for his participation in the activities which resulted in the social insurances. It is noted he was unable to pursuade his colleagues in the administration of the wisdom of trying for health insurance at the time old age insurance was adopted.

Two other significant influences are described. In the struggle with Secretary Ickes over work relief as contrasted with funds for public works built by private contractors Roosevelt was pursuaded to take funds originally earmarked for Ickes and shift them to work relief. The second major shift in policy was his acceptance of the necessity for partisan politics to become significant in the Works Progress Administration.

There are a few words about Hopkins's desire to become president.

One is left with the feeling of having been let down when he has finished the book. These years were rich in the development of American social change and politics were closely intertwined. Hopkins was a major participant. Professor Kurzman has noted some of Hopkins's creative activities. But there is little of his fights and friendships with a crowd who believed in what they were trying to do. The New Deal hardly appears. Hopkins was one of some hundreds who made that era what it was. But Hopkins was a good deal more than just one of them.

THOMAS C. BLAISDELL, JR.

University of California, Berkeley

As Orange Goes: Twelve California Families and the Future of American Politics. By Karl Lamb. (New York: W. W. Norton, 1974. Pp. 322. $8.95.)

Karl Lamb's book celebrates what he considers the "remarkable diversity" of political views found through intensive interviewing of twenty-three Orange County voters, all living within a few doors of each other in the affluent suburban housing tract of Ramona Heights. They have climbed the social ladder in different ways; they share common residential preferences, but their life styles show the persistent influence of ethnic, class, and regional origins. Their party loyalties are divergent and attenuated; their beliefs predictably are negative – politics is a dirty business, politicians cannot be trusted – and Watergate, about which they were asked a year after the main interviews, elicited some feelings of betrayal, a few increments of activism, and much fascination. Still, in his closing chapters, Lamb finds in them a "yearning for integrity." They are ready for positive leadership – to restore balance in government, to foster individual achievement, to safeguard the environment. A future "coalition of hope" will surely include suburban voters like these.

So much for the cumulative thrust of Professor Lamb's inquiry. Twelve families make up his "microsample," as he calls it. No sample design is made explicit, nor does one find an interview schedule, although from time to time the topical focus of conversation is mentioned, and Lamb notes that he has based his own guide on the one used in *Political Ideology* by Robert Lane. The respondents seem to have been chosen in part, if one can judge by a recurrent theme, to show that the denizens of affluent suburbs are not necessarily rightist authoritarians. Orange County is well known as a "bastion of right-wing conservatism"; those in thrall to the ecological fallacy might expect any selection of its residents to be reactionaries, however well-educated, well-off and cooperative respondents they might be. Not so, we learn. "These voters are not strangers to decent impulses, nor to human sympathy. Such a statement only seems remarkable . . . [given] . . . the assumption of a political scientist like Walter Dean Burnham that repressive fascism will be supported by suburbanites as a matter of course" (p. 303).

Various considerations might well justify study of a small number of respondents intensively. Systematic and extensive studies of American electoral behavior have amply shown what behavioral science can contribute. Surely something can be added by a phenomenological approach. The "human scale" has been neglected. Political inquiries call for empathy if social reality from the perspective of those experiencing it is to be fully appreciated. Reducing the comments of human beings to numerical values is a suspect research method to Lamb. Doesn't it somehow violate their humanity? His own work, then, is not a study in quantitative social science whose findings rest on probabilities. Rather, he aims to present a few voters as they really are – flesh and blood, not skin and bones. His subjects have "intrinsic interest," he hopes. His readers are to judge for themselves whether his word-portraits resonate with their own experience.

It is true that qualitative insights are not automatically gained by statistical summaries of cross-sectional responses to precoded questionnaires. Neither, however, does Lamb's assurance that attention is confined to a "microsample" guarantee that discoveries of more than local and limited interest will emerge. The new phenomenology also, in its search for valid generalizations, must find commonalities and tell us what they are. Tracing the vagaries of opinion and belief to be found in the talk of a few individuals is not much help for disclosing the modalities of American political orientation. Nor does the outer appearance of a man or woman, posing for a closeup portrait, reveal much about the psychological dynamics of his opinion formation, or about her readiness for political action. The structuring vision of first-rate novelists like Dostoyevsky or Conrad or Proust can make their characters real for most of us. But what Beatrice Webb called "truth-telling fiction" often becomes flat and one-dimensional, even if the author is Dickens or Trollope. Political scientists who have worked with a few people studied in depth have been most successful when they were explicit and cogent about their own structuring vision and used their evidence to exemplify its empirical content. They have not let the subjects speak for themselves, nor have they introduced a sequence of straw-man arguments, stereotypes, or simplistic commentaries to be shown up as unable to account for even a few "real" people's views. Although Lamb's respondents are well known to him after many hours of informal conversation, his efforts to recreate them for us do not come off too well. He lacks the evocative power of Lane or Riesman, although he writes with clarity and conciseness. He is not very careful about definitions (on page 191 privatism includes localism, but four pages later it is out) nor about checking his own evidence (compare what Marie and Arnold think on pages 142–143 with Lamb's report on page 175 that they don't feel poor people somehow deserve their fate). He sometimes resorts to quantitative summaries, for purpose left unclear: "Ten of the respondents, or nearly half of the microsample, express intense pride in their American citizenship. . . . Six are immigrants or the children of immigrants. . ." (p. 168). We are not told how many of the other thirteen are second generation.

Lamb's cases, oddly enough, are presented with scarcely any history. Their topographical origins are sketchily and unsystematically noted; the paths by which they reached their present homes are only occasionally and partly charted. No processes of political socialization are described. "Party identification is absorbed from one's parents and other aspects of the childhood environment," he tells us, seemingly unaware of the spate of inquiries into this uncertain connection. For none of his people does he tell us much more. One is said to have become a Democrat to rebel against his Republican precinct-worker mother, but this is explored no further. Nor do we learn of the coming generation. Their offspring seem to have no aunts or uncles, no grandparents. Despite the ethnic diversity in Lamb's microsample, his discussion of privatism does not touch at all on the possibilities of differential "other-regardingness" raised by Wilson and Banfield. Lamb does turn to the classic studies for ideas. *The American Voter* is source for the notion that "party identification may be a more powerful psychological force than the voter admits, even to himself." With V. O. Key, he holds that "voters choose, stay with, or switch to parties nearest their attitudes or convictions." From his own work, Lamb finds that people have "opinion molecules" rather than consistent views:

> The riots at the 1968 Democratic convention *had* to be supported by Russia and China; but it's a mistake to blame all America's ills on the Communists. . . . International bankers obviously profit from wars, but this phenomenon is not related to the support given by American Jews to Israel. At one moment, senile southern senators are identified as "running the country"; at the next, international bankers rule the world. The Marcuses are not hampered by a requirement of logical consistency" (p. 219).

Neither here nor elsewhere do we learn what logic unifies the political world for Karl Lamb. Presumably it is superior to the Marcuses'.

For the next year or so, this study will help to provide undergraduates with up-to-date illustrations for propositions and insights housed in the now classic studies of American opinion and voting behavior. Of course, those classics included a lot of illustrative quotations also – and they were not badly chosen.

ELIZABETH WIRTH MARVICK
DWAINE MARVICK

Los Angeles

Phase II in Review: The Price Commission Experience. By Robert F. Lanzillotti, Mary T. Hamilton, and R. Blaine Roberts. (Washington, D.C.: The Brookings Institution, 1975. Pp. 209. $9.95, cloth; $2.95, paper.)

Conventional wisdom has it that governmental price and wage controls don't work, or else they do work and are a bad thing. Nevertheless, the federal government periodically has felt compelled to supplement fiscal and monetary policies with direct action against inflationary wage and price changes, sometimes by exhortation and threat, sometimes by legal regulation. Academic critics, content that such steps were meaningless or unwise, rarely have bothered to analyze alternative regulatory standards for price and wage controls, or to discuss the political ramifications of the institutional arrangements that have been or might be utilized by government controllers. The Brookings Institution's current series of Studies in Wage-Price Policy, however, has begun to address these problems, apparently on the assumption that some sort of incomes policy will inevitably be with us. The series includes the excellent *Exhortation and Controls* (Craufurd Goodwin, ed.), comparing the wage-price policies of each national administration since 1945, and separate volumes on each "Phase" of the Nixon Administration's program. In *Phase II in Review,* Lanzillotti, Hamilton, and Roberts, economists who served on the Price Commission, focus on the period (Nov. 1971–Jan. 1973) which saw the most serious effort to develop and enforce an effective regulatory regime. They set out to describe how federal price control actually worked during Phase II, what theories underlay the Price Commission's policies, what problems and economic consequences ensued, and what can be learned from this massive experiment in regulation.

The unarticulated theory of the Price Commission's regulations, one gathers, was to produce pricing behavior similar to that which would prevail under perfect competition. Price increases would be permitted, but only as "justified" by increases in a firm's unit costs. Furthermore, no firm could raise prices if that would increase its profit margin. This controlled "pass-through" of costs would avoid production-distorting cost-price squeezes resulting from increases in factor prices not subject to controls (such as imported commodities, raw agricultural and forest products, and taxes) but would not allow a firm to reap windfall profits. Unfortunately, the authors suggest, the Commission failed to implement this theory fully, bowing to a variety of practical, administrative, and political considerations. Most firms, it turned out, had unreliable data on unit costs and productivity for each product; the commission therefore adopted regulations which allowed more flexibility in calculating cost pass-throughs. Processing thousands of individual price-increase applications threatened to hamstring both the firms and the Commission staff; the Commission therefore negotiated flexible pricing agreements with large multiproduct firms. These departures from "economic logic," the authors charge, were "too generous." Economic theory suggests that regulatory energies should not be expended on competitive markets, like retail trade; nevertheless, political considerations, the authors state, led the administration to extend coverage to retail prices with bad results. Retailers, pursuant to the regulations, passed along cost increases from the unregulated farm sector; it therefore appeared to consumers and workers that price regulations were ineffective, while wages were effectively controlled. Labor representatives quit the Pay Board. Small firms ultimately were decontrolled.

Whether strict adherence to "economic logic" is possible, or would have avoided these problems, is doubtful. Since regulators lack perfect information, and stringent pricing rules risk allocative distortions or noncompliance, regulators are always likely to "err" in the direction of "generosity." In the absence of a completely closed system, the cost pass-through theory fails to confront consumers' and workers' concern for distributional equity – the central political problem of governmental wage-price controls. By concentrating on price control economics alone, Lanzillotti *et al.* provide little insight into the political dynamics surrounding the problem of equity during Phase II, and do not suggest rules or institutional arrangements which might provide solutions.

The book does offer an interesting analysis, based on several econometric models, indicating that despite the relatively moderate Price Commission regulations, the rate of consumer price increases (except for the basically unregulated food sector) was about 2 per cent *lower* during Phase II than it would have been without controls, and that the controls contributed to recovery from the 1969–70 recession. The authors report relatively few controls-induced distortions in production. Why then were the Phase II controls abandoned? Were they becoming more disruptive as the economy approached full production, and if so, could sensible adjustments have been made? Was the decision to loosen controls in January 1973 based purely on a postelection return to orthodoxy by Nixon and his economic advisers, or were there persuasive business-community complaints behind the change? As insiders, the authors might have addressed these broader issues fruitfully. Instead, they provide a rather formal and narrowly focused analysis of the Price Commission experience, of interest to economic historians and future regulators, but a disappointingly limited contribution toward understanding the politics, potentials, and limits of governmental wage-price regulation.

ROBERT A. KAGAN

University of California, Berkeley

Racial Conflict and the American Mayor. By Charles H. Levine. (Lexington, Mass.: D.C. Heath & Co., 1974. Pp. 149. $13.50.)

Are political science models, approaches, and techniques appropriate to studying mayoral leadership in older American cities where racial polarization has occurred? Charles Levine thinks not. Levine critically evaluates the pluralistic model of mayoral leadership and presents alternative models of mayoral leadership within racially polarized contexts. The author draws upon case studies of three cities and their mayors – Carl Stokes of Cleveland (1967–71); Richard Hatcher of Gary (1967–) and George Seibel of Birmingham (1967) – to illustrate the problems of applying a pluralist model of governance, positing that "The obstructionist tactics of racial politics help to explain political immobilism in many American cities and the demise of innovative-minded mayors" (p. 23).

The book deals with four central concepts: leadership, race relations, pluralism, and public policy. Levine examines leadership from the perspective of mayoral enterpreneurial qualities – goals, resources, and leadership structures. His central hypothesis is that under conditions of racial conflict and polarization, opportunities for mayoral leadership are vastly minimized. When scarcity of resources grows and conflict over allocation expands, Levine finds the pluralist model to be deficient in relating such an urban setting to leadership performance. Consequently, the pluralist model is also deficient in measuring leadership effectiveness.

Levine studies mayoral leadership in the contexts of polarized cities (Gary and Cleveland) and a pluralized city (Birmingham). He assesses the effectiveness and policy impact of their mayors both from a pluralistic perspective and what he terms a "conflict perspective." In applying these two frameworks, Levine finds significant differences in leadership performance. The author concludes by developing a rough sketch of a contingency theory of mayoral effectiveness.

This is one of the better books written about mayoral leadership and the changing urban environment. Like Kotter and Lawrence's book, *Mayors In Action* (1974), Levine draws upon a rich interdisciplinary background to take a fresh look at the relationships of environment, leadership, and politics. The author's style makes rough reading in early chapters, torn as he is between demonstrating a grasp of research theories which relate to the subject and writing clear, concise, and understandable prose. Levine has some meaningful points to make, but they are frequently obscured by jargon and rather vaguely applied concepts. What he means by a "conflict model," for example, is not fully developed, and to the extent that it means how well the mayor mobilizes the black community and distributes rewards largely to that constituency, this definition comprises a very narrow, and even highly normative assessment of effective leadership performance. This is not a model. Rather, it is a value-laden judgment about the desirability of such a leadership style (as opposed to be the pluralist model which may have normative characteristics as well) without much concern over the costs, trade-offs, or even policy consequences of this approach to mayoral leadership.

Levine's work raises numerous questions concerning how political scientists study urban governance. Many older cities are operating under the new, more intense politics of scarcity, declining tax bases, heavy service demands, growing black population and declining white population, high unemployment, and loss of manufacturing jobs. Such cities are ripe for more open conflict, with new more intense groups clashing more directly as they contend for or resist change. A fragile polity is highly susceptible to polarization and increased hostilities with the consequence that mayoral-led coalition building and bargaining opportunities are greatly reduced. Whether or not contingency modeling will be any more helpful to political scientists in exploring the new urban terrain can be seriously questioned. Available descriptions of mayoral behavior may well be what Levine calls "partial, static, and superficial at best" (p. 131), but contingency theory typically leads to the conclusion that "it all depends" and we're back to situational analysis. Besides advocating the application of contingency modeling as a means to develop a "science of mayoral leadership," Levine also wants political scientists "to better service political leaders . . . rather than use political leaders mainly as research subjects," and "to reenter the world affairs with something unique to offer rather than function merely as informed citizens" (p. 141). These exhortations, well intended as they may be, smack of social science engineering at its worst, and a misunderstanding of the uses and perhaps limits of social science research at best. On the whole, however, *Racial Conflict and the American Mayor* takes us beyond the world of "Who Governs" into an uncharted research field which is a major contribution in itself.

DONALD HAIDER

Northwestern University

Public Planning: Failure and Redirection. By Robert A. Levine. (New York: Basic Books, Inc., 1972. Pp. 195. $6.95.)

Most public bureaucracies have been created with the intention of directly and indirectly serving the public on a permanent basis. The fundamental planning behind the organizational structure, policies, and procedures is critical as many of these programs become institutionally petrified beyond the point of making effective changes and corrections. When intended plans become obsolete or unworkable, the forces of public immunization prevent timely remedial action. The range of purposeful functions falling into the planning dilemma category varies from those of simultaneous similarity to those of pure contradiction.

Mr. Levine, the author of *Public Planning: Failure and Redirection,* has extracted several generalizations from a series of cases in which public planning has failed and in which it has succeeded. He contends that, "to be successful, planning has to change its simple-minded premise of a world in which planners lay out optimal courses to be selected goals, bureaucracies translate these courses into action, and implementation follows as neatly as building construction follows a blueprint. It will have to move toward the sophistication of recent descriptive analyses" (p. 14). Using this contention as a foundation for public planning improvement, the author sifts through his cases in order to present a corrective to the ills of planners and analysts.

The shotgun approach is used in criticizing the federal bureaucracy and corresponding situations at the state and local levels. It is clear that the well-documented and highly criticized planning *faux pas* of monolithic public institutions provide a fertile ground for inductive reasoning. The cases concentrate on tax policy, military planning, education, manpower, urban renewal, public assistance, and general fiscal and monetary policy. There seems to be, however, a lack of continuity in drawing parallels among these cases. The wide variety and scope of subject matter makes the shotgun approach less penetrating and the proposed solutions less convincing.

The book is arranged to show that highly administered systems that were planned and implemented "to operate through detailed application being levied on one set of people called a clientele by another set of people called officials" (p. 17) have generally failed. The solution to this problem rests with Adam Smith's ideas on the principle of self-interest and the mechanics of the competitive market system. The public planning function can be improved as generally stated in the book:

> . . . the beginnings of a way out of some of our current dilemma lie in the market-like and bargaining systems that combine the workable features of decentralization, self-administration, personal economic or political motivation, and the gross application of public policy rather than systems administered in detail by public officials to private clienteles according to plans laid out in detail by public planners (p. 23).

Although the author realizes that the market mechanism is not a universally applicable solution, he advocates injecting "bureaucratic competition" internally or externally to the public organization where there is redundancy or overlapping. He suggests that the planner make this concept a part of his tool kit in examining alternative solutions to planning problems.

Mr. Levine is too idealistic, given the complexity of the many situations involved in public planning. Although he recognizes this complexity, he seems incapable of dealing with it. His philosophical solutions are analogous to Elliot Janeway's recommendation to investors to "buy low and sell high." The questions the investor must answer by himself are how low is low and how high is high? The investor needs a great deal of sophistication to recognize the quality, the relative market of comparable investments, and expectations of conditions which could affect the investment. Even after an analysis is made, an element of risk remains. The public planner covers a larger gamut of variables. He must be sensitive to internal as well as external factors that affect desired goals, and the risks are sometimes greater.

In general, as the passage quoted above illustrates, the writing is difficult to follow. An analysis of one or two well-known and controversial programs would have made the book more interesting. Furthermore, a simple approach to Mr. Levine's ideas would have been more stimulating to the reader, especially if he was a planner involved in the specific topic.

MELTON L. SPIVAK

Department of Finance and Revenue,
District of Columbia Government

Policy Studies in America and Elsewhere. Edited by Stuart S. Nagel. (Lexington, Mass.: D.C. Heath & Co., 1975. Pp. 229. $16.00.)

This volume is the first in a series of books emerging from the pages of the *Policy Studies Journal* and the enterprising efforts of its coordinator, the Secretary-Treasurer of the Policy Studies Organization, Stuart Nagel. The present work stems mainly from the first issues of the *Journal* so that many of the essays included here were published initially in 1972–73. Most of them have not been much altered or brought up to date, and consequently the usefulness of the several literature reviews is considerably curtailed. Thirteen essays are included, five on theoretical or general issues involved in policy analysis, and the rest dealing with particular areas of substantive policy. Some of the essays were originally written as two or three separate and distinct pieces by different people, and little effort seems to have been made to make them into coherent presentations. Indeed, one problem throughout is the minor irritations of mistakes in grammar, syntax, and flow of ideas that effective editing should have corrected.

Some of the individual essays are helpful guides to the literature of a subfield or to the institutions and resources available for the interested investigator. Especially helpful are the comments by Richard Rose on British policy studies, Dean Mann, Geoffrey Wandesforde-Smith and Lennart Lundqvist on environmental policy, Richard Merritt on foreign policy, and Theodore Marmor and Hugh Heclo on poverty and welfare policy. Discussions of theoretical issues and approaches and of methodological matters are disappointing and well short of the state of the art.

The larger question raised by this volume and generally by the burgeoning literature and program expansion in the field of "policy analysis" is whether there is, in fact, a field with enough intellectual bounds and coherence to encourage us to hope for important growth in policy theory. On the strength of this volume one might conclude there is not.

Here as elsewhere the efforts to construct broad theoretical structures very often sound just like general theories of politics and lack any particular policy component. Some policy-centered theoretical efforts have defied operationalization and remain, however seductive and plausible, untested and untestable. What theoretical strength there is in the policy literature seems largely to be found within particular subfields. Budgeting studies, for instance, have developed a substantial, if contradictory, bundle of theoretical arguments. But welfare budgeting may follow different political rules from defense budgeting. We have long supposed that foreign policy processes were distinguishable from those governing domestic issues. Environmental issues cannot easily be subsumed under the same theoretical rubrics that serve educational issues.

This volume helps persuade me that it might be useful, and sooner rather than later, to acknowledge that if we investigate one policy sector (recognizing always, of course, that boundaries are blurry) we will not necessarily learn anything about another; that the best way to study policy seriously is to study it in the particular; that, in short, there is no intellectual field, at least not for research purposes, called policy studies. Rather, there are many areas of policy that richly repay the investigator who is willing to master the substantive details on which the play of contending interests, the politics, the outcomes and the impacts all depend.

ROBERT H. SALISBURY

Washington University

Maryland and the Empire, 1773: The Antilon-First Citizen Letters. Edited by Peter S. Onuf. (Baltimore, Md.: The Johns Hopkins University Press, 1974. Pp. 236. $10.00.)

Ironically, as political scientists have moved away from an earlier emphasis on abstract theory toward a more sophisticated understanding of the realities and complexities of political behavior, historians concerned with the American Revolution increasingly have become addicted to a one-sided emphasis on the intellectual theorizings of the Founding Fathers. Formerly, under the influence of Beard and Jameson, American historians derived their causal explanations primarily from economic and social factors. Now, under the influence of Bernard Bailyn (who is not responsible for the excesses of his disciples), the pendulum has swung toward a dependence on intellectual abstractions and constitutional theorizings as the primary determinants of political events during the formative years of our national development. Both the introduction and the content of this edition of the literary exchanges between two of Maryland's leading Revolutionary figures give solid evidence that it is time once again for the pendulum to reverse itself, this time, one hopes, stopping at a point somewhere between the extremes of earlier followers of Beard and Jameson and the current excesses of Bailyn's disciples.

In 1773 Daniel Dulany, author of the most effective protest against the Stamp Act of 1765, was Maryland's accepted intellectual leader. When the Legislature failed to establish a list of fees to be collected by the colony's officeholders in the course of their duties, Governor Eden opened a lengthy controversy by proclaiming the establishment of a set of fees on his own authority. Dulany rose to the defense of the governor in a series of exchanges in the *Maryland Gazette*, while Charles Carroll – fifteen years his junior and a Roman Catholic – responded with charges that Eden had usurped powers properly belonging to the Legislature.

As Peter Onuf points out in his introduction to this reprinting of the Dulany-Carroll exchanges (previously published in 1902, but without annotations), the "issue was peculiarly local; its implications, however, were as large as the first British Empire, and central to the constitutional crisis that destroyed it" (p. ix). In his thirty-page introduction, Onuf puts aside the "personal bitterness" and the "insult, innuendos, and recrimination" which "seem to overshadow the discussion" and concentrates instead on the "two diametrically opposed and ultimately irreconcilable ideas of the constitution" which underlay the debate (p. 16). In doing so, Onuf adds to our knowledge of the intricacies of Revolutionary constitutional theorizings, but he misses an opportunity to bring to light the very real and very human complexities of colonial political life.

Onuf correctly notes that Dulany was "the defender of established government" (p. 17) while Carroll was a member of an embattled Catholic minority. But, as the articles in the *Maryland Gazette* make clear, the debate between Dulany and Carroll involved far more than simply a mirror image of "the country and court philosophy which dominated English politics in the eighteenth century" which Onuf ascribes to it (p. 17).

The issue under debate was control of revenue, and both Dulany and Carroll were fully aware of what was at stake. Carroll complained that in Maryland "government is almost independent of the people," and "if it can settle their fees without the interposition of the legislature, administration will disdain to owe" any obligation at all to the people (p. 214). Dulany answered that neither "patriots" nor legislators were to be trusted any more than governors and judges, and that legislative supremacy was no more desirable than executive domination. The two men were not debating abstractions; both understood that the exercise of power is not a theoretical problem. The personal "insult, innuendo, and recrimination" which Onuf feels will be of interest only to the superficial reader are in fact essential elements in this debate between two men who represented not simply different constitutional theories but bitterly competing political factions and contending interest groups.

As this literary exchange records, the dispute with England – and Dulany's mistake in siding with Governor Eden – gave the representative of the Catholic minority faction (and allied interests) an opportunity that might otherwise not have been available. How well Carroll took advantage of his opportunity is readily apparent in these selections from the *Maryland Gazette.* Onuf's well-written but one-dimensional introduction offers little that is new. But the Antilon-First Citizen letters which record the heated exchange between Dulany and Carroll are in themselves worth the attention of anyone interested in gaining additional insights into how the crisis with England merged with and contributed to dramatically accelerated changes in the political life of the emerging American nation.

THOMAS C. BARROW

Clark University

Public Claims on U.S. Output: Federal Budget Options in the Last Half of the Seventies. By David J. Ott, Lawrence J. Korb, Thomas Gale Moore, Dave M. O'Neill, Attiat F. Ott, Rudolph G. Penner, and Thomas Vasquez. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1973. Pp. 218. $3.75, paper.)

The American Enterprise Institute has been described as a right-of-center "counter-Brookings." As one might expect, this volume is something of a counter-counterbudget. Despite its subtitle, the study focuses upon the 1974 budget much as the Brookings products have dealt with the annual budget documents. Rather than engaging in wide ranging speculation, the AEI scholars confine themselves mainly to a critical examination of the specifics of the 1974 Nixon administration recommendations, looking more for evidence of waste than for missed opportunities.

To some extent, this conservatism can be attributed to the circumstances of the time: 1974 was a year for keeping the lid on expenditures, even Brookings agreed. More importantly, however, the AEI analysts appear to share a conservative disposition toward government and its role. On the whole, they find that government is probably doing too much – especially, but not exclusively, in the area of social programs. Worse, what government does do, it often does not do well: needed government activity (e.g., in support of agriculture) is often seen as going not only too far, but in the wrong direction. Great concern is shown for the health of the private sector, and fear that government will take over legitimate private functions (e.g., health insurance and care). Unfortunately, this general perspective, which seems to be increasingly fashionable and perhaps even influential in the middle 1970s, is not clearly spelled out. It must be inferred from the disparate essays on major budgetary categories, for the editor attempts little real summation or synthesis.

The most common recommendation throughout is that the budget be cut. Thus, in the areas of defense (the B-1, among other things, is deemed expendable), agriculture, science, technology, industry, manpower, and housing, the AEI analysts suggest that we would be better off without some of what the Nixonians were willing to propose. In most cases, policy alternatives not included in the administration's budget proposals are not discussed, but there are exceptions. In agriculture, the idea of providing support tied to the farmer, not the farm, is proposed as a temporary measure, but is scarcely elaborated. In education, the idea of a voucher system for primary and secondary education is considered at length, and is viewed with favor for its potential for encouraging the private sector and providing consumers of education with a wider array of options. The plan is not fully endorsed, apparently because it would be expensive. National health insurance and income maintenance alternatives are also explored, but major departures from the status quo are rejected in favor of modest modifications.

These essays – ten in all, counting an introduction and a none-too-helpful "overview" – follow a common format, moving from projections (through 1981) to point-by-point analysis. The projections are of little interest today, because too many unanticipated events have occurred since 1973. The analyses, each by a different author or authors, are uneven, but all are at least reasonably thorough discussions of the substance of the 1974 budget. Their most common shortcoming is in detailing too few alternatives, but the most *serious* weakness overall is the book's failure to explicate the philosophical basis for the generally conservative approach taken. The explanation that the various authors would not always agree in detail rather misses the point. If the volume has enduring value – i.e., if there is any reason now for us to examine a study of the 1974 budget – it is because such efforts can be, in effect, works of applied political philosophy. This one is that, and it is interesting mainly for that reason. Future efforts would be more useful if, in this respect, more explicit.

CHARLES WALCOTT

University of Minnesota

Mediacracy: American Parties and Politics in the Communications Age. By Kevin P. Phillips. (Garden City, N.Y.: Doubleday, 1975. Pp. 246. $8.95.)

Kevin Phillips is a young newspaper columnist, author of the much-discussed *Emerging Republican Majority* (1969), a former appointee in the Nixon Administration (before he resigned in disgust in mid-1970), and a leading right-wing populist intellectual.

As early as 1970, *National Review* editorially expressed some reservation about Phillips's class-oriented mode of political analysis, which it characterized as "a sort of neo-Marxism cum statistics" (see: "Marx Goes Statistical," *NR*, 22 [July 14, 1970], 720). I suspect that one of the causes of *National Review*'s discomfort was not Phillips's attention to class, but his preference for the "wrong" class – namely, the working or lower-middle class. More recently, Phillips has had a falling out with America's pre-eminent conservative spokesman, William F. Buckley, Jr., whom Phillips has portrayed pejoratively as a culturally chic elitist. Buckley, in turn, called Phillips an "eccentric theorist" and a "voice from Philistia" whose writings are "orotund" (see: Buckley, "A Voice from Philistia," *NR*, 27 [August 15, 1975], 900). These squabbles indicate that Phillips is no run-of-the-mill conservative. Furthermore, he is no ordinary pundit, for as Walter Dean Burnham of M.I.T. says in his jacket blurb, Phillips is "one of about five people in the United States who have a really good understanding of American electoral politics."

Phillips states the thesis of this book in these words:

> During the previous century, the nation's economic elite was conservative, and liberal-conservative struggles were rooted in that economic context. This book will examine the rise of a 'new class' of affluent liberals – and the impact of that rise. A new correlation is arising – among education, wealth, and *liberalism* – to replace the old one – among education, wealth, and *conservatism*. And, on a number of issues, opposition to liberal elitism is strongest among the groups historically in the vanguard of opposition to conservative economic elites (pp. 1–2).

There is, indeed, a good dose of neo-Marxism (usually unacknowledged) in Phillips's analysis of American politics. It is axiomatic for Phillips that "economic upheaval and political change go together" (p. 13). With the Industrial Revolution, says Phillips, manufacturing became the dominant economic factor, and political power was transferred from a rural-based aristocracy to an urban middle class. With the *post*-industrial Revolution (which America – more than any other country – is now going through), the production, consumption, and dissemination of "knowledge" is becoming the dominant economic factor. As a result, political power is being transferred from the conservative business elite to the knowledge (or "liberal") elite. By 1980, it is projected that roughly 40 per cent of the U.S. gross national product will be accounted for by the shaping and marketing of ideas, information, and services, but only 25 per cent by the manufacture of goods. The new knowledge elite will – presumably in the fashion of all elites – promote its own interests: "Instead of having a vested interest in stability, as did previous conservative business establishments, the knowledge sector has a vested interest in change – in the unmooring of convention, in socioeconomic experiments, in the ongoing consumption of new ideas" (p. 17). If "liberalism" connotes an openness to change, the knowledge elite cannot help but be liberal, for "change is as essential to the knowledge sector as inventory turnover is to a merchant or manufacturer. Change keeps up demand for the product (research, news, theory, and technology)" (p. 33).

Phillips foresees a shift in the meaning of the terms "liberal" and "conservative" – heretofore defined according to social relations obtaining in the industrial era. In the *post*-industrial era, liberals will divide into a New (knowledge elite) Left and an Old (blue-collar) Left. Conservatives will divide into an Old (business elite) Right and a New (anti-elitist) Right. Phillips foresees an intensification of *ideological* politics based on *class* conflict. As the elitists of the Old Right and New Left coalesce, the populists of the Old Left and New Right will converge. Although the populists will be "conservative" on social and moral issues, Phillips leaves no doubt that the New Right will be "profoundly *unconservative*" (p. 206; italics in original) in that it will favor an activist New Deal-style government which will control the economy. Moreover, the New Right will push for *public* control of the free-enterprising media elite.

In contrast to his *Majority* book, Phillips does not expect that the Republican Party will be the vehicle for transferring power to the people: "despite the shift in control of the national party from the northeastern establishment to the new forces of the South and West, one can only say that power has passed from the boardrooms of Manhattan and the clubrooms of Boston to the petroleum clubs of Texas and the defense industry suburbs of California; as of 1974, it has not passed to blue-collar workers, northern urban ethnics, or southern Wallaceites" (p. 197). Indeed, Phillips speculates that the Republican/Democratic party system may itself be obsolete.

Phillips's book is thought-provoking, but it is not as persuasive as it might have been. It is not well organized. Its tone is too often flippant and chatty. The book abounds with maps, charts, and poll data, but too often we are not told the sources of the information. Too often evidence is marshalled for the purposes of polemical generalization and speculation, omitting consideration of exceptions, nuances, and other complexities. But despite these difficulties, this book will serve as a foil for significant controversy.

DALE VREE

Stanford University

Electoral Reform and Voter Participation: Federal Registration — A False Remedy for Voter Apathy. By Kevin P. Phillips and Paul H. Blackman. (Washington, D.C.: American Enterprise Institute for Policy Research and Stanford, Cal.: The Hoover Institution on War, Revolution and Peace, 1975. Pp. 135. $3.00, paper.)

In this small book Kevin P. Phillips and Paul H. Blackman examine turnout in U.S. elections, assess the importance of various factors affecting it, and review proposals for "some version of federal voter registration" (p. 1). In pursuing these objectives, Phillips and Blackman discuss the law and practice of registering voters in various states of the union, the United States in earlier periods, and in Canada, Great Britain, Finland, Sweden, West Germany, and Australia. The book is intended to be of value to policy makers.

The authors' argument can be summed up as follows: Federal registration of voters would be an easy and popular "reform" that most probably would not greatly affect turnout in elections. Proposals for such action should be carefully scrutinized, nonetheless. They may be an opening wedge for greater federal regulation of elections, and they *might* alter the American party system radically.

Thus, Phillips and Blackman hedge their bets. The book's subtitle, "Federal Registration: A False Remedy for Voter Apathy," sets its general tone, while an assertion that federal registration has a "far-reaching possible impact" (p. 4) provides an escape clause. At their most precise the authors suggest that "a federally initiated voter-registration process is unlikely to raise U.S. participation much more than half the distance between our current rate (55 per cent) and the Anglo-Canadian average of 70 to 75 per cent" (p. 53). This estimate implies that federal registration might increase participation by 7.5 to 10 percentage points, representing (in 1972) some 10 million to 14 million voters. From many points of view such a change would not be trivial.

The quality of the evidence that this book musters in support of its thesis is poor. Its authors seem wholly ignorant of the standard techniques of multivariate analysis. Though many examples could be cited, one will suffice to show where such innocence has led.

Phillips and Blackman note that California "has worked diligently to increase voter registration" (p. 15), and they list some of the ways it has tried to do so. Then they say, "compared to its neighbor Oregon, where efforts to increase registration *have not been quite so extensive* (my emphasis), California's turnout has not improved" (p. 16). But consider: (1) By the authors' own estimate the registration procedures in both Oregon and California are relatively liberal; one would therefore not expect such differences as do exist to lead to great differences in turnout. (2) California's registration procedures are not more liberal than those of Oregon in all respects; in 1972, for example, California stopped registering voters more than three weeks before Oregon did. (3) Phillips and Blackman suggest at least twenty factors other than ease of registration that might cause turnout to differ in different jurisdictions, and they control for none of these. All that the comparison does conclusively is to show that the impact on voting of differences in the two states' registration procedures is not greater than that of all other factors combined. And so it goes.

Sloppiness, as well as primitive analytical methods, contribute to the shortcomings of the evidence that Phillips and Blackman present. Consider the following statement: "According to ... [a study conducted by the University of Michigan's Survey Research Center] ... restrictive or permissive requirements make only about a 3 percentage point difference in whether persons regularly vote or do not vote" (p. 14). The finding is misreported. The figure cited applied only to the North. (See Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* [John Wiley and Sons: New York, 1960], p. 277). In the South the proportion of those always voting in states with restrictive requirements was 9 percentage points below that in states with moderately restrictive requirements. (No southern states were counted at the time of the study as having permissive requirements for voting.)

At various points in their book, Phillips and Blackman suggest possible consequences of federal registration: increased fraud; invasions of privacy; pranks; inefficiency; "federalization" of the electoral process; "homogenization" and "nationalization" of the major parties; increased effectiveness for trade union leaders in mobilizing their followers; the dissolution of the two-party system; and changes in the socioeconomic composition of the electorate, the balance of power within and between parties, and the features of successful candidacies. Policy makers should welcome a specification and exploration of any or all of these possibilities. How much space do Phillips and Blackman devote to that task? About three pages.

STANLEY KELLEY, JR.

Princeton University

School Finance in Transition: The Courts and Educational Reform. Edited by John Pincus. (Cambridge, Mass.: Ballinger Publishing Company, 1974. Pp. viii, 319. $15.00.)

This volume of analyses of the opportunities for school finance reform in the United States is valuable in two respects: it brings together an excellent set of essays on various political, legal, and economic aspects of the school finance problem; and it demonstrates by example a kind of analysis which should be applied to other policy areas.

John Pincus, as editor of this Rand Educational Policy Study, has brought together the most knowledgable students of this subject. Essays by Stephen Barro, by Robert Hartman and Robert Reischauer, and by Charles Benson focus on the alternative school finance systems – including both the revenue and expenditure sides – which might be adopted to overcome present inequities in the availability of resources for public schools. These technical analyses are important, even for the lay reader, because they demonstrate the precision needed to develop policies and programs which will address the complicated issue of equity in diverse economic and jurisdictional situations. The effectiveness of these resources in the educational process is examined by Henry Levin. A fifth essay, by Joel Berke and Robert Goettel, focusing on educational issues, investigates the future of "categorical programs" – special-purpose, often compensatory educational programs – under the new legal decisions seeking to improve equity in school finance.

In contrast to these essays, another group focuses on the political and institutional problems in school finance reform. R. Stephen Browning and David Long write about the courts, and their probable behavior after the Rodriguez decision by the U.S. Supreme Court. Two articles – one by Alan Campbell and Dennis Gilbert, and one by Arnold Meltsner and Robert Nakamura suggest the political obstacles to developing and implementing school finance packages consistent with judicial mandates in different states.

Although all the essays are of high quality, perhaps the most intriguing is David Cohen's chapter on the meaning of equality as reflected in education and social policy more generally. Cohen shows that despite the sophisticated analysis of financial and judicial arrangements, proponents of school finance reform are still quite far from resolving the philosophical problems implied by the drive toward equality of educational opportunity. His dictum that "life is sometimes stranger than art" is certainly true in school finance, a subject which received a great burst of attention from the Serrano decision in August 1971 to the Rodriguez decision in March 1973. Progress was made in raising the consciousness of legislators and citizens concerning equity in public education, but even the experts have found analytical problems very difficult to resolve without leaps of faith concerning important questions, such as the impact of dollars on learning. And so, despite progress at the state level, the subject has become less attractive, leaving many competent analysts, such as the authors in this book, to rethink the problem of developing practical tools for social policy. The experience has been an important one, particularly in terms of the sociology of knowledge, and deserves to be studied by others intending to undertake detailed policy analysis.

This book also is worth reading from another perspective: it tells of efforts for reform, of analysis, of frustration, and of rethinking. In this respect, it is an important addition to the "revisionist" view of the War on Poverty, for it shows that social problems in America are more complicated than they seem to be at first glance and that despite valiant efforts, they will not necessarily be solved by those who set out to do so. This is an important lesson from which we can all benefit, regardless of our area of interest.

MICHAEL A. COHEN

The World Bank, Washington, D.C.

Federal Programs and City Politics: The Dynamics of the Aid Process in Oakland. By Jeffrey L. Pressman. (Berkeley, California: The University of California Press, 1975. Pp. 178. $10.95.)

Popular politics demands villains, and among the favorites these days are the "best and the brightest," those national government planners whose foreign and domestic policies came to grief in the late 1960s and early 1970s. True, much of this sentiment is transient and will change with time. But at least some of it is more durably grounded in fundamental changes occurring in the structure of the country's politics.

One of these, the changing shape of the federal bargain, is the major concern in Jeffrey Pressman's thoughtful and timely analysis of *Federal Programs and City Politics.* The setting is Oakland, California, where between 1967 and 1969, Pressman served as a participant-observer in the office of then mayor, John Redding. From this and other sources, including extended interviews with federal administrators, Pressman developed a rich data base which he uses judiciously to assess the increasingly intricate ways of modern federalism.

The federal system, as Pressman notes, has been troubled of late by mounting tensions. This is not to say strife is pandemic; there is cooperation as well as conflict, and Pressman is concerned with both. But his emphasis is on the latter, and on the sources and the consequences of strains associated with federal aid to cities.

These strains derive partly from the novelty of the situation. Although long a presence in cities, the national government's active concern for urban problems was, until the 1960s, quite modest. During that tortured decade, however, Washington both broadened and intensified its domestic policy concerns. Whatever the substantive merits of these initiatives, they entailed expanded intergovernmental contacts, and increased the occasions for intergovernmental disagreements.

Neither federal nor local officials were fully prepared for this change, and their early contacts, in Oakland at least, were marked by considerable groping. This included efforts to promote better communications through such conventional devices as joint task forces and liaison committees. While these may have facilitated the process of mutual discovery, they by no means eliminated conflict. If anything, they heightened it. "The differences," according to Pressman, "[went] far deeper than misunderstanding or confusion" (p. 137), and these differences were simply made more explicit by improved communications. Further, communications occurred within a restricted framework, "pseudo-arenas" (p. 15) staffed by federal and local officials unable, or if able, unwilling, to confront the political questions separating them.

These political questions, Pressman argues, create enduring difficulties within the federal system. True, intergovernmental relations are often symbiotic since each participant is dependent on the other for important resources – city officials for federal funds, and federal officials for local implementation of programs. Even so, these officials confronted each other in Oakland with conflicting organizational perspectives and needs. To justify their existence to Congress, for example, and to obviate attacks from competing federal agencies, national officials needed to move boldly and with dispatch. This was at odds with the needs of local officials who were more measured in their approach. Being on the front lines, and forced to deal with the electorate in more direct and consequential ways, local officials were more likely to "limit their horizons to [programs] which [could] be understood and protected" (p. 127).

Local officials limited their horizons, additionally, since no one of them had broad programmatic responsibilities. In Oakland, as in other cities, there were multiple jurisdictions, making it difficult to mount "the glamorous and exciting programs" of the sort championed by federal officials (p. 88). This fragmentation, Pressman notes, is a serious obstacle, not only to cooperative federalism, but to the effective workings of local democracy as well. As such it is a national challenge, and he urges federal officials to supplement their substantive concerns with programs to promote "the development of strong and open local institutions." These, he maintains, "would make the city's political system better able to articulate and respond to its citizens' demands, as well as provide potential sources of support for federal programs" (p. 146).

Whether federal officials could achieve this goal is unclear, as is the political wisdom of their attempting to do so. Others before have tried, but failed, to create "strong and open local institutions." The problem, in large measure, may be a national trait, namely a deep and persistent ambivalence toward strong political leadership. Indeed, Pressman himself seems to share this ambivalence, for example in his assessment of revenue sharing – a policy that could strengthen elected local officials, but might threaten important policy minorities (p. 139). As long as this ambivalence persists, and there is no reason to expect it will not, local political leadership will continue to be an uncertain commodity. As a result, tensions between city politics and federal programs will continue, as will the complaints, both from Washington and from the provinces.

RUSSELL D. MURPHY

Wesleyan University

A Little Group of Willful Men: A Study of Congressional/Presidential Authority. By Thomas W. Ryley. (Port Washington, N.Y.: Kennikat Press, 1975. Pp. 198. $12.95.)

The present work is an historical account, in fascinating detail, of how, in Wilson's famous denunciation, a "little group of willful men . . . rendered the great government of the United States helpless and contemptible." The filibuster in March 1917 that frustrated Wilson's requested authorization to arm American merchantmen against the German submarine, and "to employ such other instrumentalities and methods" as he might choose against Germany at sea, is another of the dramatic encounters in American history between the executive and legislative branches of government over questions of foreign policy.

There seems to be little choice but to treat these recurring confrontations as discrete episodes. While the armed ship instance led soon thereafter to adoption by the Senate of its first cloture rule, it is hard to see anything fundamental that might be done to mitigate the constitutional overlap between Congress and the Executive in the conduct of foreign policy. There was no such sequel to the fiasco over the Versailles Treaty, while one is not impressed with either the cogency or permanence of the congressional resolution of 1973 limiting the amount of time that American troops may be committed to battle without the approval of Congress.

An historian, Ryley tells a story; he attempts neither to moralize nor generalize. The political scientist can hardly do better. Clashes over foreign policy between the Executive and Congress are inherent in the constitution. Just now they are increasing in frequency and intensity. Trying though these clashes are, resignation seems the only recourse, tempered by retrospective judgment that vindicates now one and then the other of the two branches, locked forever in uneasy embrace.

EDWARD H. BUEHRIG

Indiana University

Patterns of Recruitment: A State Chooses Its Lawmakers. By Lester G. Seligman, Michael R. King, Chong Lim Kim, and Roland E. Smith. (Chicago: Rand McNally and Co., 1975. Pp. 269. $8.95, cloth; $4.96, paper.)

Patterns of Recruitment: A State Chooses Its Lawmakers is an analysis of the recruitment process of candidates for the lower house of the Oregon legislature during the 1966 campaign. The study had its beginnings in a political science graduate seminar at the University of Oregon under the tutelage of Professor Lester G. Seligman. This study adds to the impressive list of works on political recruitment that Professor Seligman has written during the last twenty years.

The literature on political recruitment has been plagued by two major problems: a lack of coherent and commonly accepted concepts which are operationalized (measured) in the same manner over time, and a lack of an integrated theory of the recruitment process. The result has been a plethora of socioeconomic background studies of political elites with little or no supporting political theory except for questionable assumptions of social determinism. Only in the past few years have scholars become interested in looking at the entire recruitment process, including both the causes of certain recruitment patterns and the consequences of those patterns on public policy. Seligman, Michael R. King, Chong Lim Kim, and Roland E. Smith successfully address most of the inadequacies of the political recruitment literature and make an important theoretical and empirical addition to our knowledge of the recruitment process.

Seligman's early work and certainly this latest analysis have been rich sources of hypotheses on the recruitment process. In *Patterns of Recruitment,* the authors present a coherent model focusing upon five key recruitment elements: (1) from what sociopolitical group are political activists recruited; (2) who are the recruiters; (3) what criteria are taken into the recruitment equation; (4) how are they selected; and (5) to what role is an individual recruited?

The data for the analysis are based upon interviews with both winners and losers in the 1966 primary and general elections for the Oregon legislature. The interviews were administered in three waves: preprimary, postprimary but pregeneral election, and postgeneral election. The interviews elicited basically three types of information: general socioeconomic and political data; data on the process of recruitment (the questions on decisions to run and opportunity and risk are especially good); information about candidate's evaluations and perceptions of their election experiences; and the impact of winning and losing the election.

Unfortunately there are few instances of interval-level statistical analysis, and most generalizations stem from tables reporting bivariate relationships. The entire analysis is beset by the continuing problem of the small number of respondents used in the statistical tests. At one point the authors report several chi squares (without reporting measures of association) with four total respondents. In the only section utilizing partial correlation, the number of respondents used is twenty-one. In a section called "A Causal Analysis of Certification" (pp. 146–151), the amount of variance explained in the dependent variable of each model is not reported, and partial correlation ratios are used for the paths in each model. Generally the statistical analysis could have been much more sophisticated, considering the amount of time and care taken in collecting the data, but this does not seriously detract from the rich set of generalizations the authors reap from their respondents.

Some of the most interesting findings are related to the reasons that people run for political office. Most candidates felt that interest groups were the most important legislative recruiters in Oregon. According to the authors, interest groups have filled a major recruitment role because the direct primary fragmented the importance of political parties. They support Prewitt's findings from the Bay Area city council study, that small primary groups become important sponsors of candidates when there is an absence of political parties. They also discover that most of the respondents perceived little risk in candidacy because, "the outcome is uncertain only in a few elections" (p. 187). Most candidates know when they are going to win or lose, and they know what the costs are going to be to their political, social, and occupational futures.

The loss of an election has little attitudinal impact on legislative candidates in Oregon: "For most candidates, the loss of an election does not alter their political attitudes toward the system or diminish their zest for political activity" (p. 187).

An important distinction is found by the authors among legislative districts and incentives to run for political office. Urban areas generate stronger incentives for candidacy (i.e., more self-recruitment) than do rural areas. Rural unsponsored candidates are a rarity; therefore, a variety of sponsors approach rural candidates, but because political party organizations are diffuse and fragmented in rural Oregon they are not significant recruiters. Primary groups and secondary associations are the most important candidate sponsors in rural Oregon legislative districts.

Although the study is a major contribution to our understanding of the key variables involved in the recruitment process, it has a major shortcoming. The authors did not examine the linkage between the four recruitment patterns which they discover (competitive, majoritarian, rural oligarchic, and rural competitive) and legislative behavior and ultimately public policy. Chapter II is the only part of the book where the linkage between recruitment and public policy is discussed. At no point do the authors analyze the linkage between the recruitment process and Oregon legislative decision making. The central question to ask of their study is: What impact does each of the four recruitment patterns have upon policy preferences and legislative behavior?

Scholars are interested in how political elites are screened and selected because such study can shed light not only on the distribution of power but also on the linkage between elite, constituents, and public policy. *Patterns of Recruitment* does not analyze this linkage. Therefore, a major reason for studying recruitment is missing from this analysis: how people gain and monopolize political power and what they do with it once they have it. In conclusion, although this book is a step in the right direction, we still need research on both the causes and the consequences of recruitment patterns over time and in a comparative context.

JAMES A. THURBER

American University

Personality and Democratic Politics. By Paul M. Sniderman. (Berkeley and Los Angeles: University of California Press, 1975. $13.75.)

This book is based on Sniderman's dissertation, which won for him the APSA's E. E. Schattschneider Award in 1972. That this prestigious award was given to a manuscript on politics and personality is a favorable harbinger for students of that field. After all, political psychology continues to move haltingly at best around the periphery of political science, very much like an unwanted guest at a fancy ball. One begins Professor Sniderman's book with the hope that its apparently high quality will help make the study of political psychology *persona grata* at the political science cotillion.

Are these hopes realized? In very large measure, yes. Certainly one can understand why the Awards Committee thought so highly of Sniderman's work. First, there is the major theoretical problem Sniderman has set himself. He returns us to perhaps the central question of contemporary political psychology: What can we expect psychologically of leaders and followers in a democracy? Do the psychological characteristics of leaders and followers place any limitations on democratic practice? More specifically, are leaders psychologically more "competent" than followers, or are leaders driven by scarred self-esteem and battered self-images of the sort predicted by Lasswell and exemplified by Nixon?

Sniderman not only chooses a major problem for analysis, but he also proceeds in an extremely sophisticated fashion. From the vast array of psychological theories and findings available to him, Sniderman chooses the concept of self-esteem as his focus. He then lays out a motivational version of self-esteem theory (largely psychoanalytic in origin) and a social learning version of self-esteem theory (largely social psychological in origin). Throughout the book he blends these two theories in a most complex and undogmatic fashion, revealing both their limitations and their advantages. Most important, unlike most writers in the field, Sniderman is alert to the configurational basis of personality. He deliberately avoids simplistic, single-motive theories. For this reason Sniderman is able to show that self-esteem helps knit together other psychological qualities which, *in concert,* produce important differences in political styles. For this reason, as well, Sniderman is able to dismiss with such success the notion of "democratic personality." As Sniderman shows, there is no single source of personality support for democracy; indeed, the very psychological qualities of openness, flexibility, and rationality deemed part of democratic personality by Lasswellians actually predispose the person toward *non*democratic attitudes in undemocratic regimes. In sum, personality typologies and political typologies should be kept unmixed.

Lastly, there are the findings themselves. After carefully examining large samples of both the mass public and political leaders, Sniderman concludes that neither the motivational nor the social learning conception of self-esteem tells the whole story about American mass/elite psychological differences. The most general application of the Lasswellian argument seems clearly incorrect: in general, elites are *not* driven by compensatory behavior to recover lost self-esteem. Instead, in the aggregate, political leaders have higher self-esteem levels than the mass sample. Indeed, low self-esteem acts to screen out political stimuli, and therefore reduces political participation. Nonetheless, a substantial proportion of the political leaders studied manifests a syndrome in which low self-esteem combines with obsessiveness and materialistic ambitions. The result, therefore, is that the political elite contains a high proportion of "driven" individuals. More disquieting is the fact that, contrary to the views of Lane and others, the profession of politics does not penalize these low self-esteem compulsives. As Sniderman shows, once *into* politics, this relatively small group is as successful as the "well-adjusted." In sum, according to Sniderman, leadership in American democracy is generally distinctive for its high level of self-esteem, a characteristic that contributes to the survival of the system. Nonetheless, the elite contains sources of potential disarray. Much to his credit, Sniderman does not find it necessary to please either hardline elitists on the one hand or pluralists on the other.

Nevertheless, there are worrisome aspects of this research. Perhaps most questionable is the data base. Sniderman's work is a secondary analysis of Herbert McClosky's by now extremely well-known mass and elite surveys which were conducted in the late 'fifties. Whether these data tell us much about 1977 is very much open to question. Sniderman argues that other studies conducted in recent years provide evidence for stability in the relationships he uncovers; therefore, we need not worry. But reassurances of this sort are at best suggestive. Only if the McClosky instrument itself were used in a contemporary replication would our doubts be stilled.

This is not the only hazard of secondary analysis that Sniderman encounters. As he points out in one place (p. 48) the original McClosky data were not gathered for use in a study of self-esteem. Not surprisingly, therefore, despite Sniderman's extremely meticulous scale-construction procedure, the face validity of some of the items used to measure self-esteem is uncertain, at least to me. For example: "When I disagree with people it turns out later that I was right," is no more necessarily a measure of Snidermanian "interpersonal competence" than it is of simple belief in one's own purely *intellectual* capacity. Nor is "I often prefer to be with people of wealth and good breeding" necessarily a statement of "status inferiority"; "I" could prefer to be with such people because I find them more interesting, intelligent, and enjoyable than others. After all, snobs have rights, too.

The overall status of the self-esteem concept is also a bit worrisome. Sniderman shows – quite properly – that, because his study is correlational, the causal links between self-esteem and the many other psychological qualities to which it is related must remain uncertain. He settles for the argument that self-esteem sits at the center of a seamless web of psychological qualities. This rather indefinite conclusion – though undoubtedly responsible – makes self-esteem a less important component of political psychology than Sniderman would prefer. Nor is the matter improved by Sniderman's inability to show that self-esteem endures over time as a true personality characteristic must. Nor, finally, is the matter improved by Sniderman's inability to demonstrate the *origins* of self-esteem, a problem which Stanley Renshon tackles with some success in his work (Stanley Renshon, *Psychological Needs and Political Behavior* [New York: The Free Press of Glencoe, 1974]). As a result of these lacunae, the concept of self-esteem has a somewhat disembodied quality.

Indeed, of the three components of self-esteem Sniderman identifies, only "interpersonal competence" discriminates strongly between leaders and followers. Since four of the seven items measuring interpersonal competence directly relate to leadership ("I hate to tell other people what to do," "I would rather not have very much responsibility for other people," "I dislike to have to talk in front of a group of people," and "I doubt whether I would make a good leader,") it would be surprising if this were not so. Still, the other two components of self-esteem (personal unworthiness and status inferiority) do play some role. On the whole, however, the tools of the analysis are not entirely adequate to the task.

Sniderman shows that the motivational and learning aspects of self-esteem are related in complex ways. Therefore, any conclusion from this study that viewing self-esteem purely from a learning perspective will solve the problem of designing therapeutic practices for democratic citizenship would be a bit facile. True, the Lasswellian approach seemed to portend hordes of psychiatrists marching about in an orgy of mass psychotherapy intended to spur democratic participation. But the learning approach as Sniderman outlines it does not really substitute anything uniquely its own in place of this silly alternative. Indeed, Sniderman takes pains to point out that *political* therapy – such as "participatory democracy" – will not really improve self-esteem. But surely such therapy *would* if self-esteem could be improved merely by social learning coupled with practice. Nor should this be construed as a serious argument against participatory democracy, for that theory does not derive its main justification from theories of personality. Thus, Sniderman does not really supersede Lasswell, but, through precision and craftsmanship, he makes a substantial contribution all his own to the study of personality and politics.

RICHARD M. MERELMAN

University of Wisconsin-Madison

Managing Local Government Under Union Pressure. By David T. Stanley. (Washington, D.C.: The Brookings Institution, 1972. Pp. 177. $6.95.)

The Unions and the Cities. By Harry H. Wellington and Ralph K. Winter, Jr. (Washington, D.C.: The Brookings Institution, 1971. Pp. 226. $7.95.)

These small volumes examine the impact of unionism and collective negotiations on employer-employee relationships in municipalities and other local governments. Both are tautly organized and well-written works by highly competent scholars. There are differences in points of view and judgments.

Wellington and Winter observe in their Foreword (speaking of unions) that "few other groups can so profoundly affect the life of a community" (p. vii), and in their final paragraph refer to "the power the unions will win if we mindlessly import into the public sector all of the collective bargaining practices developed in the private sector" (p. 202). The intervening discussion reflects this point of view. Stanley closes his examination on a different note, i.e., that despite employees' gains, there is "little prospect that the transaction will become overbalanced against management, given the continued functioning of the American political system and the exercise by management officials of a reasonable mix of resolution, ability to listen, decisiveness, labor relations knowledge and good will" (pp. 151–152). Stanley has fewer reservations about bargaining in the public sector.

Wellington and Winter accept the practice of collective bargaining in the private sector but view the public sector as "very different indeed" (p. 8). They assume that competitive forces in the private sector "the discipline of the market" act to limit the impact of bargaining. I believe that this faith in the discipline of the market is not well-founded: Oligopolistic conditions, with a considerable element of administered pricing, are typical of much of the private sector in which bargaining is widespread. Nor are consumers sufficiently well-informed, organized, and powerful to resist effectively administered price adjustments that may result from negotiations.

Wellington and Winter take the position that strikes in the public sector properly may be curbed, although they recognize that not all strikes among public employees have a similar impact. But because such stoppages result in the disruption of services and may produce, or exacerbate, political problems, the authors believe that government "must be in a position to protect itself and its citizens" (p. 73). In my judgment, the issue of strikes by public employees often is overemphasized. While militancy and strike action is growing among public employees, even today the public sector is much less subject to strikes than is the private sector, and many types of work in the public sector (such as the services of billing clerks in the water department) are not important on an hour-by-hour basis as are the services of firemen, police, or prison guards. Wellington and Winter recognize this in concluding that "the emergency dispute problem does not compel a complete ban on strikes" (p. 194).

The authors advocate procedures that call for third-party involvement in search of solutions, including arbitration when the authority of the arbitrator is restricted; and fact-finding, when fact-finders are allowed to attempt mediation. As a last resort, say the authors, the approach of selecting one from a number of possible procedures "will not stop all strikes but has the best chance of reducing their incidence" (p. 186).

Stanley's study concentrates on the impact of unions on the administration of nineteen urban areas, largely cities, but also a few counties, whose populations range from 75,000 to nearly eight million. Based on his study Stanley observes that "employee unions are changing the way local governments are administered" (p. 1). As a result of the steadily increasing rate of change, Stanley believes, "employees appear to be less acceptant and more challenging; . . . management is less dominant" and "employee-management relationships tend to become more formal and potentially more hostile" (p. 58). Further, "unions contribute to pressure on local governments to find new and expanded revenue sources" (p. 135), but "in only a few instances have the impressive political power and tactical position of the unions been used in efforts to achieve more effective local tax measures and more satisfactory systems of intergovernmental fiscal relations" (p. 135).

Stanley's judgments will produce mixed reactions among readers. He asks, rhetorically, "what is happening to government achievements under union pressures. . . ?" and answers "not much" (pp. 138–139). He believes that unions sometimes may improve program effectiveness but that "management effort is invariably more tense, more time-consuming, more public than that applied to non-union personnel problems because of the adversary posture of the unions and their generally aggressive attitudes" (p. 140).

While unionism has affected the time and effort devoted to employer-employee relations in the public sector, Stanley gives a warm evaluation of the end result, holding that "strikes have inconvenienced the public and have probably increased government costs, but . . . they have also corrected injustices, secured gains for employees, and been a major cause of change in public administration" (p. 144). And, finally, "recent history suggests that . . . in the future . . . disastrous outcomes are hardly likely" (p. 145).

Since these books were written, many publications have explored labor relations among public employees. Such studies are desirable because the impact of bargaining and its effects on the work of public employees and problems created for local administrators should be understood. Employment conditions in the public sector fail to meet the defensible needs of workers in many of the smaller cities, towns, and counties of the nation. The well-publicized salaries of New York or San Francisco are not typical. Public workers have been left behind in many instances; they are becoming "less acceptant." The position of Stanley is more nearly in step with the judgments being expressed by many recent authors. My own study of the impact of bargaining in Kansas, where public employee bargaining is just beginning, supports Stanley's view that the early impact of unions is slight. Yet, many city managers and other public officials are afraid of the impact of such bargaining, sharing the reservations of Wellington and Winter. Collective negotiations will undoubtedly create problems for local administrators; these are manageable problems, though the efforts and priorities of officials may have to be reordered. Nevertheless, bargaining is bound to grow, and many more public officials will have to reckon with it. These books are valuable contributions to the literature on unionism and bargaining in the public sector.

GLENN W. MILLER

Wichita State University

The Rise of the City Manager: A Public Professional in Local Government. By Richard J. Stillman II. (Albuquerque: University of New Mexico Press, 1974. Pp. 170. $8.95.)

While no older than this century, the city manager movement has undergone a variety of distinct phases in its development. Richard J. Stillman II in *The Rise of the City Manager* offers a concise and comprehensive chronicle of this movement that should long serve as an excellent point of departure for further work in this area.

The most useful part of the book is the first three chapters, which trace the rise of the movement from the first well-publicized installation of council-manager government in the Staunton, Virginia, of 1908 to the revitalization of the International City Management Association in the mid-1960s. By the end of World War I, nearly a hundred jurisdictions had council-manager structures. During the following decade that number would quadruple because the growing complexity of the concerns of public management in the automotive age demanded administrators with technical – usually engineering – competence; and because the notion of "economy and efficiency" in government was given an added impetus by the widespread acceptance of scientific management, or Taylorism, in the private sector. Stillman compares Karl von Clausewitz's *Vom Kriege*, which provided an intellectual rationale for nineteenth-century military professionalism in Europe, to the scientific management movement, which furnished the twentieth-century profession of city manager with its intellectual and theoretical bases. The profession did not experience a comparable growth spurt until the post-World War II era when an expanding suburbia sought new institutional structures to cope with a new array of public facilities and intergovernmental concerns – not the least of which was insulation from the central city and the problems that the city both had and symbolized.

The historical portion of the book, its first half, is well written and exhaustively documented. While the overall presentation is thoroughly academic in tone, Stillman occasionally writes in a more lively style, as for example in his description (on page 15) of one of the publicists who first gave the city manager concept national prominence: "Richard Childs was the quintessential progressive reformer – a man of means, with a social conscience, a professional background, and extra time to devote to his pet hobby, municipal reform." Stillman's efforts at documentation are not limited to the ample footnotes; he thoughtfully provides a thirteen-page bibliographical essay on his sources as an appendix, which advises the reader that the extant literature is spotty and should be read selectively – all the more reason for first reading Stillman's essay.

The second half of Stillman's work is uneven. Chapter four offers a profile of the city manager that is based upon a 1971 questionnaire sent to a random sample of 11 per cent of the profession and published earlier as a monograph by the ICMA. The following chapter, which compares and contrasts city managers with career diplomats and school superintendents, is academic in the worst sense of the word. Stillman is too skilled a writer not to have a point; however, in this case it is forced. But he rebounds from this minor *faux pas* with his final chapter, an excellent analysis of the present dilemmas and future directions of the profession.

One of Stillman's more significant findings is that the politics/administration dichotomy, first proclaimed by Woodrow Wilson in 1887 and then reified by Frank J. Goodnow in 1900, is an archaic cross that the modern city manager must still bear. Citing the numerous studies on the role of managers in local politics and the urban policy decision process, Stillman asserts (on p. 104) that the "acid test of survival for managers is their ability to come to grips with the complex political realities of the communities in which they operate." Unfortunately, the persistent fictional doctrine of the total separation of politics from administration under the council-manager structure both strains the city manager's relations with his council and obscures the manager's valuable contributions to the politics of the community. There is no easy solution to this dilemma. The best city managers may well be the best hypocrites – always proclaiming their neutrality in political issues while at the same time working quietly to resolve those issues to the advantage of their community. The definitive book on the history and problems of the city manager's profession remains to be written; but Stillman's work is undoubtedly a leading contender for that accolade in the interim.

JAY M. SHAFRITZ

University of Houston at Clear Lake City

From Poor Law to Welfare State: A History of Social Welfare in America. By Walter I. Trattner. (New York: The Free Press, 1974. Pp. 276. $9.00.)

This simplistically written book is intended primarily for historians and social workers who wish to obtain some perspective on the magnitude and character of the problems with which they are dealing. But it may also be of general interest to those concerned with an interpretive history of social welfare policies in the United States. The author claims that most social welfare histories are accounts of various chronological periods of specific developments within the field such as child labor reform or housing reform and that his study assimilates numerous earlier studies to provide a broad overview of American social welfare. This book, however, does not satisfy that claim nor is it unique in that respect, although it does attempt to provide a general synthesis of accounts.

Two main trends are emphasized throughout the book. The first is the early belief that the only cause of poverty is individual weakness. The influence of the classical economists and the general laissez-faire philosophy that public relief tended to demoralize recipients and depress the standard of living in particular was based on the belief that accumulating wealth was a moral virtue and that dependency was a vice. If destitution was the individual's fault and most of the needy were recipients of help from the public treasury, it followed rationally that public aid was a cause of pauperism and thus inherently bad. If aid was to be bestowed it should be given to public institutions where careful control could be enforced. The Protestant ethic, which demanded benevolence of the rich but also hard work and morality of the poor, complemented the belief that the individual had the power to achieve economic success through his own efforts.

This emphasis on individual weakness and the visitation of God's wrath on the erring person gave way to a focus on environmentalism or the belief that one's economic and social surroundings affected the way one lived. From this new focus came discussions about helping delinquent children to lead productive lives by transplanting them to acceptable environments. Furthermore, society started believing that a dependent person needed its assistance and was not inherently evil. Michael Harrington's popular statement in *The Other America* highlighted this vicious circle of poverty which brought self-supporting families to the dependency level and kept them there.

The second point was reflected in the attitude of President Franklin Pierce to Dorothea Dix's appeal to Congress to appropriate ten million more acres to the states to help pay for the construction and maintenance of mental hospitals. Pierce vetoed the measure, saying in part: "If Congress has the power to make provision for the indigent insane . . . it has the same power for the indigent who are not insane," and thus all the nation's poor (p. 62). Harry Hopkins in *Spending to Save* illustrates the same sense of fear that came with federal government programs that dealt directly with the individual and did not rely directly on the state and local government for action. The flexibility of federalism worked against a single legal code affecting social welfare matters throughout the nation and continues to do so.

According to Trattner, the Depression answered the vexing question of whether private or public agencies should be responsible for relief giving. Voluntary charity simply could not cope with the situation, and only public agencies could deal with the collapse of the economy and mass unemployment. Although President Hoover adhered to Pierce's earlier thinking of self help, such thinking was doomed to national centralization through such programs as Social Security and the War on Poverty.

The book concludes abruptly, telling little about the 1960 and 1970 eras and ends on a somewhat syrupy plea for an understanding of social ills and the poor.

FRANK T. COLON

Lehigh University

Black Republicans: The Politics of the Black and Tans. By Hanes Walton, Jr. (Metuchen, N.J.: Scarecrow Press, 1975. Pp. 217. $8.00.)

In this work Professor Walton seeks to trace the impact of racial conflict on factional cleavages within the Republican party in the South after the Civil War, and particularly to locate those forces leading to the decline of the black-and-tan faction of the party. He charges that "although the Negro issue has occupied a central place in the development of national party organizations and in the formation of political alliances, its effect upon intra-party cleavages has been dealt with in only a peripheral and superficial manner" (p. 2). Walton's charge is startling to those of us who learned so much about race and politics in the South in this period from the works of the late V. O. Key, Jr. and the scholars he inspired to further research. Nothing in Walton's book challenges these writers' interpretations. But in bringing together a mass of hitherto scattered information on factional in-fighting within the southern Republican states and local parties after the Civil War, Walton adds depth to our knowledge of the decine and fall of biracialism in the GOP.

Black Republicans contains three major sections. The first reports the attraction of free blacks to the Republican party at its inception, and the mixture of idealism and opportunism that moved white leaders of the national party to champion the cause of emancipation and racial equality. The second and major section is a detailed narrative, for each of the eleven southern states, of struggles between the black-and-tan and lily white factions in southern Republicanism from the 1870s to the 1920s. Walton has prodigiously combed fugitive sources as well as published materials in order to give a minute account of state and local Republican factional struggles. These struggles, he finds, were fought with varying weapons and somewhat different objectives in the eleven states. In Louisiana, for example, the two factions largely resorted to legal actions in pursuing control of the state party machinery and convention delegations; in Texas, however, the factions primarily sought mass support at the ballot box. For each state, Walton carefully and sometimes vividly reconstructs the strategies and tactics of the black-and-tans and lily whites and reveals the strengths and weaknesses of factional elites. A final chapter links the fate of the black-and-tans to Republican national convention politics up to the Hoover period, when the lily whites were finally victorious and southern blacks began their exodus to the Democratic party.

The reader closes Walton's book rather stunned by a mass of names, dates, and events, yet with a deep overall impression of the often scurrilous measures used by both factions, the ineptitude of many of their leaders, the venality of national Republican party figures and their insensitivity to the plight of southern blacks in this period. Here is a portrait of American party politics at its worst. Yet political scientists are apt to come away from *Black Republicans* somewhat unsatisfied. Walton's study is primarily narrative and presents almost no systematic data analyses that might sharpen our knowledge of the similarities and differences in the two factions, nor does the author apply theoretical approaches that might relate his research to some broader framework of understanding. Considered as history, Walton's work is careful and thorough, yet his concentration on details of the intraparty battle leave the reader with a feeling that the possible effects of some broader historical forces remain unexplored. Walton seems not to rank Populism as a factor of great significance to the black-and-tan decline although, in view of the importance accorded to it by Key and other scholars, he might have been expected to consider it at greater length. And while he is undoubtedly right in highlighting the effects of white supremacist attitudes on the withering of the black-and-tans, he ignores the flowering of other ideological trends after the Civil War – e.g., the rise of social Darwinism and the economic gospel of individualism and competition – that might help explain the disappearance of white Republicans' zeal for racial justice. A full-scale analysis of the transformation of the Republicans from a biracial to a lily white party remains for the future, yet Walton's book will be an indispensible resource in any such analysis.

DOUGLAS GATLIN

Florida Atlantic University

U.S. Tactical Airpower: Missions, Forces, and Costs. By William D. White. (Washington, D.C.: The Brookings Institution, 1974. Pp. 121. $2.50.)

William White's book grapples with the problem of escalating costs in the procurement of tactical fighter, bomber, surveillance, and transport aircraft for the military services. In current 1975 U.S. dollars, the P-40 Fighter cost $54,000 when it became operational in 1940. According to White, the F-15, by contrast, will cost $9.8 million per copy in 1975, excluding research and development costs. Economies of scale no longer apply. In the 1940s, thousands of aircraft were made by assembly-line methods and the cost per unit fell as the number produced increased. Now much smaller quantities are produced, with aircraft largely handcrafted. Fewer aircraft can be procured, therefore, because each is so costly.

White's principal argument is not that the skyrocketing trendline for aircraft costs has been affected by pervasive price inflation in the U.S. economy. Rather he attributes the cause of rising costs to a peculiar American procurement strategy of maximizing unit capability so that each tactical aircraft is a multipurpose platform. The optimal aircraft, in this view, is both fighter and bomber combined, able to perform close-air support and deep interdiction missions, and capable of all-weather and night operations. White, a colonel in the U.S. Air Force, traces this procurement strategy ultimately to U.S. airpower doctrine which gives priority to defeating an opposing air force in air-to-air combat and in bombing its bases of supply. For tactical aircraft to survive in the hostile environment over an opponent's home territory as the drive for air superiority is conducted, heavy demands are placed on aircraft performance and survivability of the aircraft and crew.

The reader should note that the author makes a choice when he pursues the line of argument that rising costs for tactical aircraft are due to American procurement strategy. An alternative explanation, which he observes only in passing, would explore how general price inflation has affected tactical aircraft procurement as well as civilian consumer purchasing power for other far less sophisticated hardware. His own argument would be more persuasive if he had tried to persuade the reader initially to accept his basic assumption about the reasons for the increasing costliness of tactical aircraft. If he had laid the trendline for the decreasing real value of the dollar alongside his trendline for aircraft procurement costs, he might have been able to demonstrate some interesting correlations. His argument, however, might have suffered from the comparison.

Let us assume that aircraft costs are up because price inflation resists any correction. We are still confronted with the problem of what to do about burdensome aircraft costs. White may have misidentified its sources, but he has surely addressed an important problem which seemingly defies a solution. The solution probably lies outside the subject of White's book and, for that matter, even beyond the defense budget. Unfortunately, economists have not yet found an antidote to price inflation. White also misidentifies the solution by implicitly arguing that a change in airpower doctrine away from an emphasis on air superiority in an adversary's home air space and toward close-air support for the ground battle will cause less expensive special-purpose aircraft to be built. Undoubtedly, if we give up our traditional interest in air superiority, savings will result, but then we will be giving up the means of prevailing in a conflict. The lessons of the Yom Kippur war are still being debated, but if the Israeli Air Force had emphasized the close-support mission to the detriment of air superiority, its losses to Arab surface-to-air missile systems probably would have been even greater than they were.

White does not inform the reader of alterations in design engineering which can effect savings. He is more intent on changing mission emphases. And yet if missions do not change because of the bureaucratic inertia of the military services for which traditional roles and missions are as touchy and fundamental as ever, it would have been more realistic to identify changes in the technical parameters of aircraft, metallurgy, ordnance accuracy requirements, and other ways of reducing the cost of aircraft systems. The elaborateness of Air Force ground support facilities offers opportunities for savings. The staggering $1 billion cost of nuclear-powered aircraft carriers has stimulated a search for alternatives.

The reader must wait for another book, or for White's next book, to learn how the budget for tactical air power forces can be brought under control.

JACK M. SCHICK

Washington, D.C.

Local Administration and Politics in Modernising Societies: Bangladesh and Pakistan. By Najmul Abedin. (New York: Oxford University Press, 1974. Pp. 458. $14.00.)

The title of this book may mislead readers not familiar with governmental structure on the subcontinent because, as the author points out in the introduction, it is a study of subnational rather than local government. In addition, the relationship with local politics is given only marginal treatment.

Professor Abedin, now at Chittagong University in Bangladesh, obviously prepared most of this book, which was printed in Dacca, as a study of one nation and then made allowances for the emergence of Bangladesh. The manuscript was completed in 1973 and in no way anticipates the Bangladesh military *coup* of 1975.

The book deals heavily with prepartition influences on district government and relies heavily on secondary sources. As such, its greatest contribution is an historical analysis of the differences between Bangladesh and Pakistan and of the role and function of districts which were units of the provincial governments in Pakistan prior to 1972 and remain such today. In Bangladesh they have become arms of the central government. Chapter I, "The Evolution of District Administration," is an excellent summary of the wide range of material available about pre-British and British India. Here we have recounted the conflicts between Cornwallis and Munro reforms, Whigs and Utilitarians, separation and concentration of powers, and *Zamindari* and *Ryotwari* land tenure systems. Unfortunately, the discussion of the "political-social environment" in Chapter II is not of the same depth, reflecting obvious differences in the quality of source material. Administrative and social traditions are termed authoritarian and paternal in both nations, but religion is held to dominate political life more completely in Pakistan, while Bangladesh exhibits a stronger tradition of political participation.

The real focus of the study is the District Officer (Collector, Deputy Commissioner, Magistrate), and the author has a hard time merging his basic conclusions about the individuals who hold these hallowed jobs. He argues that they are authoritarian and paternalistic, representing an elitist tradition (p. 53); that they have declined in general intellectual ability, with serious consequences for district administration (p. 186); and "that the District Officer, whose authority remained almost unchallenged during the early part of this century, is now facing a serious challenge to his authority even from within the administration" (p. 359). In this sense, Professor Abedin reflects a conflict that has plagued both scholars and practitioners of local government in Pakistan and Bangladesh since 1947 – a theoretical desire for democratic institutions which is constantly offset by a traditional respect for authoritarianism. The end result is constant criticism of a governmental system dominated by elitist services existing hand-in-hand with a national reluctance to substitute any other form.

The book also demonstrates another continuing problem well known to political scientists who have worked in Pakistan – the extreme isolation of the academic community from the real world of government. Although major official documents are cited, a definitive analysis of the complex interaction between the provincial departments, particularly the local government departments, and district administration is lacking. No mention is made, for example, of the efforts to develop a Local Councils Service that went on in both wings of Pakistan during the 'sixties. The author evidences familiarity with available materials about East Pakistan-Bangladesh, but his knowledge of West Pakistan-Pakistan materials is limited, if the footnotes and the bibliography are valid measures. Consequently, a major portion of the book deals with what is now Bangladesh. Still, it provides a valuable summary of the role of the District Officer in British India and, to a lesser extent, in pre-1972 Pakistan. The book offers perhaps more than the average reader will want to know about land revenue administration but is superficial in its treatment of Village AID and the Works Programme and contains only a limited summary of local politics, the activities of local councils, and the Basic Democracies experiment.

The most penetrating portion of the book for readers searching for an understanding of the Bengali political system is the author's "Concluding Observations," a four-page section which is not, for the most part, an outgrowth of the earlier text. Here he makes the observation that local leadership began to make its existence felt during the 1960s, that during that time the Basic Democracies system encouraged a fusion of official and nonofficial agencies, and that communication between the bureaucracy and society, particularly rural society, was increasing. He also argues that because the BD system was an electoral college which served the political interests of an authoritarian regime, the local councils were severely undermined as effective units of local government.

These are hypotheses with which I agree, but to test them will require more than the predominantly historical account presented in this study. Unfortunately, that level of research is still not possible in Bangladesh.

GEORGE M. PLATT

Wichita State University

Agricultural Initiative in the Third World. By the Agribusiness Council. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. vi, 210. $16.50.)

Formed in 1967, the Agribusiness Council is a nonprofit organization of corporate and foundation members including, among others, United Brands, H. J. Heinz, Exxon, IBM, and Ralston Purina. The Council's expressed objective is "to aid in relieving the problem of world food supply through increased agribusiness investment in developing nations" (p. ii). In short, the Council seeks to locate investment opportunities for its members. In February, 1974, the Council organized a symposium of "Science and Agribusiness in the Seventies," and this volume consists of sixteen papers originally presented at the London conference.

At least four often mutually exclusive themes compete unsuccessfully for dominance within the book. First, several papers are devoted to a consideration of applied agricultural science's contribution to food production. These chapters generally reflect an optimism about the potential for dramatic advances expected in food output. The fact that technology cannot be isolated from its political, social, and economic context does not receive the attention it deserves here.

A second theme is that a reduction in birth rates is necessary to prevent massive famines in the underdeveloped world. The fact that poverty creates high birth rates, which in turn create poverty, is mentioned only in passing. There is no demonstrated familiarity with the rapidly burgeoning literature on socioeconomic determinants of fertility. Perhaps foreign agribusiness is not sympathetic to radical economic redistribution as the key to declining fertility.

Third, the discovery of the small farmer by the World Bank and the Inter-American Development Bank (IDB) comprises another group of essays. Both Antonio Ortiz Mena, president of IDB, and Montague Yudelman, director of Agriculture and Rural Development at the World Bank, recognize the necessity for reforms in land ownership and tenancy. Ortiz Mena believes this would create consumer demand for fertilizer and machinery. Neither paper sufficiently examines the resistance to such basic structural transformation on the part of internal elites and external allies.

Fourth, it is argued that if only they are given the investment incentive, multinational agribusiness corporations can solve the world food problem with their vision, "know-how," technological packages, and superior management judgments. In a section entitled "Agribusiness in Developing Nations," various schemes for attracting agribusiness investment receive attention. Firms have apparently hesitated to share their expertise because of "uncertainties about the future of agrarian reform and taxation policies" (p. 93).

Finally, Lester Brown of the Overseas Development Council and David Hopper, president of the International Research Center in Ottawa, contribute papers on "The Emerging Policy Issues." Hopper's upbeat conclusion is that food output can be tripled with proper financing and good will.

I found myself rather impatient with the assumption in this book that increases in food production will mean that hungry people will have enough to eat. The narrow perspective generally found here excludes the real possibility that food problems are rooted in a maldistribution of the actual means of food production. Brazil, and specifically the Industrial Development Institute (INDI) in the state of Minas Gerais, is touted as having successfully utilized the Council's skills in 1972. According to a contribution by Dr. Francisco Noronha, the Secretary of Commerce, Industry, and Tourism, INDI had promoted major agribusiness development by attracting considerable outside investment. There is absolutely no evidence in the book, however, to suggest that starving Latin Americans are eating better as a result or ever will; exporting beef to hamburger chains in the United States would seem to be of marginal value to the peasants. This raises the troubling question of whether the commercial objectives of multinational agribusiness in the Third World – cheap labor, low-priced crops, and tax advantages – have anything in common with the needs of hungry people.

Although a few of the selections, notably Brown's and Yudelman's, stray from a strictly agribusiness perspective, an obtrusive editorial commentary consistently trumpets a food-for-profits orientation, at times leaving the impression that the more thoughtful papers are functioning as camouflage for the Council's ultimate goals.

After noting that many countries, to their detriment, have discouraged private investment, the Council observes that "Perhaps the strongest message of this international conference was to the policy makers of these latter countries." To them, the business participants were saying: "We can be of help, but only under conditions that provide us a fair economic return for our efforts." The book provides substantial unintended support for declining this offer.

GARY L. OLSON

Moravian College

Gladstone and Radicalism: The Reconstruction of Liberal Policy in Britain 1885–1894. By Michael Barker. (New York: Barnes and Noble Books, 1975. Pp. 308. $17.50.)

During the late nineteenth century the British Liberals transformed themselves from a party of laissez-faire into a party of economic intervention and social reform, a change that culminated in the great reforming government of 1905–15 which laid the foundations of the welfare state in Britain. Barker details a moment in this transformation, the period between 1885, when the Radicals were a minority group in a Whig-dominated party, and 1892, when the party as a whole fought an election on a radical program, which included Church disestablishment in Scotland and Wales, electoral reform, reform of the land laws and local government, and measures to control conditions of employment in industry. The book also deals (unavoidably) with the issue of Irish Home Rule, and with the evolution of the Liberals from a parliamentary caucus to a nationally-organized party.

It emerges that these three political developments were interconnected. The conversion of Gladstone and the bulk of the Liberal leadership to Home Rule largely alienated the Whig elements in the party, including the landowning peers. This immediately changed the political complexion of the parliamentary Liberals: in the longer term, the radical cause was promoted still further by the withdrawal of Whig financial support and the party's increased reliance on its national organization (the National Liberal Federation and its Scottish counterpart), which was aggressively radical in outlook and moreover contained vocal Welsh and Scottish contingents. The final lurch to radicalism was also brought about by Irish events: the Parnell scandal in 1890 made Home Rule less attractive as an election issue, and Gladstone decided to fill the platform out with domestic reforms.

Barker's originality lies mainly in his reassessment of Gladstone's role in this transformation. The latter is often portrayed as unenthusiastic about radical reforms and preoccupied in his later years with the Irish question. Barker destroys this portrait and brings out the full ambivalence of Gladstone's character: on the one hand, the landowner and churchman who believed in the aristocracy's natural right and duty to govern the country; on the other, the committed social reformer who supported the aims of trade unionism and responded sympathetically to the striking dockers in 1889. This last reaction was again a result of events in Ireland, where the united action of the peasants against their landlords persuaded Gladstone that combination was the only effective way for the poor and helpless to obtain justice.

The book is written for a specialist audience and assumes a fair knowledge of the period's political history on the part of the reader. It begins and ends abruptly, without attempting to extract any general themes from the detailed discussion of political events which are the book's staple. This concentration of subject matter also means that no attention is given to general currents of political thought, or to the economic conditions which form the background to many of the events described. Within its chosen limits the book is both scholarly and readable, but it is rather less than a complete account of the emergence of a "new liberalism" in nineteenth-century Britain.

DAVID MILLER

University of East Anglia, England

Labour, Race, and Colonial Rule: The Copperbelt from 1924 to Independence. By Elena L. Berger. (Oxford: Clarendon Press, 1974. Pp. xiv, 257. $19.25.)

Among metal mining industries in Africa, the copper mining industry of Zambia is only exceeded in size and value of output by the gold mining industry of South Africa. It was established by British, American, and South African mining companies during the 1920s. The record of resulting relationships between the local subsidiaries of those companies, their workers, and successive territorial governments, before and after the birth of Zambia in 1964, is richly documented by archival materials, company files in particular, which have been available for scholarly research in Zambia.

Indeed Zambia's industrial enclave is renowned among students of labor history and urbanization in newly developing countries. It has even given rise to a distinctive tradition of sociological scholarship sponsored by the former Rhodes-Livingstone Institute, its Zambian successor, and the University of Manchester. Recently economists and political scientists have also mined nuggets of knowledge in these hills. Elena Berger's work, a superb history of industrial relations on the colonial era "copperbelt," meets an obvious need while it complements a number of other studies. Other writers have used materials that appear in Berger's account for diverse analytical purposes. Her book may now be read as a reliable and systematic guide to the documentary record itself.

What is there for the political scientist particularly to note in a history of industrial relations under colonial rule? To begin with, the employers were large international firms, and since there is an ongoing debate about how progressive or reactionary such firms may have been during the colonial era, students of that question would do well to read this book. They will find that the companies, the colonial government, *and* the African workers favored racially exclusive unions in opposition to the white miners' union, which wanted a racially united union, a closed shop contract, and a policy of equal pay for equal work. Careful readers will discover that sound trade union principles may lend themselves to reactionary uses in colonial situations. In colonial settler societies all industrial relationships are distorted by the mechanisms of national or racial domination. This is not exactly news. But succinct and definitive studies, like Berger's, ought to be read by those who venture to make broader assessments of the relationship between capital and labor in ethnically divided communities.

Increasingly, students of multinational enterprise may seek precedents for effective action by international labor organizations in their confrontations with transnational corporations. Dr. Berger has recorded an instructive instance of international labor action that rescued the inexperienced and factious African mineworkers' union from the brink of a catastrophic defeat in the mid-1950s. Similar actions may help to secure the rights of labor in numerous countries today.

Finally, Berger shows that public authorities in colonial Zambia (then Northern Rhodesia) repeatedly relied upon the mining companies to initiate and implement policies of broad social and political significance. For example, from 1954 onward it was public policy to abolish the industrial color bar. Public authorities, however, spinelessly shifted the burden of carrying out this policy from themselves to the mining companies. True to form, the colonial government tried to get a cheap industrial solution for one of its big political problems. To be sure, the companies went along, but resentfully and with self-serving conditions that were detrimental to the public weal. In brief, the cheaper immediate solution entailed excessively high long-term social costs: in this case, a "dual" or racial wage system that could only be abolished at great national expense to the detriment of sound policies for balanced national development.

This lesson is relevant to studies of the planning process in all countries which depend upon private enterprise to effectuate major public policies. It is surely relevant to newly developing countries which depend upon the operations of transnational firms. It also warns us to be wary of the political doctrine of corporate social responsibility.

RICHARD L. SKLAR

University of California, Los Angeles

Israel, The Korean War and China: Images, Decisions and Consequences. By Michael Brecher. (Jerusalem: The Jerusalem Academic Press for the Institute of Asian and African Studies, Hebrew University, 1974. Pp. 148. $6.50.)

Professor Brecher has written a detailed analysis of a nonevent, and a minor nonevent at that: Israel's failure to establish diplomatic relations with the People's Republic of China. But this little book is not to be dismissed: though its substantive content is of exceedingly narrow interest, its method on the other hand is of very general interest indeed. Brecher has a model (he prefers "research design") which is sufficiently structured to be presented in diagrammatic terms. The first element of the model is the "objective" operational environment, consisting of (A) external factors: the global, subordinate (i.e., Israel-centered), other subordinate (i.e., regional, *per se*) dominant-bilateral (i.e., Israel-USA) and (other) bilateral, systems; and (B), the internal factors: military capability, economic capability, political structure, interest groups and elite competition. This objective world is perceived (through a "communications" layer) by the decision-making elite, whose attitudinal prism is formed by images centered on a prior selection of the salient "objective factors." For example, elite actors directly concerned with U.S.-Israel relations will accord priority to the "dominant bilateral relations" factor, and their perceptions will be shaped by that particular facet of the prism. This entire psychological environment will then in turn shape elite conduct, as manifest in the formulation of "strategic" and "tactical" decisions in four major "issue areas": military-security, political-diplomatic, economic-developmental and cultural-status. Finally there is the implementation of decisions by the "service structures" to which the various elite members have access, or in which they have control. The outputs in turn feed back into the operational environment . . . and so on.

Some will dispute the validity of Professor Brecher's model on epistemological grounds, and some may reject his implied distinction between the realm of objective facts and the "attitudinal prism" through which these facts are perceived. Others may object to his subordination of the purely bureaucratic element in decision making (in the model decisions are mechanically implemented by "structures," and not reinterpreted to fit bureaucratic preferences). But the best argument for the model is Brecher's own use of it to tell his tale, and explain the course of events.

Briefly the facts are as follows: on January 9, 1950 Israel extended *de jure* recognition to the PRC, being among the first non-communist countries to do so. This "strategic" decision – to use Prof. Brecher's terminology – was the logical concomitant of a higher-level strategic decision, Israel's neutralist stance. Chou En-lai responded warmly to Israel's cabled statement of recognition. But Israel made no move to establish diplomatic relations. On June 20, 1950, the Chinese DCM in Moscow approached his Israeli counterpart on the subject. Five days later, before a reply could come from Jerusalem, the Korean War began. The war and its tensions soon forced Israel to abandon its preferred policy of "nonidentification." At the UN, Israel consistently supported the U.S., while reiterating its recognition of the PRC's legitimacy. Another opportunity came after the Korean War, and it was missed again. By April 1955 it was too late: at Bandung the Chinese found the Arabs, and by May 1955 the Chinese flatly rejected a belated Israeli approach on the establishment of diplomatic relations. There matters have rested ever since.

Brecher's very detailed account carefully differentiates between the various protagonists on the Israeli side, and their different perceptions of diplomatic opportunities and diplomatic costs. We follow events as the protagonists saw them, or at least as they said (or wrote) that they saw them – in the framework of the "research design" whose structure is also the structure of the book. The result is a most instructive case-study of collective decision making. For example, at one point Israel was seemingly deterred from establishing diplomatic relations with China by a warning from Under Secretary Bedell Smith that American and American-Jewish opinion would react very negatively. Then we discover that this opinion was convened at a meeting solicited *by the Israelis themselves,* in pursuit of instructions from Jerusalem to "consult" with the Under Secretary. As a protagonist pointed out to the author, *any* approach to the State Department was bound to elicit a negative reaction. Had a real decision been made in Jerusalem, the ambassador would have been instructed to explain Israel's policy and not asked to consult the State Department on its formulation. This explanation induces us to reconsider the images that guided the conduct of the actors in Jerusalem, and to reconsider the validity of their explanations of Israel's diplomatic defeat. It becomes apparent that a shorter-term Euro- and American-centered view of the world prevailed over a minority longer-term and Asia-conscious view. The State Department merely provided ammunition for a battle already decided.

Here as elsewhere, Brecher's model is actually useful as an explanatory tool, even in the presence of what is seemingly *all* the evidence on a bit of microhistory. This alone makes it a *rara avis.*

EDWARD N. LUTTWAK

The Johns Hopkins University

Interdependent Development. By Harold Brookfield. (Pittsburgh: University of Pittsburgh Press, 1975. Pp. 234. $8.95.)

This is an important book for political scientists. As its author states: "This is not a book about development. It is a book about ideas about development. . ." (p. ix). As such, it at least touches upon most contemporary thinking about economic development; but more importantly, in so doing, the work may well realize what must be its author's main purpose – namely, to stimulate thought which, when considered collectively, will provide theoretical constructs useful to the solution of the many problems within which the economic development crisis is embedded.

As the book's title suggests, the author rejects the stale "have-have not" approach to economic development and thus, most of the theory and research based upon that approach. Instead, he defines development as ". . . the whole process of change brought about by the creation and expansion of an interdependent world system" (p. xi). While he realizes that this definition, as well as the approach it implies, ". . . is so broad as to be meaningless. . ." (p. xi), it does allow for a maximization of objectivity such that "development is therefore positive and negative, according to one's goals. . ." (p. xi).

Given this point of departure, Professor Brookfield introduces relevant thinking from various disciplines as well as from various ideological perspectives within disciplines. The ideas analyzed range, for example, from those of Joseph Schumpeter and Francois Perroux through those of Albert Hirschman and Gunnar Myrdal to those of John Friedmann and Brian Berry. In addition, the ideas of those who have had a more direct impact upon public policy formulation, e.g., W. W. Rostow and Raul Prebisch, are examined with special care. Finally, the ideas of the Marxists, both old and new, are explored, not as a body of useless dead-end thinking, as is so often the case, but as a creative and dynamic, albeit limited, part of development theory.

It is in this last area of relevant thinking that Brookfield is most open to criticism, not because of what he covers, but because of what he does not cover. I am amazed that there is not one mention made of Paul Baran throughout the entire volume. Baran's writings, especially his *The Political Economy of Growth*, are seminal to two decades of neo-Marxist development theory, including much of that discussed by Brookfield, and have influenced many non-Marxist development theorists, including some of those discussed by Brookfield. Further, Baran's contributions in the area of development are based upon the argument that development is indeed interdependent. Brookfield should have devoted at least some space to indicating why he feels Baran's ideas are irrelevant to the issue at hand.

In his treatment of "ideas about development," Brookfield relies heavily upon several concepts, the appropriate consideration of which should stimulate relevant and useful thinking. Brookfield is clearly too sensitive to the complexities of the economic development crisis to establish a model for the achievement of this purpose. Instead, throughout the book, he develops several dimensions upon which he obviously feels future theory and research ought to be based. In relation to development, these dimensions include: (1) its historical nature; (2) its spatial nature; and (3) its structural nature. While these are superficially very general categories, within the context of "ideas about development," Brookfield disassembles each of them and suggests how each might be reassembled, leaving the final decision concerning reassembly, however, up to the empirical results of future research based upon one or more of the dimensions. It should be added that even though the dimensions indicated constitute the unifying principle of Brookfield's effort, he does not ignore those dimensions upon which perhaps too much of the extant theory and research about development are based.

A final note: Harold Brookfield is a geographer with both academic and field experience. As a result, he brings a refreshing view of reality to a body of thought which thus far has been introduced to political scientists primarily by economists. Thus, assuming that none of the above remarks have been sufficiently motivating, reading this book should at least broaden the relatively limited perspective from which most political scientists view the economic development crisis.

PHILLIP ALTHOFF

Kansas State University

Legitimacy and Ethnicity: A Case Study of Singapore. By Peter A. Busch. (Lexington, Mass.: D.C. Heath & Co., 1974. Pp. 157. $14.00.)

Peter Busch is interested in the Lipset theory that economic development breeds legitimacy, and in exploring the effects of racial attitudes upon legitimacy. In 1970 he and his wife administered 3,316 questionnaires to Singapore secondary school pupils, and he conducted several intensive interviews as well. Utilizing linear regression coefficients and multiple correlations he finds that many more Malays (especially poorer ones) feel inferior than do Chinese. Among Chinese, there is strong correlation between a feeling that Malays are inferior and that they are advancing more slowly than Chinese, but perceptions by Malays that Chinese are advancing actually seem to ease their inferiority feelings a bit. Those respondents (Chinese and Malay) who feel their race is inferior, and who feel that Malays are advancing more slowly than Chinese, also tend to score lower on the legitimacy scale. Significant numbers of poor respondents score high in legitimacy, however. Malays who score high often do not believe that racial equity exists, but do believe that the government wants such equity. Those Chinese who believe the Malays are making progress are more apt to want multiracial neighborhoods. Those in nonintegrated neighborhoods are more allegiant to Singapore than those in mixed neighborhoods. Years in school tend to weaken legitimacy feelings, and strengthen racial prejudice. Although Professor Busch does not mention it, his conclusions do not match Singapore's orthodoxy; they suggest that mixing neighborhoods and indoctrinating school children does not necessarily produce allegiance or reduce racial prejudice.

Of course, one should not take any of this too seriously. The author does not reproduce the questionnaire or any numerical breakdown of responses (e.g., what percentage of respondents scored high on legitimacy?); the distribution curve seems to be quite narrow (there might be a preponderance of high scores, and little variation in responses among various groupings, so even very minor differences become statistically significant). He gives no information about the isomorphism of his sample, save for a comment that the elite English school portion of it is overweighted with wealthier pupils; in the absence of more extensive categorical grouping it is impossible to tell how subsets of the sample responded (does legitimacy decline with grade level in nonelite schools, or among those most racially prejudiced?). The R^2 in his tables (the proportion of variation explained by all the variables in the table *combined*) is often very small (e.g., all six variables in this table together can only explain 30 per cent of the variation in response to these questions). His more elaborate "policy equations" (p. 106) yield little information about the reliability of the parameters or the estimating equation itself (e.g., the standard error); there is no way to identify which of the thirty-seven variables are the most significant. His legitimacy scale is based on two questions: whether you would take a much lower-paying job if the government asked you to, and how willing you are to fight and die for Singapore. Yet he *defines* legitimacy as a feeling that political institutions are right and proper even when coercion, fear, and immediate material incentives are absent. Coercion and incentives are not absent in Singapore, even for jobless pupils; he did not ask whether the respondents would be willing to fight even if there were no coercion to do so, and a person answering "no" to these questions is not necessarily expressing disapproval of political institutions.

The findings seem, if anything, to point to a need to publicize more strenuously the economic advances of both races, to keep neighborhoods segregated and morally integrated, and somehow to reform English-stream schools. Busch instead calls for winning over cultural leaders, uplifting the poor, and inducing transactions that cross racial lines; the relevance of his findings to these prescriptions does not register with me. There is perhaps some very qualified support for Lipset here (though Busch does not focus it), but little to conclude about the relationship between ethnicity and legitimacy. He does ask (but not answer) some interesting shotgun questions about legitimacy: does it aid stability, how is it transmitted, how do you adapt socialization so as to create it, whether a person morally integrated into a neighborhood might thereby accord legitimacy to the state as a sort of overflow effect, how people separate state and government in their legitimation, etc. A journal article would have sufficed to present the data, and might have forced the author to focus more sharply on some conclusions.

ROBERT E. GAMER

University of Missouri at Kansas City

Political Change in Britain: The Evolution of Electoral Choice. By David Butler and Donald Stokes. 2nd Edition. (New York: St. Martin's Press, 1969, 1974. Pp. 500. $19.95.)

In 1969, David Butler and Donald Stokes published the first edition of *Political Change in Britain.* Based on data collected in three national surveys (1963, 1964, and 1966, with additional reference to 1959), the volume combined the schools of electoral analysis of the University of Michigan's Survey Research Center and the detailed historical work of Nuffield College, Oxford. This *tour de force* won the Woodrow Wilson Award of the American Political Science Association as the best book published in political science in 1969.

To issue a second edition of the work that is not merely an updated gloss merits additional critical praise. Butler and Stokes have extended the study through 1970 by adding several new surveys: The initial panel study was re-interviewed in 1970; a completely new set of respondents was examined in 1969 and again in 1970; and an additional group was surveyed in 1970. The second edition combines all the surveys into a comprehensive account of British electoral behavior. In doing so, Butler and Stokes have covered a period of Tory as well as Labour rule and the entry of a new generation of voters into the electoral market. There is little reason to doubt that their findings are of general significance, reflecting more than the peculiarities of a specific time frame.

A review of the second edition must serve at least two purposes. Not only should it comment on the additions but it should take the opportunity to delve more deeply into specific elements of the authors' arguments. In the second edition, Butler and Stokes have not only provided new data but they have modified the volume's structure. The "replacement" of political generations is now given the same emphasis as the analysis of party alignment and short-term flux. In their treatment of "replacement," they have elucidated a phenomenon of crucial theoretical importance. Political science has been very slow to examine the consequences of the general population growth that has characterized the world in the postwar years. It would be very surprising if the increases in the relative size of the elderly and the differential birth and death-rates of social class, ethnic, and sex groupings did not have important political consequences. Butler and Stokes have demonstrated that they do, and they have shown how to use demographic change characteristics as explanatory variables. This is illustrated particularly well in their definitive solution to the theoretical puzzle of the "Working Class Tory."

The authors have also amplified several of the "gems" of the first edition. The explanation of the symmetrical national vote swing and the exploration of the absence of issue coherence among the electorate are retained and joined to an updated analysis of the pattern of vote switching and a critique of the argument linking electoral changes to fluctuations in the economy. Each of these is a standard for the analysis of crucial theoretical issues as well as a description of British voting behavior and public opinion.

The study rests on an "open framework," which seeks to encompass numerous foci in the analysis of electoral change. It is this "open system" that gives the volume its encyclopedic quality and its immense utility as a data resource. At the same time, it provides an air of theoretical confusion. Two small but significant examples reflect ambiguities in choices of explanatory and descriptive variables: in the second edition, the "pattern of housing" emerges as a significant explanatory variable for persistence and change in voting behavior. There is, however, no discussion as to why it "emerged" in 1970 but was of little importance in the first edition, and, therefore, there is no attempt to assess its theoretical consequences. Apparently in response to earlier criticisms, in this edition, Butler and Stokes use the standard polling definition of the working class. As they note, the categorization into classes is crucial to the analysis, but they neither provide a theoretical justification nor explore the implications of that decision.

A fundamental difficulty associated with the use of an "open analytic framework" is that it is not conducive to the elaboration of interrelated explanatory propositions. As a consequence, the theoretical significance of the empirical findings is diminished. Both editions emphasize two major elements of long-term change: (1) a cleavage in the oldest age cohort based on religious ties and (2) the growth of class-based politics. The second edition also reports the "paradoxical finding" that young voters are less likely "to see politics as an arena of class conflict" (p. 411). Butler and Stokes use this "aging of the class alignment" to account for the declines in voter turnout and the increases in voting flux present in the late 1960s. Their attempt to explain these changes uses several explanatory factors: e.g., general growth in affluence, change in generational life-styles; changes in the appeals of party leaders. While these factors are not mutually exclusive, they derive from different theoretical positions and have different theoretical consequences: Will Britain's recent economic travail awaken notions of class conflict among the young? Will changes in the goals and actions of the party leaders effect greater class and party loyalties? Do the changes evident in the new generation indicate the wave of the future? Butler and Stokes provide no answers. Their concern with detailed descriptions precludes the elaboration of theoretical arguments.

Furthermore, the complexity of their framework makes it particularly difficult to join arguments found in the volume. A potentially fascinating response to the problem of the aging class alignment is to link it to the massive growth of new voters in the late 1960s. If, as Butler and Stokes argue, new voters are most prone to weak political loyalties and to vote-switching, can we account for the apparent decline in class-based voting by the difficulties inherent in socializing large numbers (the products of the postwar population growth and changed electoral laws) and to new voters' tendency to be vote-switchers? If so, will there be a return to the traditional electoral divisions as they "settle in?" Will they be socialized by the traditional political parties or new competitors? Will they remain a relatively undifferentiated political generation? There is neither an attempt to interrelate the phenomena nor to respond to the particular issues.

The answers to these questions will tell us much about the future of British politics but also about central theoretical issues. For Butler and Stokes to have sought to answer them would not have required them to don the garb of prophets, as they imply. Rather, it would have demanded the abandonment of their vague "open analytic framework" for the elucidation of precise theoretically interesting arguments. Their diversity of approach naturally results in a *vision* of highly complex change.

ALAN S. ZUCKERMAN

Brown University

Latin America: The Struggle with Dependency and Beyond. Edited by Ronald H. Chilcote and Joel C. Edelstein. (New York: Halsted Press of John Wiley & Sons, 1974. Pp. 781. $24.75, cloth; $9.50, paper.)

In an introduction, the editors outline the role of foreign economic factors in shaping domestic political and social structures in Third World nations, warning the leaders of the developing nations that natural resources are not abundant enough to provide the North American consumption level for as little as half the world's population, even allowing for technological innovation. Then other scholars, each dealing with one Latin American nation, give detailed critiques of underdevelopment in Argentina, Chile, Cuba, Guatemala, Brazil, and Mexico. Three of these countries – Argentina, Brazil, and Mexico – have significant industrial sectors in regional and world terms, yet the progress of industrialization in these nations has not lifted them from developing to developed in either the economic or political spheres.

From the standpoint of scholarship, the outstanding nation study of the six is on Cuba, by Donald W. Bray, political scientist, and Timothy Harding, historian, both at California State University at Los Angeles. They manage to chronicle the successes and failures of the Castro revolution without omitting setbacks, unlike those specialists on Cuba who portray Castro favorably at the cost of accuracy. Bray and Harding capture the significance of the basic changes in Cuba during the past sixteen years without either apologia or polemics.

Bray and Harding see the Committees for the Defense of the Revolution, which exist in every neighborhood of every town, as representing positive mass support for the Revolution. Unfortunately, they do not see the CDR as threats to the privacy and thoughts of Cubans as individual human beings. They correctly perceive the Cuban government's professed rejection of meritocracy, a point missed by many writers on Cuban politics. That is, the Castro regime rejects the notion that those endowed with greater intelligence should live better than others, though in fact the opposite happens in practice. The authors also point out that the Cuban Revolution firmly anchored the Third World struggle in the Western Hemisphere.

James D. Cockcroft, a sociologist at Livingston College, Rutgers, in analyzing Mexico, defines Mexico's mixed public-private economy as state capitalism, with the government controlling a higher percentage of major corporations than do private Mexican investors. Foreign capital remains the key junior partner, though President Luis Echeverría in recent years has expropriated new industries ranging from tobacco to motion pictures. Cockcroft cites several million Mexicans with marginal living standards – with rural Mexicans being more impoverished than urban residents – as a shortcoming of land reform programs. Despite such failures of the ongoing Revolution, he found that the aroused alienated groups were fragmented and that enough of the dissidents had been co-opted into the system to help the power elite sustain that system.

Brazilian sociologist Thetonio Dos Santos finds his country, one of the largest nations in the world both in land area and in population, not to have risen above underdevelopment despite its economic vigor. Even with energetic industrialists, Brazil retained its rigid dependence on the agrarian-export sector. Its industrial elite after 1937, aware of the weaknesses of other classes, knew how to mobilize its own class to defend its interests and to manipulate the government. Industrialization meant not only substitution of imported manufactured goods by locally produced items, but also the creation of new commercial relations characterized by importation of machinery and intermediary products. Foreign capital retained control over the machines and intermediary goods, giving Brazil a dependent industrialization. Dos Santos traces the failure of nationalism to evoke successful political radicalization, resulting in military rule since 1964.

One of the nations studied, Argentina, escapes the population explosion bedeviling most Third World nations, yet it continues to suffer inflation, violence, and political chaos. With an annual increment in population of only 1.5 per cent, Argentina cannot blame a baby boom for its triple-digit inflation in 1974 and 1975. To document the poor political and economic leadership, Juan Corradi, a sociologist at New York University, gives an historical overview from the nineteenth century through the return of Juan Perón in 1973, told in terms of class conflict.

Italian and Spanish immigrants in the 1880s kept Argentina ethnically European, free of the problem of integrating large numbers of Indians into the national mainstream as in many other Latin American nations. After some decades of middle-class power, 1930 is cited by Corradi as the year the landed upper class returned to political power under the imperative to industrialize. He categorizes the new industrial bourgeoisie as politically impotent and traces the power gains of organized labor under the Perón dictatorship. Argentina went from militarism to populism. Corradi found that after 1955 foreign capital was the powerful political factor. He de-emphasizes the decisions of Argentine leaders themselves in the failure to expand the economy without running the printing presses, which gave Argentina runaway inflation based on currency not backed by any increased productivity.

Susanne Jonas, a staff member of the North American Congress on Latin America, a private research agency, deals with Guatemala. She found that during the colonial era, Spain put Indian lands into huge estates owned by a few Spaniards, a pattern which has carried over into modern times. With the loss of British Honduras, Guatemala lost mahogany exports to Britain. Coffee became a major crop despite investments by United Fruit Company in banana production. In the 1960s, United Fruit sold its banana plantations, shifting to the less risky marketing and processing of bananas for export sales.

The Guatemalan government in 1968 took over installations of the International Railroads of Central America after the corporation became insolvent. This rescue operation cost the Guatemalan treasury $31 million in government loans and unpaid wages. Jonas found Guatemalan industrial technology to be capital-intensive rather than labor-intensive, using machinery to do a job which might have employed dozens of workers, to keep production costs competitive in world markets. Industrialization failed to alleviate serious unemployment. The value added from manufacturing more than tripled between 1950 and 1969, but industrial employment remained at 11.5 per cent of total employment from 1950 to 1964.

She summarizes Guatemalan violence as police-state law and order. Both leftists and rightists have murdered many opponents. Jonas contends that the rightist actions were indiscriminate whereas the leftist actions were discriminate. She admits guerrillas kidnapped third-party victims and killed diplomats, including U.S. Ambassador Gordon Mein and West German Ambassador Count von Spreti. The rightist White Hand machine-gunned a beauty contest winner and a congressman in a wheelchair. Yon Sosa guerrillas shot thirteen-year-old track stars. Political bloodshed has been too extensive to draw up a complete list of victims and motives. Jonas found that marginalization of the lower classes yielded institutionalized unrest. The government cannot make necessary reforms and thus continues to rely on force.

Marcelo Cavarozzi of the Di Tella Institute of Buenos Aires and James F. Petras, at State University of New York, Binghamton, cover Chile. They analyze the presidency of Marxist Salvador Allende sympathetically and that of Christian Democrat Eduardo Frei critically. The Christian Democratic policy was to achieve or to seek economic development through copper production in mixed public-private corporations with the government being the senior partner. Frei's gradual pace is contrasted unfavorably by the authors with the radical changes of Allende. But they do not include in the comparison the world's worst inflation, at the runaway rate in the increased cost of living of one per cent per day, resulting in a takeover by right-wing generals who have suppressed all political freedoms. Over all, this volume provides useful background for those seeking political perspective for Latin America's economic underdevelopment.

MARVIN ALISKY

Arizona State University

The Last Days of United Pakistan. By G. W. Choudhury. (Bloomington, Ind.: Indiana University Press, 1974. Pp. 239. $10.00.)

G. W. Choudhury is eminently qualified to write a book on the events surrounding the loss of East Pakistan in 1971. The author is a Bengali who for many years taught at Dacca University (now in Bangladesh) and later served in a number of capacities in the Pakistan government. When the curtain came down on the Bangladesh tragedy, Choudhury was a special counselor on constitutional affairs to President Yahya Khan. Since those fateful days, he has made his home in the United States, where he is professor of political science at North Carolina Central University. Choudhury identifies with that element which wholeheartedly supported the original Pakistan movement. Unlike so many Bengalis, he did not succumb to provincial passions, and this more recent of his many books is an important personal account of the final chapter in the quest for a semblance of Muslim unity in South Asia.

In examining Bengali nationalism, Choudhury is very critical of all those Pakistani leaders who from the inception of the state, ignored every opportunity to ameliorate East Pakistan's grievances. Political inferiority and economic disparity fueled Bengali emotion, and the Islamic religion could not solidify a multi-ethnic and territorily divided state. The author comments that

> a federal union can be strengthened by giving cultural freedom and autonomy, but Pakistan's attempt to impose uniformity where diversity was desirable had unfortunate consequences. Every attempt made by the Pakistan government to foster a cultural uniformity based on Islamic culture in East Pakistan produced a sharp reaction; the Bengalis began to look more and more to West Bengal [in India] for cultural affinity and bonds (p. 11).

Choudhury is emphatic in the argument that Pakistan's leaders played into the hands of those Indians who were waiting for an opportunity to fracture the Pakistan design. In the culmination of their self-fulfilling prophecy, Pakistanis who had always insisted India would sever East Bengal from Pakistan as a prelude to destroying the entire state, did virtually nothing to preserve their tenuous unity.

The crucial period for Choudhury is the Ayub Khan decade (1958–1969). Although he admires Ayub the man, the author asserts that Ayub was no statesman and hence was easily misled by his "friends" as well as his foes. Choudhury analyzes the decisions to precipitate a war with India in 1965, to accuse Mujibur Rahman of treason and try him in the infamous Agartala Conspiracy case, and finally, to celebrate the regime's "success" during the Ayubian *development decade*. In each instance, Ayub was misguided or deliberately misinformed by his advisors, explains Choudhury. Moreover, after viral pneumonia struck him down and left him physically and mentally weak, there was a concerted effort to have him replaced. Choudhury would have us believe that Ayub could have managed the storm that blew up around him if he had endeavored to satisfy Bengali demands. But Ayub could not take the measure of the situation, and his ignominious demise heralded the eventual breakup of the Muslim state.

Yahya Khan, Ayub's successor, was given one last chance to salvage the Pakistan dream. Choudhury apparently was more intimate with Yahya than with Ayub. He therefore tends to be somewhat apologetic when describing his role in the unfolding scenario. Yahya, he tells us, tried to preserve the unity of Pakistan while politicians like Zulfikar Ali Bhutto and Sheikh Mujibur Rahman were "playing unscrupulous roles" (p. 163) and seemed not to care what happened to the country so long as their personal ambitions were gratified. But Choudhury's attack is not only directed at Mujib and his Awami League followers, or Bhutto and his Pakistan People's Party, or indeed, even at those members of the military junta whom Yahya was compelled to accommodate. Yahya might well have come to terms with Mujib, says Choudhury, had it not been for the Bengali economists and some foreign economists "financed by the Ford Foundation" who "were the loudest in making extreme demands, and they were, to a large extent, responsible for the failure of the Dacca dialogue" (p. 168).

Choudhury condemns the atrocities committed by the Pakistani army against the Bengali population but he also feels that some display of military power was essential given Indian machinations. Moreover, the Soviet Union's decision to favor India in its running dispute with Pakistan is an added feature in the drama.

The Last Days of United Pakistan makes fascinating as well as informative reading. Although it must be categorized as a largely subjective account, it represents the first *inside* interpretation of Pakistan's most trying hour. It cannot be the last word on the subject, however, and one can only hope that others with first-hand knowledge will see the necessity of rendering their versions.

LAWRENCE ZIRING

Western Michigan University

The Labour Party and the Struggle for Socialism. By David Coates. (New York: Cambridge University Press, 1975. Pp. 257. $14.95, cloth; $5.95, paper.)

Professor Coates examines the role of the Labour party in the creation of a socialist Britain. The author thinks of socialism as involving common ownership of the means of production, distribution, and exchange. He analyzes the history of Labour party programs and policies advocated and implemented with a view to assessing the Party's potential for attaining this socialist goal. Basically Coates argues that if Labour's past is prelude to the future, then parliamentary socialism will prove to be an inadequate vehicle for the creation of a socialist society and should be replaced by revolutionary socialism. The author hopes that this analysis will initiate further debate about whether or not the Labour party has been, could be, and should be socialist.

The main body of the book focuses on the least controversial aspect of this debate: historical analysis of the extent of socialism advocated and implemented by Labour leaders and enumeration and evaluation of the obstacles which precluded further socialist progress. The first four chapters examine the early Labour party, the Labour Governments of 1945–1951, the Labour party in Opposition 1951–1964, and the Labour Governments of 1964–1970. This chronological and detailed analysis of the statements and policies of various Labour leaders is followed by chapters on "The failure of the socialist promise," "The weakness of the Labour Left," and "The exhaustion of a tradition," which summarize in more general terms the author's analysis of the obstacles to socialism and recommend action, such as the formation of a revolutionary socialist movement, which socialists should take in the future. The inventory of obstacles to socialism assessed by the author includes not only long-standing institutions such as the civil service and perennial problems such as the balance of payments, but also contentious policies within the Labour party itself such as the commitment to parliamentary socialism and Labour's failure to mobilize a radicalized working class.

Coates apparently believes that the controversial element in his analysis is not so much his interpretation of Labour history as it is (1) his projection that Labour's past indicates that the Party will never be capable of implementing socialism as he defines it, and (2) his recommendation that the parliamentary route to socialism should be abandoned in favor of revolutionary socialism. These controversial conclusions are at variance with the opinions of authors such as Eric Heffer and Ken Coates who disparage revolutionary socialism and argue that socialist change can still be achieved via parliamentary action by the Labour party. (See Eric Heffer, *The Class Struggle in Parliament* [London: Gollancz, 1973] and K. Coates, "Socialists and the Labour Party," in *The Socialist Register 1973,* ed. R. Miliband and J. Saville [London: Merlin Press, 1974], pp. 155–178.)

In summary, the author does a good job of demonstrating that based on its past record, the present-day Labour party is unlikely to be able to implement socialism as Coates defines it. Although this conclusion seems to be neither very original nor very controversial, it is perhaps a necessary step in any analysis of the Labour party's socialist potential, for Coates claims that revolutionary socialism will not gain acceptance until it is clearly demonstrated that present Labour party trends will not lead to socialism.

The major weakness of the book lies in the author's failure to support his claim that the "revolutionary Left need not always fail" (p. xi). Coates is hopeful about the possibilities of forming a revolutionary socialist movement because the Labour party's "total domination" of working-class political loyalties is fragmenting, thereby allowing the growth of a revolutionary socialist Left. He does not, however, explicate strategies which would or could be used by revolutionary socialists in coping with British economic problems.

This criticism would come as no surprise to Professor Coates who himself anticipates this argument in his own Preface and answers in rebuttal that loyalty to revolutionary socialism can only form after the parliamentary road to socialism has been demonstrated as inadequate. I feel that Coates's rebuttal begs the question, since it provides no indication of how revolutionary socialism would deal with concrete problems.

LYNDELLE D. FAIRLIE

San Diego State University

The Chinese Red Army: Campaigns and Politics since 1949. By Gerard H. Corr. (New York: Schocken Books, 1974. Pp. 192. $8.95.)

The People's Liberation Army (PLA), the armed services of the People's Republic of China, is one of the key institutions to be considered in the analysis of politics in contemporary China and of the socioeconomic transformation of that nation. The importance of the PLA has been reflected in the work of long-standing merit of such China scholars as Chester Cheng, Ellis Joffe, John Gittings, and Ralph Powell. Since the time of the Cultural Revolution in the late 1960s, research and analysis focusing on the PLA has received additional impetus resulting from the new levels of involvement in China's political life by the Army. Among these more recent efforts are the work of William Whitson and of Ying-mao Kau. By and large, however, these writings tend to be addressed to scholars concerned with other aspects of Chinese politics rather than to students of other societies or to the general reader. A book which satisfied these broader needs would undoubtedly be welcome.

Gerard Corr, a British journalist with experience in Asia, claims, rightfully, that his book, *The Chinese Red Army,* "is not for the expert." It is not likely, however, to suit the needs of the nonexpert either. If it is intended as an overview of the Chinese Army since 1949, it fails to treat the all-important nation-building role of the PLA as an economic work force, as a cultural and educational vanguard, as a training ground in technical skills, and as an integrative and politicizing environment for its personnel. Corr does devote one chapter to the history of the tensions between the Army and other elements of the political system, such as the Communist party, but the bulk of the book is concerned with the four major post-1949 "campaigns" of the PLA: Tibet; Korea; the Indian border; and the Soviet Border. Aside from Korea, however, these have been very limited endeavors by the standards of world military history.

A common problem of popular treatments of military history is the need to deal with the political context of military activities without detracting from the military emphasis. The result is usually an inadequate treatment of either aspect, and Corr's book is no exception. The narrative is superficial and tends to confuse evidence with conjecture.

The principal identifiable theme of the book is that the four military campaigns were restrained and were justified on the grounds of historical claim or national security. Corr is attempting to counter the cold war image of China as militarily aggressive. The aim has merit, and is consistent with the revisionist trends in recent studies of China's foreign policy. Unfortunately, Corr's effort is not convincing on two counts. First, he does not cite any sources. There is perhaps no need for footnotes in a general work of this type, but one would expect that the bibliography would reasonably lead the reader to reputable books in the field.Yet the section on the Korean War, for instance, makes no mention of the classics on that subject by Alan Whiting, Alexander George, or I. F. Stone. Secondly, Corr's credibility in attempting to look at China's military activities "as they may have been viewed at the time from Peking" (p. 10) is weakened by his persistent use of racial stereotypes: "the men who fill the ranks of the PLA today . . . have a quick, intuitive intelligence without being over-endowed with imagination" (pp. 25–26) and "Mao . . . anxious for quick results, with his patience, like that of most of his fellow Hunanese, burning on a short fuse" (pp. 47–48). I would suggest that it is precisely this kind of "pop" psychological evaluation which has been at the root of cold war views of China, and of foreign policies based on those views.

Since Corr fails to provide an adequate treatment of the Chinese army, the general reader must still rely on scholarly works. Among the most readable, although it is unfortunately several years out of date, is John Gittings's *The Role of the Chinese Army* (New York: Oxford University Press, 1967).

PIERRE M. PERROLLE

Wheaton College, Massachusetts

European Parliament Digest, Volume I: 1973. Edited by Mary Edmond. (Towota, N.J.: Rowman and Littlefield, 1975. Pp. 218. $22.50.)

The full reports of the proceedings of the European Parliament for 1973 cover 2119 pages. Here they are reduced to 194 pages plus an introduction and two indexes. The latter are by far the most valuable features of the book. Every speaker and his contributions to the debates are listed (the only one omission I noted was that Mr. John Brewis's speech on page 116 is not recorded). The main topics discussed in the Parliament are covered in a separate index. Scholars wishing to study the parliamentary record of individual speakers, or of groups of speakers, will for the first time be able to do so in English without having to plough through the full reports of that body. It is difficult, however, to think of many alternative uses for this volume. Although a library which takes the "European Hansard" will find this digest a useful, but expensive aid to research, it is no substitute for the complete transcripts themselves.

The speeches are clearly and judiciously summarized. The print is handsome and the format of the book easy to use and easy on the eye. Most librarians, however, could probably spend $22.50 more usefully on other reference books concerning the activities of the EEC institutions.

D. J. WENDEN

All Souls College, Oxford

Armies in Revolution. By John Ellis. (New York: Oxford University Press, 1974. Pp. 278. $10.95.)

This book bases a successful revolution on the "militarization" of a political movement and the "politicization" of its military arm (p. 15). A movement must be militarily disciplined to win the revolutionary war or takeover – yet, paradoxically, it must also be politically democratic to assure a "social revolution" along popular lines. The paradox is resolved by an "authentic people's army" (p. 250): Its consensual base affords enough military cohesion yet permits enough political responsiveness for the revolution to succeed, as evidenced by the Chinese Civil War (chap. 8). Conversely, a "regular army" that gains cohesion through control from above rather than consensus from below can still win the military takeover but at the cost of popularly guided social transformation afterwards, as in the English Civil War, the French Revolution, the Prussian Reforms, and the Russian Revolution (chaps. 2, 4, 5, 7). But without a cohesive army, not even a takeover can succeed, as shown in the Paris Commune (chap. 6). People's war, claims Professor Ellis, is thus the only way to make revolution effective as well as responsive.

Since this pattern is nearly a truism, the anomalous case – America's War of Independence (chap. 3) – may be more instructive than the pattern itself. Here, as the author himself points out, a military revolt succeeded with an army that was neither militarily ready nor politically responsive. He attributes this success to the "personal greatness" of George Washington (p. 249); but this reason remains outside his theory even though it could apply to other cases too (e.g., Cromwell, Lenin, Mao). An alternative reason may be lack of cohesion in the regime's forces as well as in the rebel army; hence, not the *level* but the *ratio* of military discipline between rebel and regime factions may be crucial to revolutionary victory. Stated this way, the author's thesis easily covers the American case: the exception helps us to refine the rule.

However refined the account may be, its relevance seems to be timebound. The writer himself grants that "technological parity" between the ruling and rebelling forces is more likely in past than in present societies. Yet social factors may thereby be more crucial to past than to prospective revolutionary victories. Specifically, the successful occurrence of revolutionary wars in premodern states might derive from military discipline and political solidarity, but the nonoccurrence of such wars in modern states could be due instead to the regime's technological superiority. The book does not check for this possibility insofar as its design does not include states at varying plateaus of development. The work thus remains a history rather than a theory of revolutionary outcomes.

A dilemma for this and other revolution studies involves the subject and object of analysis. The object is verification of, in this case, the hypothesized effect of military strategy upon revolutionary success. Yet the subject all but eludes verification. For revolution is definitionally vague – and indeed is not defined by Ellis – thus precluding agreed-upon cases (e.g., Prussia, America) and periods (e.g., does the China case end in 1949?). The subject is also empirically rare (for Ellis, n = 7), thus precluding reliability tests. Revolutionary wars may be informationally fraught as well, since the highly charged political atmosphere may impose dual meanings upon such writings as memoranda and memoirs (Ellis's primary sources). Since the subject-object dilemma is unresolved, this book is an illustration rather than a verification of the Ellis hypothesis.

The work's prime asset is its fusion of military science and social science insights. From social science comes an awareness of the impact by "grass-roots aspirations" upon the choice of strategy; from military science comes a sensitivity to the impact of strategy, whether regular or guerrilla war, upon the outcome of revolution. The apparent assumption is that progress in revolutionary studies hinges on both the "militarization" of social science and the "socialization" of military science. In making this fusion, the author becomes part of that movement within academe – intellectual ecumenism – whose task is to airlift analytical tools and insights across disciplinary boundaries. The importance of that task, I think, overshadows the book's departure from the rigorous methodological standards now emerging in social inquiry.

CRAIG MCCAUGHRIN

University of Pennsylvania

Pluralism and Party Transformation in Lebanon: Al-Kata'ib 1936–1970. By John P. Entelis. (Leiden: E.J. Brill, 1974. Pp. 227. Guilders 68.00.)

Professor Entelis has produced a careful and provocative study of Lebanon's largest political party, the Kata'ib (LKP), better known as the Phalanges. It will be of interest not only to students of Lebanese and Middle Eastern politics but also to political scientists concerned with modernization, fragmented political cultures, and consociational democracy. It takes on added significance because of the important role the Kata'ib has played in the Lebanese civil war, in which 60,000 people may have perished by the end of 1976. But the Kata'ib's aggressive behavior in this crisis, especially at its beginning, which has contributed to pushing Lebanon toward *de facto* partition, casts a certain irony on some parts of the author's analysis.

The thesis of this study is that the LKP "is the most important and influential political organization in the country dedicated to preserving the system's essential physiognomy while concurrently encouraging meaningful forms of evolutionary or adaptive social change" (p. 9) and that it has achieved considerable success in both of these objectives. Entelis supports his thesis ably, but accepts perhaps a shade less critically than he might have done the party's self-image and its own analysis of its role in Lebanese society and politics. The LKP is important but not as decisively important as he suggests. Certainly, its efforts to preserve "the essential physiognomy" of the system have proven counterproductive and its attempts to encourage "meaningful . . . evolutionary" social change without needed political reform have come to naught.

To be sure, the LKP became a major actor in Lebanese politics during and after the 1958 crisis, and one of the most interesting parts of this book is the description of how it finally became part of the Lebanese establishment. But even after 1958 its political power remained limited. The Party held only between four and nine seats in the ninety-nine-member Chamber of Deputies. It showed no clear trend toward increasing its electoral strength: in 1960 it won six seats; in 1964, four; and in 1968, nine. In the last elections before the civil war (and after the completion of this study), 1972, the LKP won only seven seats. Similarly, the percentage of LKP winners among LKP candidates has also fluctuated over these four elections, with 86, 44, 100, and 63 per cent, respectively. As for participation in government, while the LKP held one or two portfolios in most, but not all, of the cabinets of the 1960s, this limited level of representation declined somewhat in the 1970s: there was no Kata'ib member in the Sa'ib Salam cabinets of October 1970 through April 1973; and it is significant that a key provision in one of the early – and abortive – attempts to head off the civil war in summer 1975 was that the Kata'ib be excluded from the government. Thus even up to the collapse of normal government (and contrary to the statement on p. 149) the Kata'ib was not to be automatically granted cabinet membership. Furthermore, one could make a case that other political organizations such as the army, the president and his circle, the Maronite church, the banking and business community, certain traditional notables and their clienteles, and other "parties" such as Camille Chamoun's National Liberal Party or Kamal Jumblat's Druze-socialist coalition were as important as the Kata'ib, if not more so, in system preservation. One may not be prepared to agree, therefore, that the LKP was "the single most powerful independent political force in Lebanon" (p. 213). It is more important to realize that its power was military more than political and that the scope of its influence was confined to the Maronite community. Even there it was not unchallenged, as the continuing importance of rival notables like Camille Chamoun, Suleiman Frangieh, and Raymond Eddé indicates.

As for the LKP encouraging social change, Entelis convincingly documents how its liberal intelligentsia was very much in tune with the social democratic ideology of General Fuad Chehab's regime (1958–1964). During that relatively stable era and the first part of the regime of Charles Helou (Chehab's successor and protégé), it was possible for the LKP to try to project a new, nonsectarian, integrative image. Although the author does not mention it, the "integration-through-modernization" strategy of Chehab was also very good politics for the Kata'ib: it enabled the party to consolidate its gains at the expense of the other leading Christian politicians, especially Camille Chamoun, who had been defeated in the 1958 civil strife. But as Chehabist stability withered away in the late 1960s because of corruption, financial instability, and above all the consequences of the Six-Day War, the Kata'ib reverted to its primordial Maronite nationalism.

The emergence of an armed, militant Palestinian movement in 1968 created an imbalance in the delicate Lebanese equation. Although the Palestinians had no interest in interfering in Lebanese politics, their activities against Israel brought massive reprisals which in turn led to Christian discontent and ultimately political-sectarian conflict. The Palestinians served to catalyze Muslim-left wing discontent, and the Kata'ib saw the "essential physiognomy" of the system – i.e., Christian dominance – threatened. And, as Entelis observes insightfully, the new threat was a powerful stimulus for development of the party and especially its militia. The LKP developed increasingly not as a "positive system challenger" but as the main instrument of "static system maintenance." Entelis dwells mainly on the "positive" aspects of Kata'ib behavior – and understandably so, since much of his research was undertaken during the LKP's social-democratic phase. But in his final chapter he recognizes the trend toward parochial exclusivism, and he concludes by declaring what a tragedy it would be if "this progressive and forward-looking party . . . were to regress . . . in its stated objectives of achieving an enduring modernization, democracy, and stability" (p. 215). Indeed, "tragic" is not too strong a word to apply to the LKP's policy, especially evident from the spring of 1973, of using violence to try and subdue the Palestinians. This policy, based as it was on a fear that the Palestinians would infect their Lebanese Muslim and Arab nationalist supporters with discontent at traditional Christian domination, became a self-fulfilling prophecy. While the LKP undoubtedly strengthened its position within the Christian political scene by its military prowess in "defending" Lebanese Christian nationalism, its (and the author's) hopes of the Party serving as an instrument for modernizing the Lebanese state and integrating the whole Lebanese polity, including the Muslims and the left, seemed to have been virtually destroyed by the end of 1976. Ironically, the Kata'ib by that time appeared to have become a major force working in behalf not of "static system maintenance" but of carving a "pure" Maronite nationalist enclave out of the old pluralist Lebanese republic.

While Entelis's thesis emphasizes the liberal and integrative aspects of the LKP, it is to his credit as a scholar that he does not gloss over evidence pointing in another direction. Thus, readers will find material which helps explain the LKP's regression to primordial exclusivism. The chapter on belief system and ideology formation is particularly enlightening in this respect: in it (e.g., on pp. 78–82) we see clearly a deep racial and religious prejudice against Arab Muslims.

Professor Entelis certainly is right in arguing that the Kata'ib has brought structural coherence to the Christian community, and with it more power than the traditional notables and their clienteles could muster against rising class and sectarian opposition to the system. Unfortunately, however, this uneven political development appears to have helped destroy the delicate sectarian proportional representation formula which held the state together since independence. An all-embracing "Christianized" Lebanese nationalism may be just as artificial and hard to develop as the harsh, assimilative pan-Arabism which the author condemns. If preservation of the essential physiognomy of the Lebanese system requires some modernization through improved efficiency and redistributive capacities it also seems to depend on minimizing communal political institutionalization. The only institutionalization that will save Lebanon from disintegration is that which originates from a nonsectarian basis – and the Kata'ib evidently cannot escape its sectarian identity.

Whether or not one accepts Entelis's interpretation of the Kata'ib's development or its functions as part of the Lebanese political system, his arguments are always stimulating because they draw creatively upon modernization and pluralist theory. Furthermore, the core of the book – Part II which deals with the LKP's history, ideology, structure, membership, and electoral-government performance – is very well written and constitutes a significant contribution for students of political parties in the Middle East and elsewhere.

MICHAEL C. HUDSON

Georgetown University

Backward Toward Revolution: The Chinese Revolutionary Party. By Edward Friedman. (Berkeley: University of California Press, 1974. Pp. 237. $12.75.)

Although its data are drawn from the Chinese case, this book addresses an issue of general importance in the field of revolutionary politics and social change. As the title suggests, Professor Friedman argues that modern revolutions, and modern revolutionaries, succeed only to the extent that they draw on the past in building a new future. In peasant societies such as China, Friedman believes, progressive ideologies and modern organizations are not necessarily the most effective instruments for enlisting popular support in the countryside. Instead, he stresses the ability of radical elites to develop revolutionary ideals and political organizations which tap the basically conservative and "backward" views of the peasants. In Friedman's view, "mythic return and modern advance" (p. 220) are complementary rather than antagonistic forces for change.

The case example selected by the author for the exploration of this provocative thesis is the Chinese Revolutionary Party (CRP). Founded by Sun Yat-sen after the failure of the Second Revolution of 1913, the CRP represented a retreat from the principles of a Western parliamentary party to an organizational form more appropriate to China's quest for unity against Western imperialism. The "super-Leninist" (p. 212) structure of the CRP was a throwback to China's secret societies in the sense that it projected an image of total commitment and sacrifice which resonated more strongly among China's population than the competition and careerism encouraged by the Western-type parties. More importantly, the CRP initiated a movement "backward toward revolution" through its alliance with "social bandit revolutionaries" (p. 144), such as the White Wolf, who roamed the North China countryside in the 1910s. Though not altogether isolated from national political affairs, the White Wolf drew his major strength from an appeal to the religious needs of the peasantry and their desire for renewing of family and community ties. In the face of "an atomizing war of all against all" (p. 137) in the countryside, the villager recruited into the guerrilla bands of the White Wolf did not "see himself as rebelling," so much as "trying to defend what has been and is rightfully his" (p. 222). In the eyes of the peasants, revolution was "a restoration" (p. 120), "a return to the mythical starting point" (p. 121).

Friedman's discussion of the White Wolf is a major contribution to current reinterpretations regarding the nature of peasant participation in revolutionary political movements. A major premise of his argument – that "people in the countryside who had been acting for centuries on millennial, religious, and magical notions did not suddenly undergo a transvaluation of values" (p. 131) – is a valid criticism of works which superimpose Western concepts of social change on the convulsions that have affected China and other peasant societies. It is difficult to believe, for instance, that in the absence of full-scale industrialization and mechanization of agriculture, China's peasantry has undergone such fundamental transformations as the elimination of religion and the erosion of belief in the supernatural. Indeed, Friedman's analysis challenges the common assumption that radical change in peasant belief systems is a necessary concomitant of economic progress and mass politicization. The movement to an egalitarian society in China's countryside was not antithetical to peasant folklore traditions and thus did not require the total psychological metamorphosis suggested in William Hinton's *Fanshen* (New York: Random House, 1966).

Unfortunately, Professor Friedman's sensitivity to peasant psychology is not matched by an equally perceptive analysis of rural social structure. As is so often the case in studies of this sort, the peasantry is treated as an undifferentiated and largely unchanging mass. Friedman does not relate the insightful discussion of peasant religion to rural class relations. Nor does he consider the institutional structure of religion and its relationship to the position of China's landlord/gentry class. Most importantly, Friedman's suggestion that the popular appeal of the White Wolf in the 1910s paralleled the CCP's mobilization of the peasantry in the 1940s neglects significant changes that occurred in north China in the interim thirty-year period. His hypothesis linking the two movements are interesting but cannot be taken seriously without an analysis of sociological data drawn from village case studies.

Finally, I must criticize Professor Friedman's arguments which, while treating the CRP as an organizational precursor to the CCP, consider the progressive views on party organization articulated by Sung Chiao-jen – the architect of the 1912 Kuomintang – as irrelevant to subsequent Communist theory and practice on the Party. To be sure, Sun Yat-sen's support of a highly disciplined and tightly knit organizational structure presaged Communist efforts at party-building. Yet, several important features of the CRP – most notably, Sun Yat-sen's patriarchical leadership style and the emphasis on brotherhood as a basis of party member relations – were strongly criticized by the Chinese Communists as early as the 1920s. Indeed, the CCP elite have persistently opposed organizational characteristics associated with the secret society and social bandit traditions. On the other hand, Sung Chiao-jen's commitment to a party organization structured around political issues, as opposed to personal cliques, anticipated the Communist effort in the Rectification Campaign (1942–1944) to depersonalize cadre relationships and establish ideological beliefs as the basic associational principle within the CCP. Contrary to the Friedman thesis, the formulation of a viable revolutionary party in China has involved the introduction of organizational structures and practices which were not within the framework of established traditions, and thus required a major transformation of attitudes and behavior. From an organizational perspective, the thrust of revolution was not "backward" but decidedly *forward.*

In sum, *Backward Toward Revolution* is a valuable study which should provoke considerable discussion and debate. In light of the current drive in China for the mechanization of agriculture, Friedman's emphasis on the basically conservative orientation of the peasantry deserves serious deliberation.

LAWRENCE SULLIVAN

Wellesley College

Vote, Clivages Socio-Politiques et Developpement Regional en Belgique. By André-Paul Frognier, Vincent E. McHale, and Dennis Paranzino. (Louvain: Vander, 1974. Pp. 149. F.B. 280.)

This book brings together two studies, each in its own way analyzing the relations between social reality and the vote in Belgium. They have also in common, that, starting from a sound theoretical background, they make use of refined mathematical tools, which once more proves to be a fruitful combination.

In the first study Frognier tries with success to create an image of the distances between the political parties as they were perceived by the voters in 1968, in the three Belgian regions (Wallonia, Flanders, and Brussels). The representation of these distances in a multidimensional space allows him to identify the dimensions as the cleavages, classic in Belgian sociopolitical life. As a verification, the distances between parties are compared with the flows of votes between these parties, and the results are confirmed. The method is inspired by the studies of Rokeach and of Converse and Valen, but use is made of a specific type of factor analysis: the analysis of correspondences.

McHale and Paranzino pay attention to the impact of regional developmental change on the transformation of the cultural-political opposition in Belgium. Instead of the classic hypothesis of the integrative functions of economic development, they postulate the idea that socioeconomic diversity and unbalanced growth may result in a resurgence of ethnic, regional, linguistic, and other subnational concerns which may threaten continued stability. They show that the growing political importance of linguistic differences and regional-linguistic parties in Belgium in the postwar period is in large part an outgrowth of persisting developmental imbalances between Flanders and Wallonia. Some of their results, however, confirm the hypothesis about the effects of cross-cutting conflicts too; thus, they find that the "Socialist-Catholic" factor, expressing especially socioeconomic conflict in Wallonia and religious conflict in Flanders, is negatively correlated in both regions with the variable expressing regional-linguistic conflict. They established also a relation between blancs and the divided attention of the voters for two cleavages at once. Both these kinds of hypotheses seem to be complementary in the Belgian context, since developmental change has not only increased the political salience of linguistic differences but it also loosened old political ties leading to opportunities for new forms of electoral support.

Both these studies are important contributions to knowledge of the Belgian political system, and they also provide better foundations for comparative research.

A. VAN DEN BRANDE

Ghent, Belgium

Soviet Works on Korea: 1945–1970. By George Ginsburgs. (Los Angeles: University of Southern California, Far Eastern and Russian Research Series, 1974. Pp. 168. $9.50, cloth; $6.50, paper.)

This unique bibliography of more than a thousand Russian-language works, with the titles in English translation, provides perspectives and facts often not available in Western monographs. The thoughtful and succinct preface by Professor George Ginsburgs of Rutgers University Law School, Camden, paints a vivid background and categorizes major Societ research on Korea. Professor Ginsburgs, in addition to compiling this comprehensive source, suggests useful research questions, such as: why hasn't North Korea joined Comecon, what is the impact of industrialization, etc.

Professor Ginsburgs charts a close correlation between Soviet political output and changes in the closeness of the Soviet-Korean relationship. The recent increase of publications in broader fields such as history and economics, however, suggests that the number of titles should remain relatively substantial.

This well-written and cohesive secondary source is essential for students of Korea who are able to utilize Russian-language monographs.

ROBERT R. SIMMONS

University of Guelph, Ontario

Comparative Policy Analysis: The Study of Population Policy Determinants in Developing Countries. Edited by R. Kenneth Godwin. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 333. $17.00.)

Some twenty years ago the usual stance in demography was that one could predict the consequences of alternative population policies but that, as a neutral scientist, one had no right to advocate any of them. Neither position is widely held today. With the repeated failures of population forecasts to predict at all accurately, responsible demographers are likely to restrict their statements about the future to thoroughly hedged broad generalizations. And during the same two decades, paradoxically, many in the discipline – and outside it – began to offer increasingly precise exhortations to everyone from heads of state to individual parents. This transition to advocacy showed all too often that few in the population field had ever thought about what is meant by "policy" or how one evaluates it. Some ten years ago, when political scientists started to show an interest in population policy, many demographers welcomed their intervention, even though initially it was typically expressed in throat-clearing compilations by generally young men as ignorant of demography as they were unsure about the trend of their own discipline.

This is perhaps the fifth such compilation, and eventually one's good-natured tolerance is strained. The thirteen contributors to the volume comprise a lone professor of sociology (also not a demographer), three government bureaucrats, three private counterparts, and six academic political scientists, most of them at a junior level. To make a book out of offerings from such diverse sources would require strong editorial guidance, but the editor, an assistant professor at Oregon State University, failed to exercise the control that might have made a coherent work out of this almost random agglomeration.

The papers range from original and stimulating to pedestrian and derivative. They are not linked by anything more than the broad topic stated in the subtitle, and the variation in style and mode of analysis is not mitigated by any visible overall structure. As one sees from the footnotes, this little group continues along its well trod path, citing from one another and the same few key demographers (Judith Blake and Edwin Driver) or administrators (in particular, Berelson). But they have not yet evolved a common base from which to advance, nor even a common language. In the last chapter before an appended bibliography, Bruce Shepard reviews some of the book's points and notes again the two dozen meanings that its contributors, among scholars generally, give to "policy." If the book had ended with no more than a consistent typology of usage, that would have been a minor achievement; instead, most of the authors struggle visibly to define – differently – the concept they are supposed to analyze.

The longest paper in the book, by Warren Ilchman, although poorly organized, is stimulating and worth reading. Its discussion exemplifies how an intelligent outsider can benefit any discipline – for example, by seeing practices that have become routine for the stupidities that they are. As one instance, Ilchman has compiled a table showing the principal results of some thirty-four studies to test the thesis that persons of higher education have fewer children on the average (and thus, implicitly, to test also the policy recommendation that to reduce a population's fertility one should raise its educational level). Of the thirty-four researchers, thirty-four neglected to ask questions, or even to speculate, on how the *content* of the education might have affected fertility behavior. Not surprisingly, American women who attended Catholic colleges several decades ago have had more children than one would anticipate from the general thesis, and the same seems to be the case in the Middle East with men who had an advanced Islamic education. Beyond the primary skills, "education" has a moral content that in its direct and indirect effects can be either antinatalist or pronatalist, or a confusing combination of both.

The best paper, not fortuitously, is also mainly negative – Patricia Anglim's generalizations from her work in Africa. She makes four main criticisms of the typical rationale of population programs: that antinatalist policies are judged by their supposed eventual economic benefits but seldom by their immediate political costs; that the power of the political leaders whom would-be policymakers court is often less than that of the anonymous government bureaucracy; that the simple "technological mastery" of contraception does not ensure the successful realization of antinatalist policies; and that hopes for such an achievement, even if otherwise realistic enough, typically gloss over the fact that the transition from traditionalism to modernity will last generations. It is a comment on the state of the art that such commonsensical points, when forcefully expressed and aptly illustrated, must be rated as excellent.

WILLIAM PETERSEN

Ohio State University

Race vs. Politics in Guyana: Political Cleavages and Political Mobilization in the 1968 General Elections. By J. E. Greene. (Kingston, Jamaica: Institute of Social and Economic Research, University of the West Indies Press, 1974. Pp. xvii, 198. $3.50, paper.)

The use of electoral politics for forging an effective national unity in a "new" country wracked by profound ethnic and religious divisions would appear to be almost a contradiction in terms. One would suppose that to the degree that free party competition exists and to the extent that the parties reflect the ethnic cleavages in the society, open rivalry among them would exacerbate disunity and be a cause, or at least a forerunner, of political violence. The grave history of ethnic rioting and violence in Guyana prior to Independence would seem logically to augur such a direction.

Nevertheless, it is a conclusion of this excellent study of the politics of Guyana, based on a systematic examination of the electorate following the 1968 elections in that country, that just such a phenomenon has emerged there. Radical cleavages are deep and lasting in Guyanese society; they are reflected clearly in the identifications which the electorate make of the principal parties, each one corresponding quite closely to distinct ethnic groups in its electoral support; each party openly employs appeals to ethnicity for electoral purposes. Yet the result has not been the disintegration of the national society, nor an increase in communal tensions, nor paralysis at the centers of government. How can this seeming anomaly be explained?

This book suggests, quite convincingly, that the system of party organization developed in Guyana since the time of the mobilization for the 1968 elections is a principal explanatory factor. The system of proportional representation introduced for that and subsequent elections obliged the parties to create more or less permanent organizational cadres in all electoral districts, thereby raising the immediate visibility of all the parties. The rapid mobilization of the new mass electorate which had taken place for the elections of 1953 under the aegis of the Jagan-Burnham People's Progressive Party had resulted in a highly participant political culture; the subsequent break between Burnham and Jagan and the creation of a two-party alignment in which the ethnic component of the two leaders' respective constituencies became the prominent distinction between them served, apparently, to heighten political awareness and participation. The author suggests that the creation of a third party (United Force, UF) clearly representative of the white (principally Portuguese) merchant and capitalist class and appealing as well to other non-East Indian and non-African ethnic minorities, completed the ethnic "coverage" of this participant culture. The personal styles and ideological idiosyncracies of the two great Guyanese political rivals – the pragmatic "smartman" Forbes Burnham and the seemingly ingenuous Cheddi Jagan – have apparently contributed as well to a continuing interest and participation in the party process by the population at large.

The racial appeals for electoral purposes are not the result of an ideological line stemming from the party leadership. On the leadership level the parties are quite consciously interracial, and their ideologies are firmly nationalistic and antiracist. Professor Greene shows how the Guyanese parties, as electoral organizations, reflect significantly the racially divided character of the electorate; yet they seem to be providing a kind of political mobilization which balances ethnic pluralism and separateness with effective integration into a national system with sufficient self-confidence to display considerable independence in its domestic and international postures.

In this monograph report on an empirical study of the electorate of 1968, the author is methodical and careful in his presentation of the data and is lucid and professionally cautious in his interpretations and conclusions. As a profile of the attitudes and behaviors of an electorate in a "new" nation, the book is informative, very well organized, methodologically elegant, and cogent. In addition, and for all its brevity and the modesty of its pretensions, the study never loses sight of the larger questions of politics which are posed so dramatically in the new nations of the Caribbean, faced with the challenges of national integration in the context of cultural and ethnic pluralism. The book is a solid contribution to the understanding of the possible role of party and party organization in the working out of these challenges in one particular nation. The concepts and methods used by the author lend themselves comfortably to the tasks of comparative studies in the Caribbean and elsewhere. It is a welcome additional example of the high standards of responsible and relevant scholarship we have come to expect from the Institute of Social and Economic Research at the University of the West Indies.

ROBERT W. ANDERSON

University of Puerto Rico, Rio Piedras

Willi Münzenberg: A Political Biography. By Babette Gross. (East Lansing: Michigan State University Press, 1974. Pp. 337. $12.50.)

An important aspect of certain political movements is the scope they give to individuals who would otherwise be confined to restricted fields of action. This enlargement may proceed along social, cultural, or geographic dimensions; while the individual experiences hitherto unimagined opportunities, the movements gain capable individuals. Revolutionary movements perform this function, at least during their "heroic" periods, and of course Marxism and Leninism have been no exception.

An outstanding but neglected figure who exemplifies this general process at work in the international Communist movement during its Heroic Age (ca. 1915–1935) is the German publicist and politico-cultural impresario, Willi Münzenberg (1889–1940). Born into humble circumstances in Thuringia, poorly and sporadically educated, he had seemingly found his predestined niche when, at the age of fifteen, he was apprenticed to a barber. Yet Münzenberg became, in turn, the leader of the Communist Youth International, inventor of the "Communist Front," master organizer of intellectual fellow-traveling, and an outstanding propagandist, first for the Soviet Union and then against Hitler, particularly at the time of the Reichstag Fire and Trial. This range of activity was possible not only because of Münzenberg's abilities but also because of the existence of an international movement and an organization, the Comintern, and it flourished as the expression of an international outlook. Significantly, Münzenberg's effectiveness declined when the Stalinization of the Comintern deprived him of the ideological and organizational flexibility he required; he had remained autonomous of the KPD apparatus, even though he belonged to its Central Committee.

Münzenberg was the prototypical popular-front participant. While maintaining his own convictions, he found common ground with those of different political opinions. Relatively infrequently did he deceive his collaborators about the objectives of their actions (more often about the nature of his own financial and organizational resources). It is part of the disappointment of this book that the one flagrant instance of such deception which is described (pp. 220–221) is left unanalyzed.

In historical perspective, Münzenberg's striving for personal and organizational autonomy made him an increasingly anomalous figure. He lost control of youth affairs because he wished them to remain autonomous of the party; his work flourished in the NEP period because it was useful, and it survived the sectarian "third period" because of its organizational resources and loose ties to Moscow. Eventually such independent spirits were fed into the Gulag machine. Münzenberg seemed to have escaped this fate by not returning to Moscow, but died, probably at Soviet instigation, fleeing southward in France in 1940.

His particular genius expressed itself chiefly in two directions. He sensed that the modern "progressive" intelligentsia would respond to someone who did for it the tedious work of mobilizing its good will; and he was a master propagandist, capable of reaching the apolitical masses with modern journalistic techniques and of appealing to intellectuals with book and cinema. Münzenberg's genuine enthusiasm for intellectual products and packaging, his organizational talent, and his internationalist and socialist convictions made him the perfect master of his many-ringed circus.

Fortunately, the fascination of Münzenberg's career comes through in this disappointing book (I did not have access to the 1967 German original; the English-language edition is marred by *gaucheries* of translation, such as "fire-raisers" for arsonists [p. 248), and editorial lapses, such as misdating the Fourth Comintern Congress to 1923 [p. 134]).

Babette Gross, who was Münzenberg's companion from 1922 on, although she remains an almost anonymous figure in this book, has unwisely decided to write a "political biography." She would have done better to have written in vivid detail from her unique perspective. For Münzenberg's early life, including his antiwar activities, Swiss period, early meetings with Lenin, and almost all of his youth work, she relies on a few older sources, including Münzenberg's memoirs, but gives neither a brief summary nor a fully rounded picture. It is typical of Gross's lack of depth that despite Münzenberg's complex relationship with his father, she remarks on the occasion of the father's accidental shotgun death that "it did not seem to have made a great impression" on 13-year-old Willi (p. 12).

Even as a close observer and participant, she gives us superficial political summaries, sprinkled with digressions (such as the description of a Comintern spy school, with which Münzenberg seems to have had no connection, on pp. 264–265). What were Münzenberg's views of the important German and Soviet officials he knew? How did he view his causes, the intellectuals he managed, the campaigns he organized? How did Münzenberg, who was not an intellectual, and spoke no language other than his native German, get along with Rolland, Einstein, and the rest? Is Gross right that after 1933 Münzenberg's work shifted from pro-Soviet to Anti-Hitler? To these and many other questions, this book gives no answers.

Nevertheless, by virtue of the unique materials which Gross does provide, her life of Willi Münzenberg makes valuable reading for anyone interested in the early Communist movement or (as with her account of the Berlin premier of *Potemkin*) European political-intellectual history during World War I and the two decades following.

HENRY KRISCH

The University of Connecticut

Military Regimes in Africa. By William F. Gutteridge. (London: Methuen & Co., Ltd., 1975. Distributed by Barnes & Noble, New York. Pp. 195. $12.75, cloth; $7.00, paper.)

William F. Gutteridge is the doyen of African military studies. In the early 1960s, when only a few scholars were concerned with the military in the Third World, Gutteridge published his pioneering *Armed Forces in New States* (London: Oxford University Press, 1962) and *Military Institutions and Power in the New States* (New York: Praeger, 1965). These general works were followed by a more specialized study of *The Military in African Politics* (London: Methuen, 1969), and now by *Military Regimes in Africa*. In addition to these major works, Gutteridge has written numerous papers, articles, and short monographs on related subjects.

Military Regimes in Africa is really an updated and revised edition of *The Military in African Politics*. Whereas this earlier volume was concerned primarily with the origins and causes of military intervention, the present work evaluates the character and conduct of African military regimes. As Gutteridge explains in his preface, the purpose of this book was "to write a sequel" to *The Military in African Politics* which would "take up the threads of the first work and at the same time concern itself more with performance in power than with the seizure of it." It is altogether fitting that Gutteridge should now address the behavior of military regimes. In the first place, a continued preoccupation with the causes of military coups in Africa would have added little to what is already known. And so the author's introductory chapter merely adapts – without specific attribution – Claude Welch's general classification of eight factors that have contributed toward African coups. Second, the proliferation of military regimes in Africa during the last decade provides ample cases and time-depth for a retrospective analysis. Finally, the shifting of research interest from causes to consequences is consonant with the general state of the art in contemporary African military studies.

The heart of the volume consists of six case studies of military rule in Ghana, Dahomey (now Benin), Nigeria, Zaire, Uganda, and the Sudan. In each case Gutteridge sketches the basic features and trends of national and international politics. Rejecting "elaborately structured analysis" and "essentially meretricious" categorizations, the author prefers to stress the uniqueness and novelty of each case. The result is an interesting collection of brief historical essays which unfortunately contributes little to our understanding of the performance of African military regimes. Many of the biases and imbalances of Gutteridge's earlier work have been carried over into this volume. Nigeria and Ghana, which together claimed 66 of the 102 pages of case-studies in *The Military in African Politics*, continue to dominate *Military Regimes in Africa*. In fact, they both occupy the exact number of pages in this new volume, thirty and thirty-six pages respectively, and far outweigh the forty-nine pages allocated to the other four cases. There is no apparent justification for this imbalance; indeed, the duration of military rule in Dahomey (Benin), Zaire, and the Sudan constitutes a *prima facie* case for comparable attention. The distribution of attention among the six cases was obviously determined more by the author's personal inclinations and infatuations than by their intrinsic interest.

A more serious objection can be raised regarding the absence of any admissible criteria for the selection of cases. Gutteridge admits in the preface that each of his six cases was selected as "significant, if only because it tended to emphasize the unique nature of the particular situation." Presumably then, each case simply cumulates novelty but yields no comparative perspective. This methodological dilemma notwithstanding, Gutteridge does manage to draw some conclusions from the "unique" particulars. These conclusions, however, amount to no more than a distillation of the conventional wisdom that (1) the performance of African military regimes has not been very different from civilian governments; (2) military regimes are essentially alliances between the ruling officers and the civil service or bureaucracy; (3) civilian-military distinctions are blurred because the army and bureaucracy are conservative, order-loving elites who treasure their mutual self-interest above all else; and (4) military regimes have tended to succumb to the intractable problems of governing new states rather than master these conditions.

Military Regimes in Africa therefore is more a synthesis than a singular contribution. It reflects, reinforces, and recapitulates current views instead of reassessing them. Like all other general studies of the political role of the military in Africa, this volume is based on the case-study approach, but the absence of a comparative analytical framework is especially deplorable. The high cost of such a slender volume of such prosaic rendition will only irritate the reader who expects more from William Gutteridge.

JOSEPH P. SMALDONE

University of Maryland, College Park

Struggle in the Andes: Peasant Political Mobilization in Peru. By Howard Handelman. (Austin: Institute of Latin American Studies, The University of Texas at Austin, 1975. Pp. xvii, 303. $10.00.)

In his introduction to the study, Howard Handelman, Assistant Professor of Political Science at the University of Wisconsin-Milwaukee, informs us that his purpose "was to study the role of the village *campesinos* in the national political system," and to determine "how and under what circumstances the peasantry may become an effective participant in Peruvian politics." On page 46 (at the bottom of footnote 24) he indicates that "A major objective of this study will be to probe the attitudes of *sierra* villagers to ascertain whether or not they are inclined toward cooperation and to see how such attitudes might affect their potential for political mobilization." In spite of a number of months diligently spent collecting data on the views of community leaders and surveying the existing literature and periodical sources, I am forced to conclude that the author has failed to achieve any of his objectives. Let me go into some detail.

The first part of the book (chapters 1 to 6) covers the historical background of the period that Professor Handelman had chosen: The invasions of large landholdings by peasants employed by them (in various capacities) and/or living in nearby communities, which occurred in Peru between 1960 and 1964. It is important to keep this period in mind when one analyzes the second part of the book. These first six chapters, while presenting a competent survey of the literature already available on the subject, fail to provide any information not previously published. To be fair, the author scrupulously lists his sources and gives credit to those who preceded him in the discussion of peasant movements in Peru. But obviously, honesty alone does not provide the raw material of which scholarly books are made. Furthermore, his survey of the existing literature, while competent, is not comprehensive enough. Professor Handelman deals at length in chapters five and six with two individuals who played an active role in the events he describes. Yet, he makes no reference to their writings, such as Genaro Ledesma Izquieta's *Complot,* which deals with some of the same episodes covered in the book, and Jesus Veliz Lizarraga's *El Peru en la Cultura Occidental and Principios Fundamentales de Aprismo.* Finally, and perhaps more importantly, the author fails in this first part to provide a comprehensive vision of the Peruvian political system in the period under study, the type of vision that the nonspecialist reader needs in order to place the specific subjects of Professor Handelman's study in the systemwide context.

The second part of the book – chapters 7 to 9, as well as appendices I and II – contains the presentation and analysis of the field data collected by the author. It consists of the information obtained by interviewing the leaders of forty-one peasant villages in the central and southern highlands of Peru; the dwellers of twenty-one of these villages had participated in land invasions during the period under study, and the inhabitants of the other twenty had not. The author tells us that the universe might total 6,000 such villages. With the help of university students from the areas being surveyed, the author interviewed the leadership group at joint meetings, often attended by other dwellers. He sought information on level of socioeconomic development, contacts with the outside world, intravillage solidarity, political mobilization, and political attitudes. Since it is apparent, at least to me, that the second part of the book is supposed to be the author's original contribution, the data ought to be analyzed at length and the "fine print" read carefully.

As with the first part of his study, Professor Handelman honestly discusses some of the weaknesses of his survey. He points out that he is interviewing village leaders of 1969 to investigate events that had occurred in 1960–64, although many of these leaders had not led during the period under study. Having dismissed the possibility of surveying a representative sample of village dwellers, the author arbitrarily assumes that the views of the leaders coincide with those of the villagers, both in 1969 and in 1960–64. By employing joint (even mass) interviews, he made it difficult for individual leaders to express dissenting opinions, thus converting villages into homogeneous units. The survey was conducted in some villages before and in others after the announcement of a new agrarian reform program by the military regime, a fact that undoubtedly affected the village leaders' perception of the political system. Finally, the author tells us (p. 268) that the questions dealing with political orientations, internal solidarity, and political efficacy were open-ended and "thus, the criteria for assigning a particular response to a rank were necessarily subjective." The assignment was handled by him and his two assistants who, apparently, were not the same individuals throughout the study. Thus the reader is told in Appendix A that the core of the "objective" data is in fact highly "subjective."

The statistical handling of the already questionable set of tables also deserves some comments. Assuming that the views expressed by village leaders were uniformly shared by their constituents and ignoring all the other difficulties listed above, we must note, nevertheless, that the author's measurement of the strength of the relationships is based on small sample sizes (at most forty-one cases). When samples of this magnitude are utilized, the appropriate measures of statistical significaNce should accompany the values of tau-c provided in the text. In the absence of any discussion of statistical significance I am unable to place much reliance on Professor Handelman's conclusions, which are based on tau-c's varying in magnitude from .063 to .409 and averaging .264. (I gratefully acknowledge the advice of my colleague Linda Powell on this subject.)

When one combines the inadequate presentation of the data with the weaknesses listed above, the inescapable conclusion is that Professor Handelman's evidence is extremely shaky and does not provide his opinions and observations with the backing that he appears to take for granted. I do not believe, for instance, that on the basis of this data, he has successfully disproved the theories advanced by Samuel P. Huntington and others regarding the political involvement of the rural lower class. Nor has he proven the views he advances in the second part of the book, and particularly in chapters 8 and 9 regarding the mobilization of the Peruvian peasantry and its relationship to the entire polity.

The book also includes an epilogue which attempts to update the discussion of the political role of the peasantry in terms of the measures taken by the military government that took power in October, 1968. It discusses the characteristics and possible consequences of the agrarian reform program decreed by the military leadership in June, 1969. Unfortunately, since the manuscript was completed in 1972 and has not been further updated (although it was published in 1975), most of the facts presented in chapter 10 have already been published and in some cases superseded by more recent events. So have many of the comments presented by the author regarding the military government and its policies toward the rural areas.

Intellectually, one issue that appears throughout the book deserves some comment. Professor Handelman keeps raising the question of legitimacy of peasants' land invasions and other political actions. Whether the author recognizes it or not, he leaves the reader with the impression that peasants' political moves enjoy a high level of justified legitimacy. To put it quite simply, the peasants are the "good guys" in the Peruvian struggle for power. I'd like to suggest that the peasants are motivated by the same factors that moved *mestizos* and whites, both now and throughout history. Their claim to land and to a share of political power is based on as weak (or as strong) grounds and claims as those held by white and *mestizo* landowners, military officers, bureaucrats, and industrialists. Property relationships have been altered more than once in many parts of the world; what evidence is really there to claim that ownership rights of Indians or peasants are more legitimate than those of the colonizers, particularly when one is familiar with the precolonial history? The conception of legitimacy hinted at by Professor Handelman should have been explicitly discussed, since it affects not only some of the authors's hypotheses but his perception of events as well.

Finally, at a different level, I want to question once more the unfortunate habit of writing comparative politics in a mixture of languages. The author feels compelled to use Spanish words and phrases on nearly every page, as if they had a meaning of their own which can not be conveyed by their English equivalents. Next to them, to prove that this is not the case, he gives us the translation, thus unnecessarily increasing the length, and cost, of the book. The volume is also lengthened by the needless repetition of facts and thoughts; examples can be found in information provided on footnote 28, p. 131, and repeated on p. 135, as well as with the facts described on footnote 54, p. 242, and repeated on footnote 17, p. 251.

I am regretfully forced to conclude that Professor Handelman's book confronts us with the work of an honest observer who contributes virtually nothing to his field.

CARLOS A. ASTIZ

State University of New York at Albany

Divided Nations in a Divided World. Edited by Gregory Henderson, Richard Ned Lebow and John S. Stoessinger. (New York: David McKay Co., 1974. Pp. 470. $12.50, cloth; $5.95, paper.)

A world that is divided into innumerable territorial entities called nation-states, each claiming an exclusive right over a domain, is a fact of international life with which students of world politics must begin. The book under review is a path-breaking enterprise which initiates a systematic investigation of the phenomenon of political fragmentation and reintegration of nation-states in a divided world. The book is grouped into two parts and eleven case studies of six "divided nations" including Germany (John H. Herz), Korea (Gregory Henderson), China (Nathaniel Thayer), Vietnam (Le Thi Tuyet), Cambodia and Laos (Bernard K. Gordon) and Mongolia (Harrison E. Salisbury) as well as of five "partitioned countries" including Ireland (Richard Ned Lebow), India and Pakistan (Craig Baxter), India, Pakistan, and Bangladesh (Rounaq Johan), Ruanda-Urundi (Warren Weinstein), and Palestine-Eretz Israel (Gidon Gottlieb). "Divided nations," according to the editors, are countries that have been arbitrarily separated into two or more political units from the outside and "partitioned countries" are the case of political fragmentation that results from internal causes of ethnic, linguistic, or religious conflict among the subgroups in the political system.

A very brief, almost too cursory, introduction is provided by one of the editors, John Stoessinger, in the capacity as director of the seminar sponsored by Ralph Bunche Institute on the United Nations of the City University of

New York, for which most of the papers contained in the volume were written. The concluding chapter of the book, written by the remaining two editors (Henderson and Lebow), provides a useful and stimulating theoretical overview of the book which is intended more to raise questions and hypotheses than to provide answers. Henderson and Lebow suggest, for instance, that the "relations between divided nations change as a function of (a) the degree of stability and legitimacy of each divided state; (b) relations between each divided state and its respective superpower; and (c) relations between the superpowers themselves" (pp. 438–439).

Henderson and Lebow break down the process of political fragmentation and reintegration of the nation-states into four separate stages. These are (1) initial division, characterized by intense hostility between divided units; (2) middle-term division, noted for declining hostility between units; (3) rapprochement, marked by the creation of intergovernmental linkages and of formal consultative machinery; and (4) unification, resulting in a kind of loose federation initially and involving free movement of persons, extensive economic cooperation and some symbolic gesture like adoption of a common flag and national anthem. In light of the subsequent development in Vietnam, which led to a forceful takeover of the south by the north, the paradigm of political fragmentation and reintegration suggested in the books seems limited to its validity and usefulness. Other less sweeping as generalizations are more convincing and seem accurate as hypotheses. For instance, Henderson and Lebow argue that the divided nations "are likely to possess greater freedom of action and seek improved relations with each other," as they "develop internal strength" and as the level of "hostility between their respective superpower backers decreases" (p. 439).

The book contains useful data on each country and on all twelve cases under review. In an attempt to explain the degree of "hostilization" between the component units, an interesting set of data are displayed on the background characteristics of these countries. These background variables, fourteen of them in all, include such common attributes of a country as the length of historical experience, degree of economic development, the population and resource distribution, as well as some unusual attributes like the regime stability and ideology, proximity to great powers as neighbors, ethnic-linguistic-religious homogeneity, the number of refugees. Unfortunately, the measurement of these interesting variables is based on a subjective criterion, rather than on an objective standard. Some actual statistics, such as percentage of the dominant group in the population as a measure of ethnic homogeneity, for instance, would have been far more useful and valid than the judgmental scoring of ethnicity based on a scale with a range of ± 5.

The case-study materials contained in the book are generally historical and descriptive, geared to the need of those outside the area specialization. The book is by no means a theoretical breakthrough in the study of the process of political fragmentation and reintegration of the nation-states in the international system. The concluding chapter, in my opinion, should have been placed at the beginning to provide an overarching framework for the remaining chapters of case studies to follow. As a result the book suffers from the lack of theory-guided perspective that could have tied the material together. In fact, the style, focus, coverage, and quality of the essays vary and the accuracy as well as the value of some of the case studies may be questioned. It is not clear, for instance, why the countries of Cambodia and Laos are lumped together as divided nations and why Pakistan-Bangladesh and India are partitioned countries.

The volume nevertheless represents a fascinating story of the making, the breaking, and the remaking of political entities in the world the landscape of which continues to change. The study makes one aware of the importance of an act of "bounding" territory – whether in the developed countries or in the newly emerging countries, often with an arbitrary drawing of the frontiers following the departure of the former colonial powers of the West.

YOUNG WHAN KIHL

Iowa State University

Chinese Historiography on the Revolution of 1911. By Winston Hsieh. (Stanford, Ca.: Hoover Institution Press, 1975. Pp. 165. $9.00.)

This valuable monograph, composed of an illuminating historiographical essay and an extremely useful bibliography, is of much greater interest to students of modern China than its highly specialized topic might suggest. One cannot arrive at an adequate understanding of political development in modern China without first coming to grips with the revolutionary movement which led to the 1911 Revolution and beyond. There are still relatively few Western scholarly examinations of this period, and, as Professor Hsieh demonstrates master-fully, one must approach the Chinese language studies with caution. Interpretations of the revolution by both Chinese Nationalist and Communist historians (whom Hsieh labels Orthodox and Neo-Orthodox respectively), on the whole faithfully follow the changing political and intellectual needs of the ruling elites. The first school magnifies the role of Sun Yat-sen and his associates while the other seeks to compress history into Marxist class categories, although valuable compendia, documentary collections, and monographs have resulted from their labors. Both schools fit squarely into the grand tradition of Chinese historiography which valued intellectual orthodoxy above all else and viewed scholarship as an ideological tool.

Professor Hsieh brings to his analysis the cool iconoclasm which is the hallmark of the historiographer's craft. Despite the shortcomings of earlier studies of the revolution, enough of the picture has emerged over time to enable contemporary scholars to pose the questions which their Chinese predecessors were inhibited from exploring. Here the monograph makes its greatest contribution. Professor Hsieh offers us a rich menu of suggestions for research topics, including the development of ideology and political institutions, local politics, the relationship between the Manchu reforms and the revolution, the interaction between traditional and modern modes of rebellion in China and much more. The broadminded student of post-1949 Chinese politics cannot fail to recognize the family resemblance between many of these topics and his own concerns. Political scientists specializing in China, then, should view this book as an open invitation to join historians like Professor Hsieh in a continuing effort to deepen our understanding of this seminal period in modern Chinese history.

STEVEN I. LEVINE

Columbia University

Reform and Revolution in Asia. Edited by G. F. Hudson. (New York: St. Martin's Press, 1973. Pp. 310. $16.95.)

The eight essays of this collection concern Asia, but as a group they do not focus upon "reform and revolution." They are, rather, "special studies" of "aspects of the Asian political scene" (pp. 9, 24).

This work has nine parts. The first are an editor's introduction and a chapter on prerequisites to Asian modernization (the latter by Werner Klatt). There follow two essays on China (by W. A. C. Adie and by Ralf Bonwit), and one each on Japan (Richard Storry), India (W. F. Crawley), Pakistan (H. Khuhro), and Indonesia (L. H. Palmier). It concludes with an essay by the editor on Asia in the 'seventies.

Klatt's topic raises immediate problems, for it assumes that modernization is a rather concrete goal and that progress toward that goal can be assessed. The terms are familiar: industrialization, economic development, gross domestic product. To Klatt's credit he is sensitive to issues of land and agriculture, asserts that politics deserves the "commanding position" in change (p. 53), and recognizes the risks of foreign advice (p. 30). He has difficulty assimilating China to the text, writing as if about Asia ("foreign contribution to the investment programme . . . is invariably essential") and then acknowledging that China has had little foreign assistance (and hence a "much reduced rate of growth" [pp. 36–37]). Klatt repeats the conventional but distorting assertion that China spends 10 per cent of her annual income on defense (p. 35) and judges that Chinese agricultural planning "has been faulty in the extreme" (p. 52). Klatt's commitment is to reform rather than revolution, but through "well-timed economic, social and political 'inputs' " to overcome "tensions"; enriching the few by merely raising the economic growth rate, he asserts, can have a "palliative effect" but "cannot avert ultimate disaster" (p. 56). Thus new policies must be sought, but not in the Chinese experience: "China's prerequisites for modernization are unique, and thus make its political, social and economic pattern unsuitable elsewhere in Asia" (p. 52).

Adie's essay traces Chinese arguments on education from 1949 to 1970. His theme is the contest between full-time study ("less but finer," elitist, professional, specialist, Liuist) and half-work half-study ("more but worse," political, generalist, Maoist). The topic is important, and Adie asserts his argument deftly. He is drawn into an unproductive excursion, however, as he takes up the alleged "special quality in Chinese society" of repressed aggression against figures of authority, and likens themes in "the propaganda of Maoism" to Nazi themes during the Third Reich (p. 63).

Adie's assessment turns on one perception and one predisposition: He sees the role of China's army, from 1964, to have been increasingly influential and even controlling ("usurped the powers of the constitutional Party Committees," "ultimate control rested with Army men"), though with the final – and contradictory? – insight that "the real effect of the guiding role given to PLA is to disperse and demilitarize it" (pp. 95–96).

His predisposition is to modernization and specialization. China may devise "further solutions" to the conflict between egalitarianism and specialization, but this juncture witnesses "the inevitable restoration of power to regular teaching staff and to managerial bureaucracy" (pp. 94, 96).

Adie's viewpoint is best captured by his closing remarks on the army's role as model, an orderly horde:

> The values transmitted to the younger generation are meant to be those of a group whose development was arrested at the stage of adolescent revolt, idealism and fantasies of assimilating the world, as described by . . . Jean Piaget.
>
> . . . Piaget's remarks on . . . a mode of thought proceeding from the concrete to the concrete without elaboration of abstract ideas . . . call to mind many passages of Mao Tse-tung's theoretical writings, while his description of the intellectual egocentricity of adolescence . . . not only fits the mentality of Red Guards as revealed in their publications, but may be extended by analogy to the collective mind of emergent nationalist states (p. 96).

Bonwit – "Mao's Continuing Revolution" – is the most useful essay in the volume, an exegesis and interpretation of texts on central themes. Written in July, 1970, it asserts that PLA control is decisive and that "outside the PLA, there appears to be no properly constituted Party authority below Central Committee level" (p. 142). Thus it can be read not only as a carefully argued example of one school of analysis, but also as an illustration of the dangers in interpretation of a fluid and inaccessible situation.

Storry's brief, inconclusive entr'acte on the Mishima affair locates it politically, with grace and effect.

The next three essays survey the post-World War II political histories of India, Pakistan, and Indonesia. Leaders, parties, and interest groups march in the quest for "control." Each essay, succinct and felicitously worded, is an exercise in compression. On the other hand, the authors have little scope for argument. Social and economic facts, if not reducible to shorthand, are never explored. Miss Khuhro, for example, loses an important opportunity to consider underlying change which preceded the December 1970 victory of Z. A. Bhutto in West Pakistan. And Mr. Palmier tells us that the "sober economic plans" of the Suharto government called for encouragement of "foreign investment that the country desperately needs" (p. 263) with no intimation of the issues of source, quality, consequence, and terms which that prescription necessarily implies.

G. F. Hudson concludes that the three main types of political system in Asia are democracy, communism, and military dictatorship, and that "there is no government in the world which is quite secure against communist takeover" (p. 274). And he adds that "if Asians want political democracy as a condition of development, they can look to India for a model; if their concern is with advanced technology and high standards of living they can look to Japan; in China they can only find utopian ideology and a still backward economy which painfully drags itself upward with 'politics in command' . . ." (p. 299). Of course, this was written in November 1971.

Romanization of Chinese names is inconsistent; Hudson repeatedly refers to Soga Gakkai [sic]; and the index is a shamble of errors, both of spelling and page reference. This is the American version of Publication No. 7 of St. Antony's College, Oxford, published in 1972 by George Allen and Unwin under the same title, but appears simply to reproduce the errors of the original.

BRUCE D. LARKIN

The Chinese University of Hong Kong

The Politics of Massacre: Political Processes in South Vietnam. By Charles A. Joiner. (Philadelphia: Temple University Press, 1974. Pp. 346. $12.50.)

Charles Joiner of Temple University claims his book is highly critical of every party to the war in Vietnam. In view of Joiner's early association with Vietnam through the Michigan State University Advisory Group and his subsequent diligence in studying this country, such a declaration of critical intent deserves attention. Unfortunately, Joiner's critical detachment is sometimes more a posture than a method and does not compensate for his lack of a comprehensive, consistent perspective.

Part of the problem is that the book consists mainly of articles on loosely related or overlapping subjects published between 1963 and 1970 and reprinted evidently without revision. Three chapters or sections of chapters were co-authored with Guy Fox, John Donnell, or Roy Jumper. Not surprisingly, the book has a problem of continuity and lacks a steady focus. The impact of the author's criticism is further vitiated by his clear preference for non-Communist government in Vietnam. This preference does not necessarily require support for any of the non-Communist regimes that have collapsed in South Vietnam, nor is it inherently incompatible with critical analysis, but in this instance it leads to sympathetic "balanced" criticism of the Second Republic and unalloyed aversion to the Communists.

The principle theme is that the internal political process in South Vietnam always has prevailed over the external forces which have sought to shape it. Hence, it is futile to "explain" Vietnamese politics in terms of external forces, and the proper subject of study is the process by which Vietnamese have sought to re-establish legitimate institutions. This is hardly a startling argument, but it bears repeating as a corrective to the simplistic image of Vietnam as a passive arena of Great-Power politics. For Joiner, however, the "internal political process" is mainly the politics of the now-defunct Republic. This may be the natural consequence of his experience and sources, but it leads to a surrealistic picture nonetheless. The fortunes of non-Communist political groups are traced in tortuous detail, as are the elections of 1967, while the revolution remains little more than background noise. A brief section on "The Organization Theory of Revolutionary Warfare" – actually an uncritical "critique" (Joiner's word) of Douglas Pike's *Viet Cong* – and assertions that the insurgents were in some way part of the process, do not adequately compensate. Pike's "organization theory" (which inspires Joiner, in a lapse of analytical detachment, to recommend that Saigon turn the "organization weapon" on the insurgents) merely prevents him from seeing how Communist effectiveness was contingent upon inequities that Saigon elites sought to preserve, as Jeffrey Race convincingly demonstrated in *War Comes to Long An*. The view of both Saigon politics and revolutionary politics, and of the intersections between them, is from the limited vantage point of Saigon.

Heterogeneity is held to have been the principal cause of political fragmentation, disunity and powerlessness of the Republic. Joiner provides a great deal of useful detail in support of this view. Certainly, the reliance of Saigon elites on particularistic bases of support was a major factor keeping the Republic's politics in a state of petty, debilitating contention. But Joiner does not successfully explain why the PRG succeeded where the RVN failed. To argue, as Joiner does, that the NLF and PRG were held together by indoctrination and organizational complexity, or that "Northerners" imposed discipline on unruly "Southerners," whereas the Republic did not do these things or did them less well, only begs further questions. Very different conclusions might have been reached had Joiner appreciated that Saigon elites represented narrow interests within their claimed constituencies and had he not ignored the importance of class cleavages.

One of the curious consequences of his insistence upon the primacy of heterogeneity is to portray all Vietnamese political movements as equally plagued by it. This is valid in the sense that they all had to operate in the same environment, but not in the sense that they were equally unable to do anything about it. Joiner's vision of Vietnamese society overlooks the strong continuities which overlay the heterogeneity and cannot come to terms with the fact that of all modern Vietnamese political movements, the Communist Party was the most successful in strengthening these continuities by establishing institutions on the presumption of regional, sectarian, sexual, and ethnic equality. The assertion that "perhaps nothing is truly 'national' about Vietnamese politics" (p. 98) and the labeling of the evanescent cliques and factions that orbited about Saigon from 1954 to 1975 the "nationalists," in addition to being contradictory, betrays a shopworn, ideologically inspired ignorance of what Vietnamese communism is and what it has achieved. Yet in fairness it should be said that knowledgeable students of Vietnam, reading with circumspection, may find in this book some useful insights and information on the politics of the non-Communist experiment.

WILLIAM S. TURLEY

Southern Illinois University at Carbondale

The Pattern of Sino-American Crises: Political-Military Interactions in the 1950s. By J. H. Kalicki. (New York: Cambridge University Press, 1975. Pp. 279. $17.95.)

Dr. Kalicki's study of Sino-American crisis interactions attempts to contribute to the quest for nomothetic statements about international crises by moving in the direction of comparative inquiry for the purpose of generating hypotheses and theory about the general pattern of crisis behavior. This commendable analytic goal is pursued by what is termed a "comparative case study" approach focusing on the single Sino-American dyad over time, rather than comparing crisis-prone dyads comprised of varied participants. Given the prominence of the Korean conflict, the two Indochina episodes, and the two Taiwan Straits confrontations in any conventional inventory of international crises, this happy choice of cases should have facilitated the search for behavioral regularities while permitting the analyst to hold constant a number of potentially contaminating factors intrinsic to a cross-sectional design. Unfortunately, however, the author fails to develop an explicit framework for comparison across cases, so that what emerges is a series of "historical narratives" detailing the peculiarities of each successive crisis; different variables are observed in each case, with the result that a rich contextual account of the unique characteristics of each crisis is provided, but the reader must sort this mass of detailed information and read between the lines in order to discern commonalities in the record of interactions. Had the narrative histories been probed in a systematic manner with a nonelastic category system (by jettisoning juxtaposition in favor of controlled comparison), perhaps the conclusions would not have appeared so theoretically weak, descriptively trivial, and often tautological. For example, to learn that communications problems are aggravated by the nature of the channels (p. 72) and that "the exercise of ambiguity . . . can result in increased confusion over intentions" (p. 86) is not to acquire wildly exciting information or perceptive insight into crisis phenomena.

More useful, perhaps, are the conclusions which emerged from Dr. Kalicki's *cumulative* diachronic perusal of the record of Sino-American interactions throughout the entire period. The author's unfortunately brief (10 page) examination of the total chronological sequence of interactions in the 1950s suggests rather convincingly the extent to which past dyadic exchanges served as a prologue to future interactions. That is, each crisis set precedents for future crisis behavior and each party to the dyad developed through experience a patterned response-set or institutionalized behavior repertoire which, the author contends, enabled the two superpowers to manage each successive disruption more effectively by anticipating the other's behavior. The primary thesis stemming from this observation is that the crisis behavior of Chinese and American decision makers became increasingly sophisticated over time; as a result of shared experience in dealing with each other in crisis situations, both sides learned to become more flexible, creative, and capable of crisis management. While some readers may regard the conclusion that the "learning curve of US-PRC crisis handling" grew at an exponentially increasing rate as hyperbole (since most learning grows at a decreasing rate, resembling a diminishing returns curve), the evidence that internation learning occurs under conditions of chronic crisis constitutes a valuable insight and promising hypothesis for future empirical research. A set of corollary hypotheses also deserving of further research may be derived as well: that crisis conditions tend to promote the clarification of national goals and interests, and that rather than increasing the probability of war, crises may stimulate compromise and agreement between conflicting parties.

This book is likely to elicit differing reactions from readers, depending on their methodological and epistemological predilection. Diplomatic historians will probably find the presentation balanced; the interpretation of events is an orthodox one, but the contribution of revisionist works is occasionally acknowledged. Some readers will undoubtedly be disturbed by the book's tendency to employ reified concepts and teleological reasoning, by the occasional use of normative adjectives for descriptive purposes, by the alacrity with which motivations and intentions are attributed to actors on the basis of mere assertion, or by the tendency to vacillate without warning between treatments of crises as independent and as dependent variables. Students of comparative foreign policy should appreciate the sensitive treatment given to the linkage between domestic and international politics, but will be inclined to look askance at the way in which variations in systemic and idiosyncratic factors are underplayed. Theoretically inclined readers in search of empirically verifiable hypotheses are certain to find this book unrewarding when compared with the methodologically sophisticated and encyclopedic coverage provided by Charles F. Hermann's *International Crises: Insights from Behavioral Research* (New York: Free Press, 1972). And traditionally minded students of contemporary international affairs will be distressed to discover that an assessment of the legacy of Sino-American interactions in the 1950s for current relations between the two states is not made. Nevertheless, despite some apparent deficiencies, *The Pattern of Sino-American Crises,* which is based on the author's doctoral dissertation at the University of London, addresses a complex subject in a highly readable manner. One would hope that Dr. Kalicki will pursue some of the promising ideas expressed in the present study in subsequent work.

CHARLES W. KEGLEY, JR.

University of South Carolina

Revolutionary Transformation in the Arab World: Habash and his Comrades from Nationalism to Marxism. By Walid Kazziha. (New York: St. Martin's Press, 1975. Pp. 118. $8.95.)

The Arab Nationalist Movement has not received much attention in the West, largely because it has been overshadowed by political movements such as Nasserism and parties such as the Baath. Nevertheless, the ANM, as it is abbreviated, is worthy of notice. One regime in the Arab world, that of South Yemen, grew out of the ANM, and two important, albeit numerically small, factions of the Palestinian movement, the PFLP and the PDFLP, can trace their origins to the ANM.

Walid Kazziha, presently a professor of political science at the American University of Cairo, brings to his subject one invaluable asset: for eight years, he was an active member of the ANM. His book is not, however, a personal history, nor is it a theoretical or ideological tract. Instead, Kazziha sets himself the modest goal of describing the evolution of the ANM as a political phenomenon in the Arab world.

He tells us that starting from an early preoccupation with the loss of Palestine, the ANM gradually evolved toward Nasser's vision of Arab nationalism and unity, to the point where the ANM nearly lost its separate identity. In 1964, however, the ANM took a new turn, this time to the left, as a younger generation of party militants began to press for the inclusion of socialist concepts in the ANM ideology. Here the author is able to draw on first-hand information to clarify the nature of the debate that eventually split the ANM into more and less radical factions.

The author also sheds light on the organizational structure of the ANM, describing its evolution from a cell-based movement in its early years to a looser structure by the 1960s. As the ANM spread to various Arab countries, it adapted itself to local conditions. The author provides useful descriptions of the accommodations made in Jordan with some of the traditional political forces, and in the Gulf states, where the ANM remains an important political force today, especially among the large expatriate Palestinian community.

Kazziha has done well what he set out to accomplish. The book does not claim to provide a general theory of political movements, nor does it rely on concepts derived from the social sciences. The result, apart from the pretentious and misleading title, is a modest and useful case study, particularly welcome because of the dearth of books on the topic and the moderate, nonpolemical tone adopted by the author.

WILLIAM B. QUANDT

University of Pennsylvania

The Military in the Third World. By Gavin Kennedy. (New York: Charles Scribner's Sons, 1975. Pp. 368. $17.50.)

This book opens a new set of questions relevant to the study of the military in the Third World. The now voluminous literature in this area began with often normative and relatively data-free analyses of the reasons for increased intrusion of the military into the politics of developing states. In the intervening years we have seen a greater interest in the role of the armed forces after their attainment of civil power and the employment of more quantitative data and methods. Professor Gavin Kennedy also analyzes military intervention, utilizing case studies from the Middle East, Africa and South-Southeast Asia, but in addition he brings to the reader's attention a number of less-discussed issues.

The most interesting issues include the context of violence in which the military operates, the reasons for the nonintervention of the armed forces, and the economic impact of military expenditures. The first chapters assess violence against patterns of ideology and political structure in the Third World. After noting the prevalence of violence in the area (he concludes the book by stating that violence is endemic in the world and "the normal means of settling human conflict" [p. 335]), the author argues that the intervention of the military is a consequence of violence rather than the cause of it. Another chapter reviews the fascinating question of why the military has not intervened or been depoliticized in what are now a minority of Third World countries, presenting for observation the examples of Turkey, Israel, and Mexico. Given the virtual ubiquitousness of military interference in civil government in the Third World, this question is as crucial as the earlier queries about why coups took place. Noting the importance of strong, charismatic, and legalistic leaders in the three case studies, the author goes on to argue that: "Where civilian governments fall and the military succeeds this expresses the crisis of civilian government and not the inherent pathology of the soldier" (p. 154).

The last half of the book is an overview of subjects too little discussed in the literature, i.e., the politics of defense budgets; the relation of defense expenditures to growth, welfare, and production; arms races and production and their relationship both to the economics of the developing world and to the issue of disarmament in the region. Numerous charts are presented on expenditures, types of military equipment, economic and social data on individual countries, suppliers and production of armaments, etc. In his analysis the author seeks to correlate economic and social data with military expenditures. While cautioning the reader about the unreliability of the data, Kennedy is not prepared to argue that military expenditure and production have a necessarily deleterious impact upon development, welfare, and industrialization in Third World countries.

It is in this area of analysis that I find Professor Kennedy the least persuasive. It is true that his cross-national data do not show necessary injurious relationships to various aspects of growth and development from military expenditures and production; furthermore, high-growth states have also spent high percentages of their budgets on their armed forces. This type of analysis, however, begs the question of whether there would have been even higher GDPs, more welfare, and higher levels of industrialization *without* such attention to the military. To a degree these problems with cross-national data are ameliorated by the extensive case studies describing the economics of war in such individual countries as Nigeria, Israel, Egypt, Pakistan, and the neighbors of Vietnam. While the reader may question Kennedy's general tendency to discount the harmful effect of the military, he does bring considerable data and careful reasoning to bear in his confrontation with many accepted arguments.

I recommend this book highly to both the general reader and specialist. The country or area expert may quarrel with details of specific case studies; the reader may be put off by Kennedy's tendency toward asides, and some overstatement; and I myself was unable to accept some of the conclusions drawn from the cross-national analysis. Nevertheless, the book asks many of the right questions and includes most welcome compilations of data.

FRED R. VON DER MEHDEN

Rice University

Le fédéralisme soviétique: Ses particularités typologiques. By Theofil I. Kis. (Ottawa: Éditions de l'Université de'Ottawa, 1973. Pp. xiii, 191. $4.95.)

This study of federalism in the U.S.S.R. is not directly concerned with the Soviet nationalities problem, nor is it an attempt to understand the actual political relationships between the men in charge of the major federal units and the central authorities in Moscow. It is, instead, an examination of the concept of federalism as it developed in the U.S.S.R. and is expressed in Soviet constitutional arrangements. Professor Kis's basic thesis is that Western scholars should not dismiss Soviet federalism as a mere sham but should accept it as a unique form of federal system based on theoretical principles which, though generally couched in traditional terminology, differ radically from those underlying the federal systems of the West. The most important difference is that Soviet federalism, unlike others, was never intended to provide decentralized administration or local autonomy but rather was conceived as a means for moving eventually toward a fully centralized, unitary state.

Although the author asserts that his approach is a "normative-structural-functional" one, his work is overwhelmingly historical and legalistic in the most traditional sense. The first half of the book is devoted to an examination of the development of the concept of federalism in Russia. Kis provides an interesting and concise survey of some of the prerevolutionary Russian attitudes toward federalism and shows that the generally antifederalist views which prevailed among the non-Marxist Russians were matched, for different reasons, by strongly antifederalist attitudes on the part of Marx, Engels, and Lenin. He then presents the reasons that Lenin and his associates decided to alter their attitude on this question during the years preceding the formation of the U.S.S.R. in 1922–1924. Essentially Kis sees Lenin as having accepted federalism as a temporary compromise designed to alleviate dangerous nationalities pressures, to prevent the breakup of the multinational Russian state, and to facilitate a gradual reintegration of the nationalities into the kind of centralized state which Lenin, as a Russian and a Marxist, preferred. Stalin, an advocate of maximal centralization during the debates on the constitution for the U.S.S.R., overtly accepted Lenin's formula but thereafter reverted to his initial preference in practice.

Most of the second half of the book is devoted to an examination of the "typological and Constitutional particularities of Soviet federalism" in the period since 1944 when the Stalin Constitution of 1936 underwent a theoretically important modification which allowed the union republics to conduct their own foreign relations or at least to maintain institutions ostensibly intended for that purpose. Although the author clearly recognizes that a number of features of the Soviet system, including the centralized Party structure, the role of the central authorities in economic planning and foreign policy making, and the overwhelming strength of the Russian republic within the federation, contradict in practice the Soviet claims concerning the sovereignty, autonomy, independence, and right to secession of the union republics, he nevertheless insists on examining each of the provisions of the Constitution as well as the theoretical and legal arguments put forward by various Soviet authors related to these claims. For most readers, including many specialists on Soviet politics, much of this analysis will inevitably seem rather tiresome and futile. It may be somewhat more interesting for scholars engaged in the study of comparative federal systems and constitutions; but even for them, Kis's insistence that Soviet federalism is a unique phenomenon, and his tendency to avoid discussion of actual political interrelationships between the center and the union republics will restrict the book's usefulness.

Finally, the book contains a very brief discussion of some of the implications for Soviet federalism of Khrushchev's attempts at economic decentralization and of the debate among Soviet academic specialists both at that time and after these reforms were abandoned concerning the desirability and speed of the future "coming together" and "merging" of the peoples of the U.S.S.R. Kis finds that while there are profederalists and antifederalists, most of the Soviet theoreticians whose works he has examined do not take an extreme position and that Soviet theory on federalism has generally tended to stagnate in recent years. One could feel more confidence in this assessment if the author had examined more of the Soviet periodical literature on the subject and had included more materials which have appeared since the mid-1960s, particularly the many Soviet works which were published in connection with the fiftieth anniversary of the U.S.S.R. in 1972.

DAVID L. WILLIAMS

Ohio University

Political Regime and Public Policy in the Philippines: A Comparison of Bacolod and Iloilo Cities. By Howard M. Leichter. (DeKalb, Ill.: Center for Southeast Asian Studies, 1975. Distributed by the Cellar Book Shop, Detroit, Michigan. Pp. 165. $7.00, paper.)

Professor Leichter argues in this monograph that regime type (oligarchy or polyarchy) has been the determining factor in the adoption of differing public policies in Bacolod and Iloilo, two Philippine cities. Machine politicians in Iloilo City, having governed since 1955 and lacking an independent economic power base, more intensively exploit office-based resources than their Bacolod City counterparts. In Bacolod City, by contrast, a homogeneous oligarchy owning sugar centrals and large tracts of sugar acreage dominates politics. Political pressures and expediency in Iloilo affect policy in several ways: expenditures on General Administration (i.e., the Office of the Mayor) comprise a larger percentage of total city expenditures than in Bacolod and absorb a larger percentage of mayoral supporters in the local bureaucracy; budgeting is less orderly (as indicated by an excessively large number of appropriations ordinances); fewer permanent civil service eligibles are in the police force and in various policy areas; and there is less continuity than in Bacolod between past and current expenditures.

By "politicizing" protective, administrative, health, and other services to broaden their base of political loyalty, Iloilo mayors and councilors demonstrate a different decision-making style than their Bacolod counterparts. The latter, homogeneous, are able to depoliticize some policy areas such as zoning where policy is made by a combination of government, professional, and civic leaders on the City Planning Board, rather than by the City Council. Since continued elite rule in Bacolod has never been questioned, factional conflict (measured by the number of different factions or parties represented on the city council and by the number of ordinances between 1960 and 1970 receiving negative votes) more fully expresses itself. In Iloilo, by contrast, the political elite is less secure, and consequently seeks to suppress interfactional conflict.

In order to substantiate his argument, Leichter assembles an impressive array of longitudinal data on different policy areas (e.g., regulatory, distributive, extractive), and carefully distinguishes among different types of public policy and the conditions which influence them. Detailed historical analysis shows, for example, that Bacolod is more active and progressive in its use of zoning ordinances for several reasons, such as an active planning board that is successful, in part, because of elite homogeneity. By comparison, the City Planning Board in Iloilo responds largely to crises (e.g., a fire downtown in 1965), often is undercut by the city council, and functions intermittently. Bacolod, unlike Iloilo, has a comprehensive zoning policy governing land use and urban planning.

Leichter's effort to show the importance of regime type is not entirely successful. Because regime type, an aggregate variable, is never measured directly, it remains unclear what specifically is more or less important in influencing policy. For example, we do not know how explicit political factionalism within the city council affects the number of appropriations bills passed. Leichter argues that Bacolod, ruled by a cohesive oligarchy, has a more rational budgetary process – that is, it passes fewer *ad hoc* appropriation bills than does Iloilo. And yet Iloilo has less *overt political* conflict and factionalism in the city council than Bacolod because, according to Leichter, the Iloilo political machine is threatened by discord. In arguing this, Leichter ignores a possible alternative explanation that factionalism in the Bacolod City Council makes passage of appropriations ordinances difficult, regardless of the rationality or irrationality of passing any particular appropriation.

A second weakness of Leichter's monograph is that his analysis of policy change rates does not link fluctuations in revenues, some of which are tied to particular expenditures, to changes in those expenditures. Without such analysis, we do not really know whether and how *politics* in Bacolod and Iloilo affect incremental budgeting.

Third, Leichter carelessly interprets his data on regulatory ordinances. He concludes that the undifferentiated elite in Bacolod passes fewer such ordinances. Yet prior to 1955 Iloilo was also ruled by an undifferentiated elite (the Lopezes and Ledesmas); even then, however, Iloilo consistently had more regulatory ordinances than Bacolod (Table 19).

Despite these oversights, Leichter's monograph is original, stimulating, and occasionally provocative. It should be useful to Filipinists and policy analysts alike.

THOMAS C. NOWAK

Indiana University of Pennsylvania

Political Corruption: The Ghana Case. By Victor T. LeVine. (Stanford: Hoover Institution Press, 1975. Pp. 169. $7.50, cloth.)

In late 1975 Ghana's most recent (Anin) commission of enquiry concluded: "The devout have vanished from the land. Corruption is endemic throughout the whole society." Employing the ample documentation on corruption provided by several regimes, Professor Victor LeVine has written an extremely careful, thoughtful, and valuable study. He first develops a model of the nature and processes of political corruption and then proceeds to delineate and test for the nature, extent, and consequences of Ghana's political corruption.

LeVine provides a brief political history of Ghana, a structural model for studying political corruption, several chapters which test for the levels, patterns, and pervasiveness of political corruption, an assessment of the critical contributing causes of corruption and its probable consequences, and, lastly, an attempt to compare Ghana's corruption and its effects with political corruption elsewhere.

LeVine's analytical model starts with a narrow definition of political corruption and postulates (1) the "core process – involving individual office-holders and political goods, resources, and transaction networks; and (2) the "extended process" – entailing the possible creation of a culture of political corruption and a corrupt informal polity. A culture of political corruption exists where networks of corrupt relationships become so ramified and systematized that they strongly influence and parallel the formal political system. These are extremely difficult concepts to test empirically; an affirmative finding involves measures of the level, pervasiveness, and impact of political corruption and a cutoff point beyond which one could say a culture of political corruption and informal polity exist.

LeVine employs a variety of measures (e.g., the value of corrupt transactions as a percentage of budgets), but ultimately he must render qualitative assessments. LeVine documents the growth of political corruption through increases in the number and value of losses to the government noted annually by the Auditor-General (although he does not give loss value as a percentage of the rapidly expanding public revenue) and the findings of commissions of enquiry under the Nkrumah regime itself. He then details some of the conclusions of the forty-plus commission of enquiry reports initiated by the successor military regime to discredit the overthrown Nkrumah government. These include a report on Nkrumah himself and reports on five institutions which, LeVine submits, were set up for legitimate purposes and "turned out to be agencies for institutionalized political corruption" (p. 30) – an expression with which it is possible to differ. This institutionalized corruption clearly characterized NADECO, set up to collect contract kickbacks, but was somewhat less so for the Ministry of Trade, whose last several ministers (but not the bureaucracy as a whole) became incredibly corrupt in distributing import licenses; it was also less true for the Guinea Press, which was legally and openly government-subsidized, even if it employed political criteria in hiring personnel. LeVine also details the more fragmentary evidence then available on political corruption in the post-Nkrumah period and concludes that it is diminished somewhat in scale. More recent findings show that Prime Minister Busia (1969–72) became quite corrupt in office, as did some of his ministers.

It is impossible to do justice to LeVine's extensive and thoughtful presentation of evidence. In analyzing the values supportive of political corruption through interviews of twelve officials found to have been corrupt, he finds that these officials expressed a high level of cynicism regarding uses of public authority, a high level of political efficacy in manipulating power and influence, a narrow and parochial sense of obligation (self and extended family), and no feelings of guilt. He demonstrates the transactional networks of persons involved in corrupt dealings with interview evidence, the extent of corruption among top Nkrumah regime officials through inquiry findings of their assets, and, in fascinating detail, a wholly corrupt bureaucracy with the enquiry report on the football pools.

LeVine concludes fairly that while a culture of political corruption certainly existed, it had probably not developed into a full-blown corrupt informal polity, such as in Morocco and Sukarno's Indonesia (pp. 71–78).

LeVine assesses the contributory causes in terms of (1) traditional contexts and the effects of colonialism (i.e., market economy and cash nexus), (2) the "new men" seeking political power, and (3) the vast expansion of government/bureaucratic activity, which increases the scope for corruption. LeVine disagrees with much of the literature regarding the possible benefits of political corruption generally, and in particular in Ghana's case, and details the tremendous costs in terms of wasted resources, reduced government capabilities, and political instability.

Several criticisms can be made. The section on political history contains a number of avoidable factual errors: e.g., the Kulungugu assassination attempt occurred in August 1962, not 1964 (p. 28); in 1956, we are told, CPP election support had "shrunk" to 57 per cent (p. xx), but it was officially 56 per cent in 1954; Nkrumah did *not* appoint many followers to the upper ranks of the *civil service*, in contrast with public corporations. More important, I think that LeVine's analysis of the role of the CPP "new men" in politics under Nkrumah as a cause of corruption fails to make some important distinctions; the borrowed notion of a "bureaucratic bourgeoisie" is employed without adequate dissection; and there were differences in the corruptability of the "socialists" and the CPP "old guard" who were parties to the intra-CPP conflicts, as the asset reports make clear. An analysis of those who were not corrupt and the struggles against corruption would have provided a more dynamic political explanation of why the forces tending toward corruption prevailed.

Nevertheless, LeVine's study of political corruption is an excellent contribution to our understanding and to approaches to analysis. For those who care about countries like Ghana, this is a painful subject. And LeVine's study is all the more welcome for the absence of easy cynicism and a hypercritical tone. It is graced with realism and restraint.

JON KRAUS

State University of New York at Fredonia

Underdevelopment in Kenya: The Political Economy of Neo-Colonialism 1964–1971. By Colin Leys. (Berkeley: University of California Press, 1974. Pp. 302. $10.50.)

It is a privilege to review a book as stimulating as *Underdevelopment in Kenya.* Colin Leys has undertaken in this succinct volume the demanding task of analyzing the way in which colonialism generated underdevelopment and how and why the process of "neo-colonial development" is being perpetuated.

Many scholars, following upon Frantz Fanon, have equated African bureaucracies with an emerging bourgeoisie arguing that members of the bureaucracy use their positions to build an economic base as a new class. In contrast, Professor Leys analyzes the still fluid class structure in Kenya in light of Marx's Bonapartist model as set forth in *The Eighteenth Brumaire of Louis Bonaparte.* Thus Leys notes how the alliance among (a) foreign capital, (2) an African auxiliary bourgeoisie of manager/partners in multinational corporations and in the higher ranks of the public sector and (3) an African petty bourgeoisie of merchants and farmers provides those conditions which ensure that no single class dominates the state apparatus in its own interest. The leadership thereby retains the autonomy of class that characterized the Kenyatta regime.

While "Kenyatta and Mboya were not tools of foreign capital," Leys argues, ". . . they were collaborating closely with it. . ." (p. 221). The auxiliary bourgeoisie was not, therefore, a *comprador* bourgeoisie in the classical Marxist sense.

The exigencies of this alliance, however, required that the Kenyatta regime depend upon the civil service, parastatal bodies, police and armed forces all of which were staffed with reliable Kikuyu. To offset this dependence, the Kenyatta regime made concessions first to one then to another element of the ruling class alliance. Such behavior involved a succession of what appeared to be contradictory measures. Foreign capital was ritually humiliated by the rhetoric of economic nationalism but welcomed back in partnership with the auxiliary bourgeoisie, thereby permitting "... a steady expansion of foreign ownership of productive assets in Kenya, and thus of the political power of foreign capital" (p. 208).

At the same time, we are told, the power of peasants and urban workers has been progressively curtailed and neutralized. Continued survival of the peasant mode of production, encouraged by the state, enables employers to keep wages low by deducting from the price of labor power that which the family earns from its own garden. Preservation of the peasant mode also guarantees the supply of agricultural commodities at low prices relative to the manufactures which must be bought, and provides for the absorption of an increasing portion of the adult population (pp. 170–173). Meanwhile, the tripartite agreement among the state, Kenya Federation of Employers, and the Kenya Federation of Labour deprived organized labor of the right to strike, thus further institutionalizing labor passivity.

Leys suggests that a neocolonial pattern of "associated dependent" development characterizes the Kenyan political economy. This pattern emerges in those ex-colonies "... where the transition from colonialism to independence permitted the relatively efficient transfer of political power to a regime based on the support of social classes linked very closely to the foreign interests which were formerly represented by the colonial state" (p. 27). The Kenyatta regime's decision to purchase the mixed European farms in the "white highlands" at full market value, to finance this purchase by loans, and to sell the land to landless Africans on an individual basis represented an official endorsement of capitalism and as such was "the linch-pin of the transition from colonial rule..." to neo-colonialism (p. 63). Dependent capitalist development also implied the granting of legal monopolies that ensured a central role for the state. Legitimized during the colonial period were monopolies of access to capital, roads, labor, most profitable crops, credits, schools and other services that benefitted European settlers. In this way the monopoly nature of neocolonial development would be preserved to meet demands from the African petty bourgeoisie for "fixed shares of specific markets, public loan funds or publicly guaranteed commercial credit, fixed suppliers, and fixed prices" (p. 155).

But this neocolonial pattern of dependent development is not static. Among the contradictions to which Leys points is the latent although growing conflict between the auxiliary bourgeoisie and foreign capital as well as between African traders and foreign manufacturing interests (pp. 158–159). The Bonapartist strategy cannot be expected to continue indefinitely, especially once Kenyatta is dead. Perhaps the most essential contradiction of neocolonialism, however, is that underdevelopment "implies limits to growth and a growing polarization of classes as the exploitation of the masses becomes more apparent" (p. 271). It is this contradiction which still requires further exploration.

LYNN KRIEGER MYTELKA

Carleton University, Ottawa

The Communist Parties of Western Europe. By Neil McInnes. (London: Oxford University Press, 1975. Published for the Royal Institute of International Affairs. Pp. 209. $16.00.)

Neil McInnes is European editor for Dow Jones, which is famous for its averages, *The Wall Street Journal,* and *Barron's.* But McInnes's book not only lacks the superficiality characteristic of deadline journalism. It also avoids the failings of the typical study of nonruling Communist parties available from front-line academics.

To confirm this second point, I surveyed my university's library holdings on West European communism, published in English since 1950. Of twenty-two books, I judged only seven as at least tentatively "comparative" while no more than two reflected the author's conviction that generalizations about communism deserve empirical support. Thus it is with a sense of professional admiration and scholarly enthusiasm that I register Neil McInnes's book as a landmark study of West European communism. It is richly comparative and solidly based on empirical research.

Even the general organization of the book facilitates cross-national analysis, with the chapters focusing in turn on the electoral and organizational strengths of the parties, the characteristics of their membership, their structure (both on paper and in fact), their ideological and programmatic orientations, and the apparent goals and hard realities of alliance politics in pursuit of national office. McInnes frequently includes in his analysis the characteristics of *non*-Communist parties, e.g., with regard to party factionalism, membership militancy, and party financing; these comparisons enhance our understanding of a particular Communist party by placing it in its broader national context. Most students of communism have treated their subject in isolation and consequently have attributed to a particular Communist party characteristics that instead may be generic to the political culture of the whole society.

McInnes assembles data on membership strength for twenty-three Communist parties, trend data on the electoral fortunes of fifteen parties, the most recent election returns for eighteen parties, and additional data for several parties on the size of their associated trade unions, circulation of the party press, membership turnover, party finance, age and SES characteristics of party members, voting support by region, sex, and occupation. There is ample detail on party organizational structure and an array of minicase studies on leadership succession, party factionalism and decision making, and party relationships with Moscow. The narrative materials, all competently integrated with the data, are based largely on original sources from the parties and from the national press (in French, Italian, German, Spanish, Dutch, and the Scandinavian languages).

The author has succeeded in imparting to all these materials a conceptual coherence that is unique to the study of nonruling communism; nor is it often found in the more broadly focused studies in comparative politics. A critical measure of an author's conceptual ability is his propensity for clear generalization and the formulation of testable hypotheses. And in the case of McInnes's densely packed 200 pages of text, I have counted approximately 30 major generalizations and 23 hypotheses with clearly specified variables. Some examples:

The electoral and organizational strength of West European communism is primarily a function of *political* rather than socioeconomic conditions (pp. 69–70).

"Responsiveness to electoral performance is increasingly a feature of West European communism. . . , even if the Leninist structure of the parties sets limits to that responsiveness" (p. 34).

"The practice of the West European CPs is electoralist, parliamentary, revisionist" (p. 181).

The programs and ideological orientations of the parties are less an expression of "forceful social criticism" than they are of "an archaic and incoherent populism" (p. xi).

The percentage of working-class members in the major Communist parties is inversely related to the extent of advanced economic development (pp. 60–61).

Declining electoral support increases factionalism within the party (p. 34).

Divisions within the international Communist movement are more likely to affect the stability of the party organization than the strength of Communist electoral support (p. 30).

The generalizations and hypotheses proposed by McInnes are in turn integrated within a more general conceptual framework that is meant to explain and predict Communist party behavior in the larger West European context. McInnes identifies three power centers or sources of loyalty and competing demands: a conservative party bureaucracy, a more radical and utopian-oriented core of working-class supporters, and the foreign policy interests of the Soviet Union. The frequent conflicts and rivalries among these three centers explains why the parties cannot be understood as monolithic or as "organizational weapons." It also explains the frequent inconsistencies within party programs, between ideology and behavior, and the various patterns of behavior across the spectrum of the nonruling Communist parties (e.g., as when support for Soviet foreign policy proves incompatible with an enlarged electoral following, or when revisionism for the purposes of political alliance jeopardizes the support of militant workers).

Undoubtedly there are problems in so ambitious an undertaking. McInnes does not explain how he reaches the conclusion that "no more than 20 per cent of communist workers" (p. 150) constitute the proletarian hard core that represents one of the three power centers, nor does he explain precisely how this particular center exerts its influence on and interacts with the party bureaucracy, or how its size and orientations may vary over time. Perhaps more important, a broadly based cross-national approach to the study of any political phenomenon seems to dictate an illustrative and narrative style that often proceeds with haste and sometimes sacrifices the accuracy of detail for the satisfaction of generalization.

This may be to say only that it is impossible for one book to be all things to all readers. But a measure of McInnes's accomplishment is the conceptual utility that derives from his rigorously comparative and empirical methodology; his book helps us to organize the findings of studies in the field that exemplify alternative methodologies – including the noncomparative, case study, and more impressionistic approaches to understanding nonruling communism. And, by itself, McInnes's book is unquestionably a major contribution to the field. It also is an example to students in comparative politics of how to be truly comparative.

THOMAS H. GREENE

University of Southern California

Suffragists and Liberals: The Politics of Woman Suffrage in Britain. By David Morgan. (Totowa, N.J.: Rowman and Littlefield, 1975. Pp. 184. $12.50.)

All Liberal and Labour Governments in Britain this century have grappled, to some degree or other, with the thorny problems of electoral reform. The present Labour Government, short of an overall majority of seats in the Commons on the basis of just 39 per cent of the votes in the October 1974 election, faces renewed demands for electoral reforms to achieve a more equitable relationship between votes and seats. As the Labour Ministers consider whether or not to flirt with some changes in the voting system they could do worse than read David Morgan's *Suffragists and Liberals,* which catalogues the agonies of the Asquith Liberal Cabinet as it faced the problem of how to respond to the demands for woman suffrage some sixty years ago. The book complements Morgan's earlier *Suffragists and Democrats* (East Lansing: Michigan State University Press, 1972), which dealt with the politics of woman suffrage in America.

There is no shortage of studies of the woman suffrage campaign in Britain, but for the most part they have concentrated on the suffrage movement and its leaders, rather than on the politicians they faced. As Morgan points out, the campaign has been written up "as though the politicians were a static 'given' " (p. 3). Morgan has attempted to fill this gap by presenting a detailed account of the interparty and intraparty machinations in the 1910–14 period, based upon the papers of leading Ministers and Opposition leaders. He has produced, in effect, an account of how women *failed* to persuade the Government to grant them the vote in the last years of "Liberal England," rather than an account of how they eventually were enfranchised after the 1914–18 war had revolutionized British politics and society.

Morgan's detailed analysis of the considerations that motivated the politicians' attitude to the suffragists serves as a timely reminder that with electoral reform more than most issues, questions of principle (is this proposal "a good thing" in itself?) are likely to be subordinated to considerations of short-term partisan advantage. (How will this proposal affect our immediate electoral prospects?) He illustrates just how complex was the party lineup on the suffrage issue after the 1910 elections. While some unionist diehards were opposed completely to votes for women, the party as a whole favored a limited measure that would enfranchise only women from the property-owning classes. The Labour and Irish parties, holding the Parliamentary balance of power after 1910, were broadly sympathetic to the women's cause. Labour, however, was unwilling to support any measure as long as the whole franchise was based upon a property qualification, while the Irish were wary of any measure that would use up the Parliamentary time and good-will that was required for the passage of a Home Rule Bill. For their part, the Liberals, though sympathetic toward electoral reform, were afraid of jeopardizing their plans for the abolition of plural voting by linking it with a votes for women proposal. Eventually, the influence of Asquith was decisive in causing the Cabinet to place the suffrage issue fairly low on its list of legislative priorities in the crucial sessions of 1911 and 1912.

All in all, Morgan has produced an impressive case-study of British Parliamentary party relationships, and of the British Cabinet at work. The history he recounts clearly has its lessons for contemporary British politics. Political scientists and practicing politicians, as well as historians, can learn much from this account of the pressures to which a British Government is subject when (be it 1977 or 1912) it lacks a working majority in Parliament, faces competing demands for legislation on the major constitutional issues of home rule and electoral reform, and is divided within its own ranks on these issues.

R. M. PUNNETT

University of Strathclyde, Scotland

Revolution at Querétaro: The Mexican Constitutional Convention of 1916–1917. By E. V. Niemeyer, Jr. (Austin: University of Texas Press, 1974. Pp. 297. $10.00.)

It is good for students of comparative constitution-making to have a book like Niemeyer's, to remind us of the different paths by which modern nationhood may be approached. The title is well chosen, stressing as it does that the distinctive feature of the Mexican revolution was not the armed conflict of 1910–1915.

After all, that phase merely displayed the same characteristics of violence, disorganization, and power struggles as other revolutions and rebellions of history. The real revolution and the real victory lay in the adoption of the first truly modern constitution, responsive to the needs of the twentieth century, incorporating not only the old eighteenth century notions of political rights but also the completely new concepts of social and economic guarantees.

For comparison it is worth noting that in 1952 when the Commonwealth of Puerto Rico included some mild clauses in its constitution referring to the right to work and to obtain an education, the U.S. Congress refused to ratify the document until those clauses were deleted. They were held to be "inappropriate" and "unrealistic."

Niemeyer's chronicle shows us how the three unique articles of the constitution – No. 27, reaffirming national ownership of subsoil resources and outlining an agrarian reform program; No. 123, specifying the rights of labor; and No. 130 (the so-called "anticlerical" article), regulating religious activities – took shape during the convention itself, with no prior examples to guide the delegates. The chronicle further reveals that another revolution took place at the convention, one in which the forces of the Left, captained by Francisco Múgica and Heriberto Jara, outmaneuvered the timid liberals, achieving adoption of a document that went far beyond the aims of Venustiano Carranza, who had convoked the gathering.

Describing the vote on Article 3 (providing for free secular education) as the most significant of the convention, Niemeyer states that the outcome "showed . . . that the delegates would decide the great issues according to their own convictions and not according to loyalty to any particular revolutionary chieftain or interest groups" (p. 78). This is one of the few occasions in which the author permits himself to interpret the record. For the most part, he seems to stand diffidently on the sidelines, presenting unadorned to the reader the chronological account of the convention sessions and a few thumbnail biographical sketches of the delegates. His selection of quotations, particularly those used as epigraphs, contributes little to the reader's understanding of the issues involved.

On the other hand, he manages to convey the general atmosphere of provincialism, roughness, and crudity from which the remarkable document emerged, like an ugly duckling. The student of American history cannot avoid comparing such quotations with the polished pronouncements of delegates to our own constitutional convention, many of whom were graduates of the Inns of the Court. At some points, Niemeyer's almost Victorian delicacy prevents him from giving the full flavor of his subjects' words, as when he mistranslates the remarks of Manuel Amaya, the presiding officer at the preliminary sessions, who had left the hall for a few moments. On returning, Amaya announced from the chair, "No salgo para fumar sino para mear" (p. 53, fn.). The author substitutes the latinized form of the final verb instead of the more accurate Anglo Saxon *piss*.

Feminists will be grateful to the author for his coverage of the background which led to omission of guarantees of women's rights. In an atmosphere of barroom *bonhomie*, the delegates on one occasion discussed the matter of rape in connection with their own libidinous proclivities, and on another occasion rejected out of hand Felix Palavicini's lone plea for woman suffrage (pp. 203–210).

Although there is no document comparable to Madison's journal, Niemeyer's painstaking reading of available materials helps fill the gaps in our understanding of the processes through which the revolutionary articles evolved. We are told, for example, that two days before adjournment, after almost frantic activity of the drafting committee, Article 27 was presented, discussed, and voted on in one evening, "with only three or four copies of the draft available" to the 220 delegates (p. 143).

A Mexican lawyer once told me, "we have the best constitution in the world, and some day it will be a reality." Niemeyer's book renders a service to the English-reading public in showing how that constitution was adopted. Reality may have to wait a while yet.

EVELYN P. STEVENS

Cleveland, Ohio

Saints and Politicians: Essays in the Organization of a Senegalese Peasant Society. By Donal B. Cruise O'Brien. (New York: Cambridge University Press, 1975. Pp. 200. $13.95.)

One of the most vexing questions about developing states concerns the nature of relations between the urban, modernized national center and the rural, traditional structures. This can be examined in many ways, such as the role of national political structures, the tutelary notions and kleptocratic penchant of leaders, the effect of ethnic fragmentation, or the growth of class struggle. O'Brien focuses on two aspects of social structure which tend toward a measure of stability in Senegal: one is a given, the dominance of the Wolof; another, more crucial, is the role played by Wolof leaders. Most Wolof are organized in Muslim brotherhoods and monopolize the production of the country's only cash crop; therefore Muslim saints – particularly among the Mourides – have become indispensable intermediaries as they enjoy peasant legitimacy, control and dispense optimal material rewards, and use their political base to extract or deflect demands from the national center, thus helping to provide an "underlying coherence of Senegalese politics" (p. 13).

The present cohesion of the Wolof is the result of historical accident and of leadership by three kinds of actors: warlord, saint, and knight. Before the French conquest, they were divided into two factions struggling for ideological and political supremacy: military Muslims, and "backsliding semi-pagans" controlled by chiefs and warriors. When the remnants of Wolof armies fell to the French, new leaders emerged and reorganized the disoriented Wolof into brotherhoods combining religious puritanism with a new economic base. Warlords faded from the scene because of their conservatism. Saints, seen as ascetic scholars endowed with Weberian charisma, provided the religious and ideological base for change. And knights, marching in on behalf of the saints, placed their organizational skills and ambitions at the disposal of the new order. The role played by such leaders is well researched and presented with drama and sympathy, but also emerges as a function of chance as much as planning and initiative.

O'Brien suggests that the current stereotype of the Mouride peasant as coldly exploited by religious confidence men is accurate only in part. French land grants, distributed without social discrimination, reinforced the saints' religious legitimacy and material power to draw converts from disenchanted lower castes. Brotherhoods like the Mourides thus serve three main functions: spiritual rewards on the way to paradise after death, material rewards providing a living standard better than that of most rural Senegalese, and political rewards through pooled wealth used to purchase favors from the national government. Brotherhoods operate in a system of "creative indigenous adaptation" which serves as an important channel for "patronage redistribution from the centre to the localities" (p. 177), an informal context in which the saints remain the major actors. Peasants thus behave rationally since they have some access to political influence. Although they are exploited, they have resisted other types of particularism because of overarching religious allegiance combined with minimally optimal materials rewards.

The author points out that disregard for formal rules was institutionalized during the colonial period, and distinguishes four styles of elite predation: bureaucrat, party politician, private trader, and saint (President Senghor's fulminations against corruption, incidentally, are directed at the first three and seldom the latter). Since only the saint has roots in peasant society, and since he combines moral sanctions with extractive skills which benefit followers, only he is a predator with an effective sense of noblesse oblige.

This volume largely achieves its stated objectives: a historical review of colonially-induced changes, and "an interpretation of the present situation which does some justice to its real complexities." The book is also a bit *décousu*, the common property of most collections of previously disparate essays. The treatment of the Wolof, especially of the Mourides, is obviously sympathetic, and the materials used provide a useful corrective to conclusions reached by other observers. The "meek did really inherit the earth," at least for a while. But the author also sees that the saints' control is slipping, if only for lack of additional land – they prefer short-term speculative gain to long-range productive investment in the national economy. In the end, earlier adaptation and innovation seem to have lost steam, and the question of the saints' future viability remains open.

W. A. E. SKURNIK

University of Colorado, Boulder

Political Development and Political Parties in Turkey. By Ersin Onulduran. (Ankara, Turkey: Ankara University Press, 1974. Pp. 116. TL 12.50.)

State and Society in the Politics of Turkey's Development. By Ilkay Sunar. (Ankara, Turkey: Ankara University Press, 1974. Pp. ix, 196. TL 20.)

The literature on modernization in Turkey continues to grow, and fortunately much of it is being produced by Turkish social scientists. Few "developing" countries have as good an indigenous body of scholars as Turkey, scholars who can study themselves with considerable objectivity and add an invaluable dimension to both theoretical and practical guidelines for appreciating the contributions which can be made by study of this extremely interesting country. Its quarter century of uninterrupted multiparty politics is a rarity among "developing" nations. How this has happened, and what are its implications, are the subjects of these two books, both originally written as doctoral dissertations at American universities.

Onulduran traces the role of political parties in Turkish "modernization." While he breaks little new ground that has not been covered in parts of other studies, his small book is useful in enabling those who are not experts on Turkey to get an overview unavailable elsewhere. A discussion of modernization theory is followed by some pre-Republic history, leading up to an analysis of the roles of contemporary major parties. The three most important ones (Republican, Democratic, Justice) are discussed in terms of their platforms, tactics, and social bases. Onulduran also supplies a very interesting analysis of their voting strength among different educational, occupational, and regional groupings.

Onulduran concludes that several important conditions of "political development" have been met, including firm anchoring of party politics; he also avers, perhaps most significantly, that the two major parties are essentially moderate and thus appeal to the electorate increasingly in terms of issues. He holds that since, in addition, mass participation is fully accepted, new groups are constantly emerging and making politics ever more pluralist and ever more flexibly attuned to the needs of the nonelites.

Sunar's book is much more ambitious, but it is striking that he basically comes to the same kinds of conclusions. Sunar traces the development of state and society from Ottoman times to the present. He sees the integration – or lack of it – of state and society as the major variable which led to the failure of most reform attempts in the Empire, and he sees shortcomings in such integration as accounting for many contemporary Turkish problems. These deficiencies include the rapidity but unevenness of social and economic change, and the conservatism of those very leaders who might have helped Turkey toward more rapid and basic change. Sunar criticizes a number of important theories, such as Halpern's ideas about the modernizing role of the New Middle Class, and some of Kemal Karpat's hypotheses about Ottoman development. He correctly concludes that working-class, collectivist movements which flourished in the 1960s failed basically because despite the rapid change which Turkey has undergone, socially conservative attitudes and structures have persisted (e.g., "urbanization rather than industrialization has been a decisive factor in the development of class relations in Turkey" [p. 151]).

I share the concluson stated or implied in both books that the populists rather than the Marxists will have the best chance of continuing Turkish modernization and that "if there is any chance of further progress in Turkey, it rests largely with those who can appeal to broad strata of society and incorporate them effectively into the polity" (Sunar, p. 185). Possibly the most intriguing fact about Turkey, however, is that it has developed *two* large parties, the moderate left Republican People's Party which has revitalized itself after bearing the image of twenty-five years of an earlier autocratic period, and the moderate right Justice Party under whose auspices much of Turkey's recent development has taken place. Each of these parties has based its strength on a combination of many strata, and this fact has enabled Turkey thus far to continue both development and multiparty politics. What will bear close watching is whether one of them, and if so *which* one, will be able to handle the ever more difficult problems of a now fully mobilized population. For example, can multiparty politics in a "developing" nation produce leaders who are courageous and strong enough to effect both mass and elite changes which will improve economic performance by mobilizing the latent talents of the lower strata? Such actions are likely not only to diminish the power of some currently dominant groups and to demand greater production discipline from all, but must at the same time respect the conservative social values of most of the Turkish population so as to minimize the social and personal dislocation of many Turks whose retreat might well be to the radical right rather than to the left.

On specific points in both books experts on Turkey will dissent. In addition, Onulduran presents some problems in terminology (is "the nonelite professions" a useful term, for example?); and one wishes (among other things) that Sunar had done as thorough an analysis of international aspects for the current period of Turkish development as he did for the Ottoman period. But both authors clearly indicate (and the 1973 and 1977 elections corroborated the point) that a relatively modern, issue-oriented, and discerning electorate has emerged in Turkey as a result of a half-century of continued political development. For all students of political development who want to see whether Turkey's extremely interesting future challenges can be met, both Sunar and Onulduran provide a solid background.

WALTER F. WEIKER

Rutgers University at Newark

Revolution Administered: Agrarianism and Communism in Bulgaria. By Nissan Oren. (Baltimore, Md.: The Johns Hopkins University Press, 1973. Pp. 204. $8.50, cloth; $4.00, paper.)

Of all the Communist countries of Eastern Europe Bulgaria has been the least affected by the winds of change that have swept across the continent in the last decade. Whereas first Yugoslavia, then Albania, and finally Rumania have strongly resisted Soviet hegemony, Bulgaria has sought to strengthen its ties with Moscow, especially since 1968 – which has made it an anomaly in an area that has become the stronghold of "national communism." One of the consequences of Bulgaria's docility in foreign affairs and of its internal orthodoxy has been a general lack of concern with – and informed opinion about – Bulgarian developments, not only among the general public but within the scholarly community in the West as a whole. While studies of Poland, Hungary, the GDR, Czechoslovakia – not to mention Bulgaria's Balkan neighbors Yugoslavia, Rumania, and Albania – continue to proliferate, Western scholars have generally adopted an attitude of "benign neglect" toward Bulgaria.

In light of the scant scholarly attention given Bulgaria in the West – especially since the Communist take-over – Nissan Oren's *Revolution Administered: Agrarianism and Communism in Bulgaria* is a welcome addition to a rather bare cupboard. His book is the eighth and last in the series of monographs by Jan Triska on "community building" and "integration" among Communist states in Eastern Europe. The emphasis in the series, as Triska notes in the foreword, is on viewing these states as a "system" and on trying to find "operational indicators" that facilitate systematic comparisons.

Despite the attempt to pay lip service to the conceptual framework of the Triska series, however, Oren's book is more a political history than a systematic analysis of Bulgarian politics. The basic problem is that the book falls between two stools. It does not really add much conceptual insight in terms of "operational indicators." At the same time the need to conform to the general framework of the series prevented Oren from doing full justice to the complexity of Bulgarian developments, as he did so admirably in his earlier work, *Bulgarian Communism: The Road to Power, 1934–44* (New York: Columbia University Press, 1971).

As it stands, the book presents a rather uneven treatment of Bulgarian politics. The first four chapters (about half the book) deal with the period prior to the takeover by the Bulgarian Communist Party on September 9, 1944. Here Oren is at his best, tracing the ins and outs of party strife and discussing the factors that facilitated the takeover. In relation to the rest of the book, however, this section is too long – especially for a book that purports to treat Bulgaria in the postwar period and facilitate comparisons with other Communist states in Eastern Europe. By comparison the treatment of the years after 1949, particularly the period after 1966, is rather sketchy and in places superficial. For instance, with the exception of the treatment of the Macedonian question, there is little discussion of Bulgaria's interaction with its Balkan neighbors in recent years or of the interesting period in 1965–1966 when Bulgaria displayed an unusual degree of diplomatic boldness and came close to establishing diplomatic relations with Bonn. Similarly, little attention is given to agricultural developments, especially the establishment of the agro-industrial complexes (AICs), an area in which Bulgaria has been uncharacteristically innovative in comparison to other East European countries.

Finally, although intraparty politics are discussed in some detail, little attempt is made to assess the role and leadership of Todor Zhivkov, who after all has now been in power longer than any other head of a Warsaw Pact country (23 years). Admittedly Zhivkov is a dull and lackluster figure in comparison to other Bulgarian leaders like Stamboliski, Dimitrov, and King Boris, whose personalities and politics are extensively and commendably discussed by Oren. Nevertheless, Zhivkov's impact on Bulgarian politics has been just as great as, if not greater than, that of the others, and his role deserves more attention than Oren has allotted to it.

The rather sketchy and uneven treatment of developments since 1966 is all the more regrettable because from the comparative point of view this period is perhaps the most interesting. As noted earlier, there has been an unfortunate tendency among Western scholars to dismiss Bulgaria out of hand as an irredeemable bastion of orthodoxy hardly worthy of attention, and to concentrate instead on other East European countries such as Yugoslavia, Hungary, or pre-1968 Czechoslovakia, whose pattern of reform has tended toward greater economic and political decentralization. This tendency has reflected an inherent, if unconscious, bias on the part of Western students of Communist societies, who have tended to associate modernization with greater economic and social pluralism. When little evidence of such pluralism can be found – as in the case of

Bulgaria – many Western scholars have seen this as evidence of "stagnation" or "degeneration."

This unconscious bias is present in Oren's book as well. It is hard to question his concluding judgment that as a result of the takeover of the BCP "[domestic] politics was converted into mere public administration and international politics all but lost its meaning." From an analytical point of view, however, Bulgaria's "deviation" from the pattern of political developments elsewhere in Eastern Europe is precisely what makes it interesting. The fact is all too often overlooked that behind its rather placid facade Bulgaria has developed a distinct pattern of modernization that combines high economic growth with tightly controlled economic and political centralization. This pattern contrasts markedly with the pattern of modernization elsewhere in Eastern Europe, particularly Hungary. Yet the Bulgarian "model" is one the Soviet Union would like to see developed in other East European countries (one of the main reasons Moscow has been willing to underwrite Bulgaria's modernization with extensive credits), and, given present trends in Eastern Europe, it may become even more relevant to developments in Eastern Europe in the years to come. It therefore deserves more attention from Western scholars than it has received to date.

In sum, Oren's book is a highly readable, if uneven, essay on Bulgarian politics, more useful to the undergraduate desiring a general overview than to the specialist concerned with acquiring a detailed understanding of postwar Bulgarian politics or to the methodologically oriented scholar interested in comparative analysis. To a large extent owing to the limitations imposed by the framework of the series of which it is a part, *The Revolution Administered* is not really a sequel to Oren's earlier, admirable study, *Bulgarian Communism: The Road to Power, 1933–44,* referred to above, which will long remain the definitive work on this period. The specialist can only hope, therefore, that Oren will one day be able to turn his attention to writing a real sequel, because he is uniquely qualified to do so.

F. STEPHEN LARRABEE

Munich, West Germany

Contrasting Approaches to Strategic Arms Control. Edited by Robert L. Pfaltzgraff, Jr. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 350. Price unknown.)

The book before us is a compilation of papers prepared in connection with the Sixth International Arms Control Symposium held in Philadelphia late in 1973. Robert L. Pfaltzgraff, Jr., Director of the Foreign Policy Research Institute, has served as editor; he and some fourteen other authorities on arms control have contributed essays. The focus of the book is on recent developments in strategic arms limitation, with specific reference to the implications of SALT I and the forthcoming SALT II discussions. Relevant documentary materials on the subject are included in three appendices.

As is not uncommon in symposia, the individual chapters vary considerably in interest, cogency, and the originality of their ideas. Not infrequently, the contributors advance contradictory conclusions about important trends and their implications.

In company with several other authorities, William R. Van Cleave is dubious about the benefits accruing to the United States from the SALT I agreement – and he is convinced that the SALT II stage of "qualitative" arms reduction will prove equally disadvantageous for America. His contention is that very few Americans have adequately understood the political and strategic motivations behind Soviet behavior on the disarmament question.

In Graham T. Allison's view, the Soviet-American "interaction" with regard to the steady escalation in strategic weapons is much more complex and ambiguous than is often supposed. Allison doubts, for example, that competition for more destructive weapons is sparked by some new development in which one side suddenly finds itself threatened (e.g., America's decision to develop MIRV missiles was *not* essentially a reaction to Russia's acquisition of the ABM). Conversely, a decision by Washington to limit its strategic and conventional forces would not necessarily lead to a Soviet decision to do likewise.

Morton A. Kaplan analyzes the extent to which SALT I, and potentially SALT II, affect the Soviet-American strategic balance, construing this broadly to include psychological and political aspects of national power. Various Soviet spokesmen are cited to the effect that the global balance now and in the future favors the Communist side. Kaplan's verdict is that the outcome of SALT I "in many respects . . . provides both for symbolic Russian superiority and potential actual superiority" (p. 78); and that SALT I and SALT II pose "deadly risks to American interests. . ." (p. 85).

Robert L. Pfaltzgraff believes that the value of SALT I for the United States will be heavily conditioned by the results of SALT II (and he is fundamentally skeptical about American gains from the latter). Uri Ra'anan is even more doubtful about the benefits for the United States of recent arms-control measures. Groups favoring disarmament negotiations, he believes, routinely overlook (or discount) the extent to which arms limitation is favored by the Kremlin, as a tactic to promote its long-run political and strategic objectives, which have not really changed despite *détente.*

Several contributors discuss the regional implications of SALT I and SALT II. Joseph I. Coffey, for example, is apprehensive about the impact of SALT I upon the cohesiveness and effectiveness of NATO and the cause of Western unity generally. Gaston J. Sigur and Makoto Momoi appraise the consequences of recent arms-control agreements upon Japan's internal and external policies.

The role of Communist China in attempts to limit strategic armaments is evaluated in detail by Harry G. Gelber. Characterizing Peking as a "silent partner" in the SALT talks, he calls attention to the dilemma posed for policy makers in Washington and Moscow by China's growing military might. On the one hand, the inclusion of China in the SALT negotiations would almost certainly guarantee their failure. On the other hand, China's exclusion from these discussions of course vitiates many of the beneficial results of a Soviet-American understanding on arms-control. Gelber believes there is considerable validity in Chou En-lai's observation that SALT I was a sham. For its part, Peking shows little or no evidence that it desires to slow down the steady buildup of its own strategic arsenal or that it has abandoned a desire to achieve nuclear parity with Russia and America.

In one of the more original essays in the collection, George H. Quester advances a novel (if not altogether persuasive) thesis on the subject of nuclear proliferation. From the evidence thus far, attempts to prevent such proliferation have obviously failed. Yet Quester is not excessively pessimistic about the future. The American public – along with other "publics that matter" – has evidently lost interest in, and become bored with, the whole problem of nuclear deterrence. Rather sanguinely, Quester interprets this phenomenon as dampening the desire of "n-th countries" to join the nuclear club.

In an incisive essay, James E. Dougherty analyzes developments on the strategic weapons from "From SALT I to SALT II.' On balance, with several other contributors, Dougherty's verdict is more negative than positive about the consequences of SALT I for American, and for overall global security; nor does he believe that the results of SALT I improved the negotiating stance of the United States in the forthcoming attempt to impose "qualitative" restrictions upon strategic weapons. Appropriately, he cautions American students of deterrence and disarmament particularly against a prevalent tendency to assume too much "rationality" in the behavior of nuclear-armed states. On the basis of recent experience in arms-control conferences, he finds no evidence that Soviet officials actually desire, or are willing to settle for, mere military "parity" with the United States. His own estimate is that Russia's military position vis-à-vis the United States is likely to improve during the remainder of the decade. After a detailed analysis of the results of SALT I, Jacquelyn K. Davis concludes that any significant reduction in America's defense effort is unjustified. She adduces several reasons why new defense projects – along with accelerated progress in research and development – ought to be completed by the United States.

As a group, these essays on an important public issue are interestingly and ably written, objective, scholarly, and provocative. They highlight several aspects of the problem of arms-control which are often neglected by other commentators. Their collective impact is to raise substantial questions – and to express major doubts – about the gains accruing to the United States from the SALT I accord and about the advantages which may be anticipated from SALT II.

The overall verdict of the contributors is less than optimistic concerning the value of recent arms-limitation agreements. Yet in most of the essays, the authors successfully steer a middle course between the kind of mindless euphoria which events like SALT I and SALT II sometimes generate in both the official and the public mind, and the kind of cynicism which devotees of *Realpolitik* often display toward any new attempt by the superpowers to reduce global tensions. Implicitly, this volume warns the reader that neither attitude is warranted; and it calls attention to a host of new, important, and difficult problems which inevitably grow out of limited arms-control agreements in the contemporary world.

CECIL V. CRABB, JR.

Louisiana State University

Willy Brandt: Portrait of a Statesman. By Terence Prittie. (New York: Schocken Books, 1974. Pp. 356. $10.50.)

Although Prittie's is not a good book, it merits brief discussion. First, the author, a long-time German correspondent for the *Manchester Guardian,* has, for prolonged periods, provided us with our main sources on both Adenauer and Brandt. Adenauer has never received an overabundance of biographical attention, and Brandt is just now beginning his career as biographical subject. Second, and much more important, this biography provides an all-too-perfect example of work which makes those of us in the discipline who are interested in developing a "scientific" approach to the study of political leadership, despair. In particular, Prittie's book serves as an unwelcome reminder that academicians eager to investigate the *contemporary* political leader are almost inevitably stuck with impressionistic materials submitted by random observers, or with a book such as this, which pretends to be a life history but which is, in fact, little more than a life chronology and a flawed one at that.

Far be it from me to chastise Prittie for all the sins of his kind. He never claims social scientists (or historians) as his intended audience. Is it possible then, that were we to shed our guise as members of the academy, we would find a new virtue here? Not likely. Indeed, by posing the question, we pinpoint the methodological and substantive flaw: a biography such as Prittie's is written *without* any clear audience in mind and is, as such, a bastardized form. This book, for example, is quite carefully footnoted and presumably aspires, therefore, to a certain respectability among scholars. Regrettably though, it lacks a complete historical accuracy as well as a social scientific exploration of causality. The lay person is offered no compensation; the literary style is so pedestrian that the reader is denied even a rudimentary aesthetic and/or vicarious pleasure in perusing the story of one of the most strikingly exciting lives in modern political history.

Prittie has taken his cues from Brandt's own autobiography (*My Road to Berlin* [London: Peter Davies, 1960]). That is, the tone of his book and the materials included pick up from Brandt – *not* from where Brandt left off. (In the case of personal and professional developments after 1959, Prittie continues the pattern of elaborating to the same limited degree and in the same cautionary mode.) The effect of this submission to subject is that the biography suffers from the same distortions, inclusions, and omissions as the autobiography. But, of course, what is tolerated as an inevitable subjectivity in the autobiographer is not excused in the supposedly more objective life historian.

For instance, Prittie allots precisely sixteen pages to the first nineteen years of Brandt's life. (Brandt had spent thirty on approximately the same span.) The twenty-eight years between 1945 and 1973 are given 249 pages. (About 110 of these cover his life as a "statesman.") This absurd imbalance occurs not only because access to information about Brandt's life after 1945 is relatively easy to obtain or because this more recent history is virtually harmless in terms of contemporary German politics. It also indicates the author's failure to shed light on those aspects of Brandt's history which are more controversial with regard to his personal life. Nor does he even begin to explore the private motivations underlying Brandt's public behavior. The effect of this abdication goes beyond stretching our patience with omission; we are forced to cast doubt on that information with which we *are* provided.

Cases in point from the early years: (1) We are told that Brandt "became an active participant in discussions and plans" (p. 23) for the International Bureau of Revolutionary Youth (1934). We are not told about the role Brandt continued to play in this organization or about the effect that the ensuing debacle had on his subsequent political development. (2) We are told that Brandt went to Spain in 1937 "*primarily* as a correspondent of Norwegian and Swedish newspapers" (p. 32). We are not told that the primary reason for his Spanish stay was political (he was to promote coordination of the parties of the Left), and the important pamphlet Brandt wrote on this subject does not receive a single reference. (3) Prittie mentions, in passing, the name of Jacob Walcher as SAP colleague. But, almost certainly because Walcher settled in East Germany after the war, he does not tell us of the major impact Walcher had on Brandt circa 1933–39.

Prittie does offer a quite accurate account of Brandt's professional life and progress after 1948, and he devotes some care to placing him within the broader political environment – i.e., within the German, European, and East-West settings. But beyond that, his book does not illuminate nor fully inform. In spite of the author's affective bias and because of his refusal to probe beneath the event, the account remains bloodless. If the book has any importance at all, it is by default. Where do researchers, teachers and students turn for single case analyses of subjects with manifest political impact – subjects who are not already six feet under?

BARBARA KELLERMAN

Tufts University

Transition in Africa: From Direct Rule to Independence. A Memoir. By Sir James Robertson. (New York: Barnes & Noble Books, 1974. Pp. 272. $15.00.)

Sir James Robertson was one of that elite corps of colonial administrators – an Oxford graduate with a second in "Greats" and a Blue in rugby – who ruled the British Empire with a minimum of fuss and expense and a great deal of integrity and skill. He entered the Sudan Political Service in 1922 and stayed there until 1953, just two years before the Sudan became independent. He served for almost twenty years in the provincial service and then rose to be Civil Secretary, the senior officer in the S.P.S. In 1955 he was appointed Governor General of Nigeria and saw that country through to independence in 1960. Thus his career spanned the period of colonial trusteeship to independence when power was transferred to Africans.

The District Officer – that jack-of-all-trades – was the key to good government. Sir James vividly portrays all aspects of the D.O.'s work – anthropologist, builder, policeman, judge, councillor, tax collector, animal and agricultural expert. The interwar period was probably the golden era of colonial governance, falling as it did between the harshness of the pioneer days and the clashes with African nationalists after World War II.

The political process whereby Africans in the Sudan and Nigeria got Great Britain to waive its rule is clearly charted by Robertson. The key was to maintain the cooperation of Africans while avoiding precipitous withdrawal or using undue force to remain. How new states were formed out of colonies by colonial officials and indigenous leaders is essentially the subject matter of this book. Robertson provides much information on his difficulties over policies in the southern Sudan from 1945 to 1953 and in forming Nigeria's three regions into a federation.

This is a good study and a personal document by one who ruled in Africa and helped transfer sovereignty to Africans.

PETER DUIGNAN

Hoover Institute

Authority, Participation and Cultural Change in China. Edited by Stuart R. Schram. (New York and London: Cambridge University Press, 1973. Pp. viii, 350. $17.00, cloth; $4.95, paper.)

What was the Cultural Revolution of 1966–69? What were the causes of its emergence and development? Was it a power struggle among the top leaders, or a massive rebellion of the youth against the authority of Party and government? Was it a violent clash between the opposing ideologies, a struggle for domination among key institutional forces, or an inflamed confrontation between policy alternatives and development strategies?

Today, even as the turmoil and violence of the movement have long subsided, the answer to these basic questions, and their practical significance and theoretical implications for China's political process and social transformation are still being vehemently debated among students of contemporary China. The field has benefited in the last several years from a number of solid works on the Cultural Revolution by competent scholars like Richard D. Baum, Robert J. Lifton, Lowell Dittmer, Edward E. Rice, and Thomas Robinson. These works have indeed contributed greatly to our detailed knowledge of the dynamics of power politics and ideological disputes. Yet few studies have focused specifically and systematically on the question of concrete policies and their impact on the course of development after the Cultural Revolution.

Hence, we are indebted to the publication of the volume under review. Representing a significant departure from the over-stressed perspective of power politics, the volume addresses itself primarily to the question of policy innovations and organizational changes during and after the Cultural Revolution and relates these to the problem of leadership and participation in China's social change and economic development. A major objective of the book, as the editor puts it, is to answer the question, "what has happened to China as a result of the Cultural Revolution?" (p. 1).

The volume is a product of the efforts of a group of European scholars concerned with contemporary China, through a series of research conferences held in 1971 and 1972, organized under the energetic leadership of Stuart R. Schram, then director of the Contemporary China Institute at the School of Oriental and African Studies in London. The eight contributors are all scholars of impeccable qualifications, competent both in the subject matters and in the Chinese language. Being Europeans, some have even enjoyed the special advantages of being able to work for an extended period of time in the People's Republic of China.

This volume consists of six topical essays by different authors with an editor's introduction. The 108-page long introductory essay by Stuart R. Schram illuminates the book from the very beginning with a penetrating analysis of the key variables of contemporary China's political process and revolutionary movements, such as cultural traditions, competing ideologies, policy choices, development strategies and dominant political personalities. The Introduction also presents a masterful synthesis of the interplay of these variables from a historical perspective, which places the Cultural Revolution in the broad context of modern China's turbulent search for national survival and development. With rare eloquence and skill, Schram demonstrates that Mao as a political genius has not only had a sophisticated grasp of the forces at work in Chinese society but has successfully manipulated such forces with imagination and courage for the attainment of his revolutionary goals. Moreover, with new rigor and fresh insight Schram raises the fundamental question of the extent to which China under Mao's leadership can continue to borrow Marxist-Leninist and other Western "techniques" *(yung)* of organization and modernization and adapt them to serve China's needs for development without eroding what Mao perceives to be the very "essence" *(t'i)* of the Chinese revolution. This was, of course, the central question that the political reformers of modern China asked themselves a century ago. Interestingly enough, the very same question is again asked today by political leaders and students of politics alike, who are concerned with the processes of modernization and political development in the Third World.

The six topical essays collected in this volume, with the exception of Jack Gray's, focus on the introduction and development of policy innovations and organizational reforms during and after the Cultural Revolution. Unfortunately, the subject matters treated lean disporportionately toward the economic sector, probably because of an uneven distribution of research interests among the participants of the project. Four out of the total of six articles fall into the realm of economic planning and management. Aside from Jack Gray's historical survey of the development of the struggle between the "two lines" with special attention to agricultural mechanization, Marianne Bastid emphasizes the trend toward what she characterizes as "decentralization" of economic initiatives and decision making at various levels and units; while Jon Sigurdson treats the increasing transfer of technology to the rural sector for a more balanced pattern of modernization; and Christopher Howe deals with changes in labor organization and the incentive system in the urban sector. In the last two essays, the far-reaching reforms instituted in the educational system are taken up by John Gardner and Wilt Idema, and the impact of the cultural revolution on social relations in selected sectors of population is examined by Andrew J. Watson with emphasis on attitudes toward authority and social participation.

While analyzing problems from different perspectives, conceptual frameworks and data bases, the authors seem to have reached a consensus that the basic policy and organizational lines introduced through the Cultural Revolution are by no means new and unfamiliar. All of them can easily be traced back to Mao's previous political experiences and theoretical writings. But the magnitude of the efforts to implement and sustain the innovations clearly exceed those witnessed in the past. The writers also agree that the impact of the Cultural Revolution varies from sector to sector. Bastid, Sigurdson, and Gardner conclude that the innovations in the areas of agricultural development, rural industrialization, and educational revolutionization are bound to have a far-reaching and positive effects on their future development, while Howe and Watson believe that the effects of radical changes pushed during the Cultural Revolution in the urban industrial sector and in social relations are at most mixed and may be of only short-range significance.

As the dust of the Cultural Revolution continues to settle and the new pattern of organizational behavior and policy orientations takes on a more identifiable trend, a definitive assessment of the impact of the Cultural Revolution will undoubtedly become easier. But, for now, this volume has clearly distinguished itself as a pioneering work in the analysis of the strategy and trend of development of the post-Cultural Revolution period. No student concerned with contemporary China can afford to overlook the stimulating analyses and insights offered in this volume.

MICHAEL Y. M. KAU

Brown University

The Wartime Alliance and the Zonal Division of Germany. By Tony Sharp. (New York: Oxford University Press, 1975. Pp. 220. $19.25.)

Drawing upon Foreign Office and Cabinet papers that have recently become available to scholars, unpublished or unused American documents, and the recollections of participants in the events under examination, Tony Sharp has presented a meticulously detailed account of "how between 1943 and 1945 Hitler's *Reich* and Berlin its capital were divided respectively into four zones and sectors" (p. 1).

In his book Sharp focuses on two themes: the planning for and negotiations that led to Germany's zonal division, and the military context within which this took place. Although much of the information on these events has long been common knowledge, "the interaction between negotiations and military strategy has not been presented before" (p. 1). The author's basic thesis is that these negotiations can only be understood if viewed in terms of their military context. Thus it is these interrelationships between military and diplomatic developments that provide the ordering framework for the book.

The book begins with a summary overview of the general relationship between the course of negotiations on the zonal protocol and the military situation before and at the time of Yalta. Sharp then examines in detail interallied planning for the postwar order of Germany beginning in late 1942, on through the Yalta Conference, to the withdrawal of British and American forces back to the agreed upon zones of occupation, the setting up of the Allied Control Council and the entry of Western troops into Berlin in July of 1945. Except for the treatment of the tripartite negotiations in the European Advisory Commission, the study focuses on the development of the British and American (and later the French) positions. Here interesting and new material is offered on the disputes between Washington and London over occupation of the north-west zone (with its North Sea ports), control of the Bremen-Bremerhaven area, and other policy disagreements. In a concluding chapter, a comprehensive discussion of the extensive negotiations over the French zone of occupation and the several proposals for the allocation of the sectors in Berlin is presented.

Sharp argues that the planning and negotiations on the different proposals for the zonal division were colored by an "awareness that the nature of the postwar order would be largely determined by the respective position of the Anglo-American and Russian armies at the end of the war" (p. 3). He examines in detail how the momentary military situation and the projected course of future operations, as well as the perceptions of what the Russians were likely to do formed the background for the Western allies' planning and negotiations. He shows how these factors influenced the positions taken as the allies sought to consolidate their hold on what they felt they were likely to get, as well as to guarantee an appropriate share of what the other side might capture. In this sense, the protocol on the division of Germany into zones of occupation was seen as "legal insurance" against a major conquest of Germany by one side or the other.

In the chapter on the allied advance into Germany Sharp points out how the protocol itself was an important political factor in the debates on military strategy between the American and British, e.g., the advance to the north, the desirability of retaining the capture of Berlin as a primary military objective, and the link-up with advancing Russian forces. The presence of Western forces in parts of the Russian zone of occupation at war's end seemed, particularly to the British, to offer an opportunity to renegotiate parts of the agreement and to resolve other outstanding issues between the West and the Soviet Union. Here Sharp discusses the significant disagreements between the British and the Americans as to whether and how the timing of withdrawal should be linked to attempts to bargain with the Soviets.

Although many of these events have been treated separately before, Sharp points out that until now "all the aspects of zonal division have never been dealt with together" (p. 2). In addition, much that has been written about in general outline before has been "supplemented and clarified by a wealth of detail" (p. 2). And this "wealth of detail" is both a strength and weakness of the book. Such an abundance of material is often hard to digest, even though the interweaving of the themes of negotiations and military context provides the reader with some point of orientation for moving through the information presented. Still the macro frame of reference is not enough to order the myriad relationships and interactions among the different planning bodies and individuals that continuously enter and leave the study at the micro level. It is when trying to keep the multifarious actors apart and to thread through the proposals and counter proposals that a reader may wish Sharp were less the compleat historian and more an analyst of the decision processes themselves through which the protocol was shaped.

With this book Sharp has given us a comprehensive and exhaustive account of an important set of policy decisions and of some of the factors that influenced them. As such this study is more suited for reference than for general reading or classroom use. Its greatest utility would seem to be as a source of information for more theoretical or general analysis of these events and the complex interactions among institutions and personalities out of which the plans for military operations and the diplomatic moves emerged.

KENNETH HANF

International Institute of Management, West Berlin

The Lions of Dagbon: Political Change in Northern Ghana. By Martin Staniland. (New York: Cambridge University Press, 1975. Pp. 241. $22.50.)

Martin Staniland's book is a political history of the Dagomba people, a group of almost 300,000 people spread over 8,000 square miles of savannah in northern Ghana. The study focuses upon the position of the Ya-Na, the paramount chief of the Dagomba. Although not stated explicitly in the book, one might conclude that three factors have led Staniland to study the paramount chief of the Dagomba, as opposed to the chiefs of other Ghanaian societies. First, Dagomba society has preserved much of its traditional character, and the position of the Ya-Na, while very different from what it was in precolonial times, demonstrates much more historical continuity than paramount chieftaincy in other parts of Ghana, where the forces of modernization have exerted themselves more powerfully. Second, the rules governing the selection of the new Ya-Na are complex and intriguing, and consequently the process of succession constitutes the core of the study. Third, violent disputes costing many lives have occurred in recent years over who should hold the position.

Professor Staniland points out that the process of succession, like other facets of chieftaincy, has been buffeted by a series of disruptive and threatening forces over the past seventy years. First British colonial administrators strove to adapt the Ya-Na's position to varied and shifting models of colonial rule. The theme of indirect rule prevailed, but controversies raged about what indirect rule meant for Dagomba. Colonial administrators could be classified into two groups: "For one school, efficiency was the final test; for the other authenticity. The paternalism of one could not, in the last resort, be reconciled with the traditionalism of the other" (p. 82).

Additionally, according to Staniland, new demands placed upon the Ya-Na and his subordinate chiefs to promote modern education and economic development and fit their kingdom into the frame of the emerging nation-state of Ghana, cast doubt on the appropriateness of the rules of succession. These rules accorded prime importance to royal blood and longevity, with the result that many Ya-Nas were senile by the time they attained the office.

The traditional patterns were also challenged by the inevitable involvement of Dagomba in national politics. Forces in conflict at the center supported the various disputing lineages in Dagomba. The winners at the center rewarded their supporters at the Dagomba periphery by altering the rules of succession so that their man became Ya-Na. This history of central government intervention has meant that "the proliferation of government measures defining, regulating, and protecting chieftaincy signified a further degradation of the political status of the institution. Its formal status was preserved as its major functions were removed" (p. 132). The central authorities (successively colonial, parliamentary, one-party, and military) have demonstrated their ineptness in comprehending and settling questions of political succession in this traditional polity.

The study is full of historical detail, and if the reader is able to compare the Dagomba experience with other traditional polities in Ghana and elsewhere, the book provides the basis for many insights. One wishes, however, that the author had provided more reflection and comparison. Moreover, when he does offer generalizations, he sometimes treats Dagomba as if it were all of Ghana. For instance, in a concluding remark, Staniland asserts that the book has demonstrated the "resilience and vitality of chieftaincy and politics in Ghana" (p. 175). Rarely, however, does he make reference to other parts of Ghana. The utilization of comparative material would probably have led Staniland to note the unusual resilience of chieftaincy in Dagomba, rather than its typicality. Nevertheless, Staniland's understanding of Dagomba is very considerable, and the intelligence with which he handles his data makes this book significant for those interested in the political history of traditional polities in Africa, and the transformation they have undergone as they interact with various types of national governments.

DAVID R. SMOCK

The Ford Foundation, Nairobi

Death of a Utopia: The Development and Decline of Student Movements in Europe. By Gianni Statera. (New York: Oxford University Press, 1975. Pp. 294. $10.95.)

The main thesis of this book is that the student protest movement of the 1960s was "an original phenomenon of collective behavior involving young people," shaped largely by a set of utopian ideas. Through a careful and detailed analysis of the student movements in

Germany, Italy, and France and informative illustrations from other European student movements the author shows that this set of chiliastic and utopian ideas (à la Karl Mannheim) greatly appealed to vast numbers of students living and studying in old, hierarchic, autocratic, overcrowded, and understaffed institutions. A situation of mass action was consequently created and was greatly responsible for the initial great success of the protest. However, as soon as utopia was replaced by ideology, that is to say by dogmatic Marxist-Leninist ideology and centralized and disciplined action, the movement declined. "Betrayed" by their leaders, the students refused to avail themselves for further action and brought the movement to its abrupt end.

The use of the concepts of utopia and ideology to denote the two historical phases of the European student movement is based of course on Karl Mannheim's sociology of knowledge, and one of the virtues of Statera's study is that this is done very convincingly. These concepts help to focus our attention on the unreducible political character of the groups concerned and enable us to understand not only why a large number of young people were available for mass action, but how did it happen that specific chiliastic ideas, developed by Herbert Marcuse, Rudi Dutschke, Daniel Cohen-Bendit and others have come to play such an important role in the protest as motivating symbols for action. Most of these ideas have thus far been considered by students of traditional political philosophy and by social critics as unsystematic *ergo* unimportant. Statera's analysis makes a significant contribution to the correction of this approach and his theoretical arguments are satisfactorily backed by rich documentation and detailed historical accounts. His emphasis on the political and ideological dimensions of the students protest does another important service for the students of the protest phenomenon. It helps them to understand the intense fragmentation of many of the *"groupuscouls"* (small groups) and the splits among student *groupuscouls,* leftist parties, and adult organizations. The result is that the reader of the book is provided with a study which is both very informative in its content and properly theoretical in its form – not a small achievement in comparison with other existing studies.

To be impressed by many of Statera's accomplishments in this book is not, however, to agree with all his arguments. I would like here to take an issue with the main thesis of the book, regarding the decline of the student movement. The decline is seen by Statera to be a result of the replacement of the creative utopian thinking by the dogmatic ideological one. What we have here, in my opinion, is a classical case of the *post hoc ergo propter hoc* fallacy. What Statera skillfully demonstrates is that toward 1969 there was (1) a marked decline in the intensity of protest, (2) a significant decline of the utopian elements within the protest movement, and (3) a significant ideological transformation of many of the *groupuscouls* in a more dogmatic and ideological (traditional Marxist-Leninist) direction. Nowhere does he show however that the decline of the intensity of protest was *a result* of the other two developments. For such a proposition to hold, Statera should have provided us either with one empirical case where continuing utopian zeal had retained a high level of intense protest for a longer period of time or with an empirical example in which an early "ideological" takeover prevented an intense protest from occurring. He does not perform these necessary operations for a very simple reason: such examples do not exist.

A close examination of the students protest movements in Europe would show that among many of the *groupuscoules,* "ideologization" *followed* the failure of the spontaneous protests to accomplish major revolutionary goals and was a direct result of this failure and of the consequent decline of the protests. What actually happened was that after the decline of the protest activities, many of the radical activists came to the conclusion that a serious revolution could not be carried out through the spontaneous and nondisciplined action of students and would be conceivable, if at all, only through the well-planned activity of disciplined revolutionary organizations appealing to the workers. This, of course, led them to the rediscovery of Lenin's lessons. The fact that this rediscovery has brought about the unfortunate rejection of some of the creative elements of utopia cannot be denied, but its depiction as a "betrayal" of the movement and as the cause for its decline is unjustifiable and untenable.

In spite of its strength and appeal, Statera's analysis lacks a whole structural dimension essential to the understanding of the protest movement of the 1960s, i.e., the process of its delegitimization. The really important transformation that the movement has undergone has not been from utopia to ideology but from a moderate opposition to the powers that be, to an intense rejection of their legitimacy, expressed by new antiregimist symbolic language and by sheer violence. Only the very active elements within the movement were ready to bear the full consequences of the crisis of legitimacy that ensued, the repressive means applied by the governments concerned, the growing resentment of the population, and the consequent need to go underground with no hopes for immediate success. Most of the protesting students were never revolutionary, were exhausted by the experience of protest, and became satisfied with the academic reforms achieved as a result of their earlier activity. They refused to respond further to groups such as *La Gauche Proletarienne* in France or *Baader-Meinhoff* in Germany. But had these groups remained utopian, had they retained all the creative elements that Statera identified within the earlier stages of the protest it would have made no difference.

EHUD SPRINZAK

Hebrew University of Jerusalem

Protest and Response in Mexico. By Evelyn P. Stevens. (Cambridge, Mass.: The Massachusetts Institute of Technology Press, 1974. Pp. vi, 372. $17.95.)

With this work, Professor Stevens not only joins the growing ranks of scholars critical of the ruling elite in Mexico, but offers one of the most devastating commentaries about the contemporary Mexican political scene that has appeared in recent years. The author's principal contention is that Mexico is ruled by an authoritarian regime which, in contrast to democratic or totalitarian arrangements, is characterized by lack of a well-defined ideology, an absence of political mobilization, and limited pluralism.

Dr. Stevens proceeds to show that there is a remarkably predictable pattern in the Mexican government's response to protest movements that do not bear the official seal of approval. After a brief period of confusion and indecisiòn, which she attributes to a temporary failure in communication channels and to the excessive dependence of Mexican bureaucrats on the ultimate problem-solving initiative of the president, the government characteristically evades real negotiation, although it might at first voice promises to "study" the protestors' demands. These are only "half a carrot" promises, however, which the government never really intends to keep. Instead, the authorities invariably embark upon a massive campaign aimed at discrediting and dividing the protest groups, and at penalizing the leaders of such groups.

While the author acknowledges that the intensity of the government's actions varies from situation to situation, depending on the nature of the demonstrations, she nevertheless maintains that variation in response is relatively minor, and then mainly in determining what "justice" shall be meted out to key offenders. Measures could range in severity anywhere from blacklisting, exile, and imprisonment, to kidnapping or even more violent types. In every situation, however, the authorities will enlist the support of a servile legislature, a largely unprofessional press, and government-sponsored unions in their confrontations with protest groups, whose bona fide spokesmen are depicted by the government as Communist agitators, foreign agents, or even Mexican traitors.

To substantiate her findings, Dr. Stevens avails herself of three well-publicized cases of protest against the Mexican regime: the railroad workers' strike in 1958–59; the doctors' strike in 1964–65; and the student revolt prior to the 1968 Olympics. She concludes that in each of these situations the regime's response was repressive, and that "in trying to deal with the new kinds of protest which arose in the 1960s, the regime did not become either more democratic or more totalitarian; it became more authoritarian" (p. 13).

On the whole, Professor Stevens's analysis is insightful, as she is able to get to the underlying truth of much of what goes on in Mexican official circles. While she does come to the right conclusions, she does so not because of methodological sophistication, but because she is both intuitive and clearly familiar with the Mexican character. In fact, the principal flaws in this volume are methodological, and they regrettably detract from some of the author's main arguments.

Most notable is her failure to exploit fully her field and survey research, which she asserts was conducted in Mexico in 1965–66 and later in 1968, and included more than two hundred interviews of persons who participated in the strikes. There is very little evidence, either in the text or in the footnotes, that she actually utilized these resources in any meaningful or systematic manner. For example, the reader learns that Professor Stevens is a frequent visitor to Mexico only by means of occasional references, such as: "a series of frustrating experiences in 1973 involving out-of-order telephone lines, convinced the author that telephone communication is still a somewhat hazardous venture" (p. 59). She was apparently not an on-the-scene observer of the strike demonstrations; most of her references are drawn from press releases (whose reliability she had already questioned), previously published accounts, and the testimony of respondents (which she admits obtaining in some instances many years after the subjects had been involved in the protest movements). Can Dr. Stevens attain her avowed goal of describing the *behavior* of strikers and government officials, rather than their *attitudes*, without having been there in the midst of things?

Similarly, it seems hardly worth the effort to conduct so many interviews when the author is unwilling to produce any systematic results of her findings. Instead, she is merely inclined to generalize about her respondents, with typical asides such as "two of the physicians interviewed for this study assured the author repeatedly that their phone wires had been tapped during the strike period" (p. 61). Did she not submit a standard questionnaire to the physicians she interviewed? If she did, shouldn't she tell the reader what percentage shared the view expressed by the two physicians? Also, why does she consistently refer to her respondents as "informants" when she has explicitly expressed reservations about previous studies on Mexico in which "too much effort has been expended in trying to force the square peg of political reality into the round hole of previously formulated theory?" (pp. 10–11). Granted that Mexicans regard revelations to foreigners of the activities of their government an unpardonable indiscretion; yet the choice of label for her respondents does seem to betray biases which she herself holds.

Despite these serious problems, this is, as noted earlier, a very provocative study. The author's examination of many aspects of social custom that have a direct bearing on the informal character of government activities in Mexico is, to say the least, most revealing, as is her account of the subtleties of what she terms "communication, Mexican style" – a process understood by Mexicans, but seldom by perplexed foreign observers. From all indications, Professor Stevens is an admirer of the work of Mexican poet Octavio Paz. While her own contribution is no *Labyrinth of Solitude*, hers comes as close to it in some respects as any other work by a non-Mexican. This, in itself, should be enough to forgive her for any "sins" of omission or commission.

FRANZ A. VON SAUER

Oklahoma State University

Land Reform and Politics: A Comparative Analysis. By Hung-chao Tai. (Berkeley: University of California Press, 1974. Pp. 586. $25.00.)

This comparative study of the land reform process in eight nations (Taiwan, the Philippines, India, Pakistan, Iran, Egypt, Colombia, and Mexico) provides an excellent summary and empirical assessment of the major political issues involved in the redistribution of land. Tai's work is theoretically and conceptually unpretentious. The questions he asks are those with which policy makers are most concerned – e.g., what are the political and economic consequences of agrarian inequality; which land reforms are most successful in terms of equity and productivity, does land reform produce political stability, what kinds of political coalitions are most effective at carrying out land reform? In the course of answering these and other questions, Tai presents the reader with a host of statistical and qualitative comparisons that are very illuminating.

The major hypotheses can be succinctly stated in the author's own words:

> 1) The perception of the need for legitimacy prompts the political elite to initiate land reform; 2) the relationship between the initiating elite and the landed class determines the manner of program formulation and content of the adopted program; and 3) the political commitment decisively affects the extent of program implementation (p. 468).

Some of these hypotheses may seem self-evident or even tautological, but they are not. For example, in discussing the second proposition, Tai convincingly discounts the role of peasant mobilization in most land reform programs and shows how the autonomy of the elite from landed interests is often decisive. There was little popular clamor for land reform in either Taiwan or Iran. Instead it was the Kuomintang's need to build a peasant political base and the Shah of Iran's need to break the hold of aristocratic landowners which made extensive land distribution a desirable political goal. Tai concludes that autocratic governments which can move quickly are, other things being equal, more capable of successfully executing a land reform program than are parliamentary systems such as Colombia and, until recently, India and the Philippines, where the electoral power of landowners can generally block any but the most token reform programs.

After dealing with the origin and implementation of land reform, Tai moves, in the second portion of the book, to consider the impact of land reform on rural political participation, national integration, institutionalization of rural organizations, rural stability, and the appeal of communism. As one might expect, he concludes that land reform "has a definite and positive contribution to make to these aspects of political development" (p. 468). It is, to be sure, a rather gross intrusion of ideology to assume that whatever diminishes the appeal of communism is a contribution to development. The reader, however, may dismiss the author's prejudices and still profit from Tai's discussion and argument.

Tai's analysis of the perennial question of the relationship between distribution and productivity will be of interest to many readers. It is a commonplace that productivity may be expected to fall in the immediate aftermath of a land reform. The data, however, fail to support this conclusion and suggest, in fact, that productivity more often remains constant or even rises. Although the revolutionary turmoil that may accompany the struggle for reform may diminish productivity, these effects are not attributable to the reform itself. Tai does document the impressive and nearly universal tendency for productivity (both in terms of output per unit of land *and* output per unit of labor) to increase greatly in the years following a thorough reform. His case would have been somewhat weakened by the inclusion of such deviant cases as Burma, but I believe his general analysis is convincing. Finally, Tai somewhat more speculatively suggests that, in the long run, the proliferation of small family farming units may eventually work against productivity gains. Evidence along these lines comes largely from the comparison between the *ejido* and non-*ejido* sectors of Mexican agriculture. In any event it is clear that new institutional arrangements are often necessary to allow small household farms to share the cost of lumpy inputs and to market cooperatively if they are to remain viable.

It is regrettable that Tai should have borrowed the most simplistic form of relative deprivation theory as the basis from which to argue the explosive consequences of agrarian inequities. There are by now convincing logical and empirical reasons to doubt the connection between some "want-get gap" deduced from economic statistics and stratification, on the one hand, and the radical mobilization of the peasantry on the other. Without examining the structure of the peasantry, its social organization, and its political history, as well as the role of the landed class and the state, it is clearly impossible to claim that rural inequalities lead in any simple way to revolutionary radicalism. If rural exploitation were alone sufficient to generate rebellion, most of the Third World would be in flames. One senses that Tai wishes to convince conservative elites that they must reform or be overthrown. The evidence, alas, should suggest to Tai as well as to those who took the Alliance for Progress ideology seriously, that reactionary regimes have generally contrived to reject reform and to employ other means of remaining firmly in power.

One major conclusion which emerges from Tai's analysis – a conclusion which I find as chilling as he finds encouraging – is that a reformed peasant sector can offer a conservative regime the kind of social base which can then be deployed successfully against urban and middle-class dissidents. One major effect of an extensive land reform is to remove the landed class which hitherto mediated the relations between the peasantry and the state and thereby to allow the state directly to penetrate the countryside. Typically, the state or ruling party then establishes a network of peasant organizations through which the beneficiaries of reform become dependent on, and identified with, the official bureaucracy. Once the peasant receives his land it is likely that he and his class will willingly, even enthusiastically, accede to the corporatist plans of the new elite. Tai goes so far as to suggest that it is cheaper to buy the compliance of the peasant through land reform than to deal with the insatiable demands of the urban working class (p. 437). Once assured of peasant support or quiescence, the elite can then beat back the more radical threats of students and workers in the cities. This conservative postreform coalition is not just an abstract possibility, for one need only look at Mexico, Egypt, Iran, and Taiwan to appreciate how land reform can demobilize the peasantry and work against further structural change. Looked at from another perspective, therefore, land reform may constitute the keystone of a counterrevolutionary strategy.

JAMES C. SCOTT

Yale University

The Future of Democracy in Latin America. By Frank Tannenbaum. Edited by Joseph Maier and Richard W. Weatherhead. (New York: Alfred A. Knopf, 1974. Pp. 246. $10.00.)

This book commemorates the late Frank Tannenbaum's thirty years of scholarly contributions as an outstanding Latin Americanist and professor at Columbia University. The two editors have accomplished this goal by writing a forty-five page biographical essay, followed by their selection of thirteen essays, both published and unpublished, to illustrate his approach to the understanding of Latin American problems.

The biographical essay on Frank Tannenbaum is exceptionally good since it consistently evaluates how his life experiences affected both his philosophy of knowledge and his belief about how a scholar should operate. The editors, both former students of Frank Tannenbaum, have looked at his life closely and empathetically present his scholarly outlook. They show in detail how his experience as a labor organizer in New York City and his consequent sentencing to a year in jail kept him from becoming a radical ideologist and helped him recognize the limitations of institutions and human beings. His later writings on labor unions, penal reform, and Latin America reflected these insights.

The next part of the biographical essay describes his especially close relationship with Columbia University, including the intellectual stimulation he derived as a student, his teaching approaches, his love for the University, and the special weekly seminars he started on Latin American problems that became a permanent fixture. The editors evaluate these seminars to illustrate how he tried to break down the traditional barriers among the academic disciplines by inviting persons from labor, business, government, other universities, and Latin America to thoroughly dissect a problem for the purposes of gaining understanding, the use of different approaches, and perspective.

Professors Maier and Weatherhead's description of Frank Tannenbaum's interest and research on the Mexican revolution fills out the remainder of the biographical essay. The editors describe his travels in Mexico, his personal friendship with Lázaro Cárdenas and other leaders, and the value he placed on such personal knowledge for research. They also discuss his general attitude toward the accomplishments and obstacles of the revolution. The central themes of his three books on Mexico are then briefly mentioned. Greater elaboration of these books would have been valuable, especially in evaluating the originality of their research findings and in analyzing how well they have stood the test of time.

The editors describe Frank Tannenbaum as a social philosopher. This is an apt characterization and would seem to mesh with what he himself recommended in one of the papers as proper training for social scientists in the Latin American field. He recommended that students have a good grounding in literature, cultural anthropology, and social psychology in order to understand the culture, people, and the inner basis of political and institutional life. Of equal importance was a minimum of two years of travel in several countries, developing personal contacts, and observing especially life outside the cities.

The thirteen papers in the book are a motley group, consisting of four unpublished essays, and articles that have appeared in *Foreign Affairs, The Year Book of World Affairs, Foro Internacional, The Hispanic American Historical Review, Political Science Quarterly, Estudios Antropológicos,* and the *Journal of World History.* No excerpts from Tannenbaum's books on Mexico have been included, on the ground that the editors felt that the lesser-known essays deserved a larger readership. Most of the essays illustrate the special scholarly approach Frank Tannenbaum used to dissect problems. Unfortunately, the editors have provided no introductions to the essays and have not grouped the essays in any coherent way. Thus one has constantly to refer to points made in the biographical essay to provide a linkage. The Latin American specialist can do this on his own, but it may be much more difficult for the general reader.

The articles fall into different types. Three are on the obstacles to and the conditions which facilitate political stability in Latin America. In these the problems of personalism, *caudillos,* transfer of power, political parties, local government, political culture, and the difficulty in finding a source and symbol of legitimacy are brought out. These articles will be of value for the general reader for a first dip into the area. The specialist will find some insights, and can react one way or another to the importance given some ideas, such as that strong local rural communities must constitute the base of democracy. Those who insist on rigorous conceptual research designs will not be satisfied with these articles, and the analysis is more appropriate for the dictatorial politics of the 1950s than for recent happenings.

I found three articles outstanding in opening up new ways of approaching a problem. The best is an extended logical dissection of Arnold Toynbee's claim that Latin America must choose between economic efficiency and social justice. Tannenbaum argues that the dichotomy is false and meaningless, and he adduces evidence from Latin American politics. In an article on adaptation in the Mexican revolution, the author draws on his knowledge of Mexico to stress the pragmatic and nonideological approach that was used by the many leaders to tackle problems as they came up. These two articles belong together since Professor Tannenbaum insists that the turbulence and unpredictability of Latin American politics leave no time to discuss rational ideological alternatives. The third article on the incorporation of the sizable Indian communities in many Latin American countries is provocative since the author argues that real governmental progress in this area could very well lead to the surfacing of long-standing Indian hatreds and to the emergence of separatist dissident nationalistic movements.

ROBERT D. TOMASEK

University of Kansas

China, the Struggle for Power, 1917–1972. By Richard C. Thornton. (Bloomington: Indiana University Press, 1974. Pp. 403. $15.00, cloth; $3.95, paper.)

This book on Chinese politics is divided into three parts in chronological order. Part I deals with the origins and development of the Chinese Communist movement and covers the years 1917–41. Detailed are Soviet Russian support and interference in the Chinese Communist movement, power struggles among the Chinese Communist leaders, and the Kuomintang-Communist rivalry. A part of this section is based on the author's earlier book *The Comintern and the Chinese Communists, 1928–31* (Seattle: University of Washington Press, 1969). Part II covers the period 1941–49 and is entitled "The American Experience in China." The struggle between the Communists and the Nationalists in China in this period is seen as a part of the global power struggle. As this struggle gathered momentum towards the end of World War II, it upset the Roosevelt-Stalin design to fashion a postwar balance of power in the Pacific, a design requiring a unified China under the KMT leadership, strong enough to keep domestic peace, but not so much as to challenge the authority of the two big powers.

Part III covers the period 1949–72, from the founding of the Chinese People's Republic, through the turmoil of the Great Proletarian Cultural Revolution, to the demise of Lin Piao. The genesis of the Cultural Revolution is traced to the CPR's program for China's industrialization and to the realization that such a program must be independent of Russian control and manipulation. In this light, the Great Leap Forward and the Sino-Soviet split led directly to the Cultural Revolution; the Sino-Soviet split had caused polarization of Chinese leaders to such an extent that Mao had to launch the Cultural Revolution to fortify his positions. Finally, considerations of China's defense needs, particularly along her northern border, brought the Cultural Revolution to an abrupt halt in late 1968. The Chinese now regard the presence of the USA in the western Pacific as a deterrent to a precipitate Russian move into Peking; after the Russian intervention in Prague in 1968, this spectre has been haunting the Chinese.

Professor Thornton has interpreted a number of historical events from new points of view. Emphasizing the early Chinese Communist movement as a part of the worldwide revolutionary conspiracy, he reconstructs Mao's difficult and protracted rise to power inside the CCP. According to him, Mao was opposed to the United Front as late as August 1937. The United Front, a Russian-designed strategy, was then being implemented in China by the Russian-returned group, the very group Mao had to discredit. Mao embraced the strategy as his own only after he achieved top leadership. Thus, Thornton puts Mao's ascendancy to top leadership at sometime after August 1937.

Also, Thornton sees the New Fourth Army incident as an instance of intraparty struggle between Wang Ming and Mao, while other scholars generally treat the incident as a case of KMT-CCP interparty struggle.

Concerning U.S. involvement in China, Thornton questions the efficacy of the activities of some junior officers of the U.S. Foreign Service in wartime China, particularly in their undermining of Patrick Hurley's credibility and representativeness of U.S. intentions. The author pinpoints the initial success of the Marshall Mission as the "seeds" of the American failure in China. With the then superior Nationalist troops restrained from attacking the inferior Communist forces, the Communists became "no longer interested in reaching a negotiated settlement in which their position would be weak" (p. 204).

Thornton singles out the battle of Szepingchieh (1946) as the turning point of the civil war in China. The battle marked the zenith of Nationalist power, soon after which, the Nationalists assumed a defensive strategy. Thornton attributes this change to an "unwise" U.S. embargo on ammunitions shipped to Chiang Kai-shek's government, which caused Chiang to adopt holding operations to conserve ammunitions. The embargo was unwise, Thornton stresses, because at the same time Soviet Russia was helping Lin Piao rebuild his Fourth Field Army. He argues that if the U.S. had not withheld ammunitions from Chiang in the crucial months of 1946 the outcome of the Chinese civil war might have been very different. Thornton does not point out that the Nationalists' inefficient use of the military stemmed more crucially from many previous and endemic problems other than supplies of ammunitions. The Nationalists' defeats were never directly due to lack of ammunitions: witness the stockpiles abandoned by the re-treating and surrendering Nationalist troops in 1948–49. The Chinese Communists' ability to organize men shows forth even as Thornton meticulously compares the weapons and ammunition of the two sides. He also questions the soundness of the Truman-Acheson hands-off policy in the last stage of the Chinese civil war. "Who Lost China?" thus still is an issue for the author.

Thornton defines history as the record of man's achievement and aspiration; "the task of the historian is to measure the one in terms of the other" (p. vi). Throughout the book, he adheres closely to his task. He advances hypotheses to interpret events, and cites sources for support. His reconstruction of events such as the Cultural Revolution and search for the motives of the major actors are on the whole plausible and sometimes ingenious. In emphasizing the international context of the Chinese power struggle, however, he does not cover adequately the socioeconomic basis of Chinese politics and ideological commitment of the opposing groups. On the other hand, the strength of this book lies precisely in its "international" focus. As such it is a valuable addition to works on Chinese politics.

I must add, though, that the author's rendition into English of Chinese place names should have been more consistent. On p. 110, it was not the Kwantung Army that captured Nanking in December 1937. On p. 346, the sequence of footnotes 21 and 24 should be rearranged. Most regrettable is the omission of a comprehensive bibliography at the end of the volume.

SUSAN H. MARSH

Brown University

Vietnamese Communism: Its Origins and Development. By Robert F. Turner. (Stanford, Calif.: Hoover Institution Press, 1975. Pp. xxix, 517. $14.95.)

The Indochinese Communist Party – the genesis of communism in Vietnam – was founded more than forty years ago, in 1930. This voluminous study of the history of the Communist movement in Vietnam graphically relates its changes and development through the years: including a mass of information about the Indochinese Communist Party, the Vietnamese Worker's Party, and the People's Revolutionary Party – all of which, according to this authority, formed one Communist Party in Vietnam. In tracing this development of Vietnamese communism, focus is directed on these three important questions: (1) How did the Vietnamese Communists gain control of the anti-French resistance movement and use it to their advantage? (2) How did the Vietnam Communists manage to maintain control in North Vietnam after the Dien Bien Phu victory over the French in 1954? (3) What has been the relationship of the Vietnamese Communists to international communism?

In various places within the work the author discusses other intricate questions which academic teachers and other thinking Americans have pondered with reference to Vietnam and communism. To cite a few: What role did North Vietnam and the Communist Party play in the recent Vietnam War? Why did the 1973 Paris Agreement fail to bring the desired peace in South Vietnam? A question provoking real debate centers on Ho Chi Minh as a Vietnamese nationalist and international Communist – as a nationalist, did he use Communist support to liberate Vietnam from Western imperialism; or, was he at heart an international Communist who used slogans of nationalism to his advantage?

The author, a Public Affairs Fellow at the Hoover Institution on War, Revolution and Peace, has made an intensive study of Vietnamese communism for more than a decade. This probing has not been performed in an isolated academic setting but is rooted in hard field research during four valuable trips to Vietnam. While in Vietnam, the author gathered a large amount of raw material from which this definitive account of Vietnamese communism has been written. His knowledge of the Vietnamese language has enabled the author to make a wide use of vernacular documents. Among the most valuable research sources he used in this study are many Vietnamese Communist publications – books, pamphlets, newspapers, and periodicals (e.g., *Hoc Tap, Nhan Dan, Lao Dong*), as well as Communist radio broadcasts. Beyond that, he used numerous accounts of personal interviews with defectors from the North Vietnamese Army and refugees who witnessed the various stages of development in North Vietnam.

The writer appears to have made an earnest effort to document all factual statements or assertions in the body of the text which might be questioned by the reader; every page abounds with detailed footnotes – thus displaying the prodigious work expended to insure accuracy. For example, to clarify and expand the fact that Ho Chi Minh – given an entirely different name at his birth in 1890 – was known by numerous pseudonyms, Mr. Turner in a most lengthy footnote mentions the twenty-eight actual names by which Ho Chi Minh was known during his lifetime. One well-known name this Vietnamese leader used from 1918 for a quarter of a century, was Nguyen Ai Quoc, which translated means Nguyen who loves his country, or Nguyen the Patriot. In 1939 when this Communist leader was in China he first used the name Ho Chi Minh, meaning "he who aspires to enlighten." It was by this name that he became well known from 1942 to his death in 1969.

Appropriately this work opens with a most informative biographical sketch of Ho Chi Minh, the man who established the Communist movement in Vietnam. Born into the home of a "patriotic scholar," or nationalist, he naturally emerged as a young Vietnamese leader – who later became converted to Lenin's doctrines, received training in Moscow, was a Communist agent in China, organized Vietnamese youth into a Communist group, neutralized nationalist competition to the wider Communist goal, and worked hand in hand with Moscow for the establishment in Vietnam of a Communist Party in 1930. Near the end of World War II Ho Chi Minh was recruited in Kunming, China, in March 1945 to serve as an intelligence agent in Vietnam for the U.S. Office of Strategic Services, using the code name of Lucius. Some years later he was misunderstood and rebuffed by the Washington administrations, a costly error which eventually caused the U.S. to become hopelessly entangled in the recent Vietnamese conflict.

In closing, it is significant to note that two-fifths of the volume comprises salient documents relating to the Vietnamese Communist Party and the international Communist movement. It is a veritable source book for the diligent researcher desiring to see the actual text of such documents – many now translated into English for the first time. For example, these appendices include: *Lenin's theses on the national and colonial questions,* the treatise which converted Ho Chi Minh in 1920 from a nationalistic patriot to an international Communist and placed on him the responsibility for the application of Leninism to Vietnam. Among the other documents are: *Action Program of the Indochinese Communist Party; Declaration of Independence of the Democratic Republic of Vietnam; Manifesto and Program of the National Liberation Front of South Vietnam;* and Documents of the 1954 Geneva Conference, and the Paris Agreement on Vietnam in 1973.

CECIL HOBBS

Library of Congress

Communism in Scandinavia and Finland: Politics of Opportunity. By A. F. Upton. (Garden City, N.Y.: Anchor Press, 1973. Pp. 442. $2.95, paper.)

This volume is for the most part (nearly 300 pages) a history of the Finnish Communist Party by A. F. Upton, a professor of history at St. Andrews University, with short essays on the Danish and Norwegian parties by Peter P. Rohde, a free-lance writer who was expelled from the Danish party in 1953, and a more extensive essay on the Swedish party by Åke Sparring, Director of the Swedish Institute of International Affairs.

Sparring interprets the history of Swedish communism as a constant struggle between "reformers" and "dogmatists" (p. 62), and his discussion of the party's attitudes toward a parliamentary transition to socialism is of particular interest to students of international communism. The history of the Danish party is seen by Rohde as the withering away of a party that never was very impressive (pp. 7, 33), and his history of the Norwegian party is a brief description of the rise and fall (1923–69) of a once powerful movement (pp. 47, 59). Both Rohde and Sparring provide interesting glimpses of Communist leaders (e.g., Axel Larsen in Denmark, Peter Furubotn in Norway, and C. H. Hermansson in Sweden), although it is the Sparring contribution that stands out as the most significant of the essays.

Upton's treatment of the Finnish Communist Party, which he rightly sees as having much greater historical significance than its counterparts in the three Scandinavian countries, constitutes the core of the book. Some fifty years of party history are covered,with emphasis on the years 1918–66, and the nonspecialist may find something useful in Upton's description of the Finnish movement. It should be noted at the outset, however, that the reader will be hard-pressed to discern a theme. It is perhaps to be found in the author's reference to the "persistence and consistency" of the Communist vote over the years and to the ability of the party to hold the loyalty of its rank-and-file in spite of "all its absurdities and contradictions" (p. 341). Upton refers to "proof" (p. 350) that the Communists are able to maintain their hold on between 21 and 24 per cent of the Finnish voters, although the results of the 1970 and 1972 elections, in which the Communists got 16.6 and 17.0 per cent respectively, seem to have necessitated an insertion by Upton of two final paragraphs that contradict his theme and that provide little explanation for the reversal in the electoral fortunes of the Finnish Communist Party.

A more apparent weakness in Upton's treatment of the Finnish party is his heavy reliance on secondary sources. Upton refers repeatedly and uncritically to the memoirs of Arvo Tuominen, who was a prominent Communist in the interwar years, and to the memoirs of Yrjö Leino, a Communist who was Minister of the Interior in the early postwar period. Upton considers the fourth congress of the party a milestone in the development of Finnish communism (p. 142), but there is no indication that he has read the stenographic report for that congress. He refers to the Left Group of Finnish Workers (p. 186), but he did not consider it necessary to look at the unpublished minutes for meetings of the Group, even though the Group included leading Communist figures who broke with the party in the late 1920s. Upton refers to testimony contained in "police records" (p. 233), but one must conclude from his footnotes that he has not worked in the police archives. Primary sources are not even used for many direct quotations (e.g., Manner on p. 139, Sirola on p. 140, Kulmala on p. 174, Lenin on p. 210, Gylling on pp. 211–212, Paasikivi on p. 247, and Leino on p. 260).

The value of Upton's work is further undermined, at least in my eyes, by numerous errors (some important, some not so important) and by misleading statements. It is curious to read (p. 108) that leaders of the Finnish Social Democratic Party were being urged by "members from below" in the summer of 1917 to take power and institute a socialist revolution. It is not "beyond dispute" (p. 119) that between seventeen and twenty thousand Finnish Reds lost their lives in prison camps during the course of 1918. Edvard Gylling and Lenin were not "close personal friends" before the Finnish Civil War (p. 210), and President Urho Kekkonen would be surprised to learn that he "always favoured conciliation with the Soviet Union" (p. 313). Kustaa Rovio did not "control" (p. 213) the Autonomous Karelian Soviet Socialist Republic in 1923 (he became first secretary in 1929), and the hint that Gylling may have been released from a Soviet labor camp in the 1950s (p. 214) is at variance with the fact that he died in the 1940s. Last but not least, it is misleading to state that the Communist vote in Finland is "essentially an urban vote" (p. 344) when statistics show that the Finnish party has on occasion (e.g., 1966) won more than half its national vote from rural communes (as opposed to cities and towns).

In sum, the above weaknesses are of sufficient magnitude collectively to discourage the specialist for sure, and perhaps also the generalist, from turning to the Upton volume for knowledge about communism in the Nordic countries.

JOHN H. HODGSON

Syracuse University

North Vietnam's Strategy for Survival. By Jon M. Van Dyke. With a Foreword by Edwin O. Reischauer. (Palo Alto, Calif.: Pacific Books, 1972. Pp. 336. $10.95.)

Among the many irrational acts committed by the United States in the Second Indochina War, perhaps the most irrational was the bombing of the Democratic Republic of Vietnam. The decisions that led to the bombing and to specific targeting policies already have become a primary data source for the intellectual descendants of Professors Snyder, Bruck, Sapin, Paige, Hilsman, and Allison. The bombing is destined to rival the Korean War and the Cuban Missile Crisis as a case study in American foreign policy decision making. Curiously, scholars have made almost no effort to examine systematically the impact of the bombing on the country it was intended to defeat – the country which, in the end, defeated the United States. The persistence of parochialism about the Vietnam War among American social scientists – matched only by Pentagon planners and American presidents – is striking. One exception is Jon M. Van Dyke, whose *North Vietnam's Strategy for Survival* remains, five years after its publication, the most thorough account of how the DRV responded to the American "air war of destruction."

First drafted for the National Security Policy Seminar at Harvard University in 1966–67 under professors Leach, Quester, and Kissinger, and subsequently expanded to cover the period of the most intensive bombing through late 1969, the book describes the specific measures which the DRV employed to survive the bombing and at the same time match the American escalation of the war in the South. Defensive weapons systems, shelters and alert procedures, transport and communications maintenance techniques, mass mobilization, propaganda and labor redistribution measures, administrative decentralization, militia training, evacuation of population and dispersal of schools and industry, revision of agricultural and marketing systems, aid from abroad, and many other subjects are described in exhaustive – and mind-numbing – detail. The principal sources are DRV publications translated by the Joint Publications Research Service, supplemented by accounts of foreign observers and discussions with persons in the "Washington intelligence community." The author's hope is to "help explain the Vietnamese system of values and indicate why the strategy of bombing North Vietnam was not a successful strategy" (p. 24).

No matter how much damage the bombing caused, the DRV found compensatory means to maintain a tolerable livelihood for its people and was never deterred from continuing the war. The DRV achieved this mainly by virtue of its institutional effectiveness and popular support, and without the slightest cession of sovereignty to the Great Powers aiding it. Van Dyke presents an incontrovertible rebuttal to American strategists who hoped the Vietnamese would be less resilient than all the other peoples whom air power had failed to bomb into submission.

In spite of this admirable quality, the book has some weaknesses that arise because Van Dyke, professor at Hastings College of Law, is not a Vietnamologist and because he places a higher value on policy relevance than on social science. The subject which needs most to be discussed in a theoretical and comparative context, namely, the political and administrative response, is treated in the purely descriptive manner that characterizes the rest of the book, perhaps because the DRV political process is not well understood by nonspecialists. The social and cultural contexts are all but ignored. These shortcomings may be overlooked by someone interested only in the mechanics of how people and cadres coped, but the book makes no contribution to the sociological study of war.

Explanation of why the DRV chose certain policies and the promised illumination of Vietnamese values are left to the reader to draw for himself from the enormous pile of facts. For example, Van Dyke accurately reports such things as the ambivalence of urban school teachers and the government toward each other (pp. 139–140) but makes no reference to the traditional values and social composition of the teaching profession in the DRV which account for this ambivalence. He also can be led by nonexpert sources into such absurdities as saying, "the Muong . . . are ethnically related to the Chinese and . . . sometimes show more allegiance to Mao Tse-tung than to Ho Chi Minh" (p. 219). These shortcomings, however, do not undermine the book's central purpose of showing how the DRV survived "the most sophisticated and sustained bombing campaign history has yet known" (dust cover). While the effect of the war on the DRV still deserves further study, Van Dyke's book stands as evidence that Henry Kissinger either did not read seminar papers submitted to him at Harvard, chose to ignore them, or was driven by other factors than those he stated publicly when he supported Richard Nixon's decision to bomb Hanoi in December, 1972.

WILLIAM S. TURLEY

Southern Illinois University at Carbondale

Populism and Political Development in Latin America. By A. E. Van Niekerk. (Rotterdam: Rotterdam University Press, 1974. Pp. 230. $20.00.)

This is a brief though ambitious book, which is the published dissertation of the first Dutch Ph.D. specializing in Latin American politics at Rotterdam University. The focus is upon the characteristics and political impact of populistic movements in Latin America. The author reviews "theoretical" material in populism and political development, in attempting to define populism in Latin America largely in regard to its ideology, its relationship to political and economic development, and its implications for governance and coalition politics. Theoretical material is drawn partly from general works on political development by Apter and Nettl and partly from major Latin American scholars such as Furtado, Jaguaribe, Weffort, and Cardoso (e.g., some of the Brazilian social scientists who have studied populism). Moreover, six cases are reviewed: Brazil, Peru, Bolivia, Venezuela, Argentina, and Colombia.

Among his major arguments the author claims that populism functioned mainly to "integrate" varied political interests into national politics through coalition-building that served to co-opt newly organized groups arising from industrialization and urbanization in Latin America. Populistic movements, once in national power, utilized charismatic leadership, extensive patronage, and a division of the spoils of bureaucratization as sources of governance. Clearly, these sources were not unique to populism but were nonetheless used generally to control or channel political mobilization accompanying capitalist economic development.

Though the linkage between theoretical and case materials is not always clear, and though the author reviews mainly orthodox material without innovative or novel interpretation, the book is useful as a general overview of basic political development literature of the 1960s and of some major cases of populism. As I indicate below, the book would appeal more to beginning students of Latin American politics than to advanced professionals.

Unfortunately, the book does not provide sufficient nor explicit linkage between populism and current topics in Latin American political study. Recent research focuses upon the bases, organization, exercise, and policy consequences of national power in Latin America. While electoral politics are basic to populism since the 1930s, such politics have been removed recently in a number of nations (including many of those reviewed by the author) in favor of military-technocratic regimes. The author does not discuss populistic movements in relation to the recent rise and persistence of military regimes, to the reality of dependency of Latin American nations, to the political mechanisms of corporatism (which are fostered partly by populism itself), and to the question of political control over technology and economic development as central issues of power throughout Latin America. Nor does the author deal adequately with the question of political recruitment and bureaucratization in relation to populism and the structure and exercise of power in Latin America.

In sum, the book may give rise to nostalgia for those longing for populism and a degree of popular participation in Latin American politics. There is, however, little novel material in the study, though, as an overview of general and orthodox material for introductory students, it is a useful reference volume.

CLIFFORD KAUFMAN

Wayne State University

Uruguay: The Politics of Failure. By Martin Weinstein. (Westport, Conn.: Greenwood Press, 1975. Pp. 190. $13.50.)

Professor Weinstein's slim volume on Uruguay attempts to explain the prolonged political decay of one of Latin America's more modernized nations. The author argues that the traditional view of Uruguayan politics – that it was different from and superior to what existed elsewhere in the region – was a misconception, based on the assumption that Uruguay was a pluralistic, participant society that had through government initiative diminished class identities and inequalities. Weinstein sees Uruguay as a nation historically divided by class and ideology, unable to generate a cohesive national identity, and victimized politically by the same Ibero-American tradition of "corporatism" as its neighbors. He argues that Uruguay's outwardly democratic institutions were emasculated by its leaders' political bias toward "coparticipation," a system for sharing power which Weinstein illustrates in considerable detail. Coparticipation, he believes, rests on corporatist assumptions that there is a natural and legitimate order to societies and that once that order is established, elements in the society must not seek through political means to alter it radically or expand their power at the expense of others. Weinstein finds a basic contradiction in Uruguay between representative institutions and coparticipation, a contradiction that could be ignored so long as economic development and prosperity were sufficiently high to minimize conflict and competition in the political system. But during the extended period of economic decline that followed the Korean War, the contradiction created increasingly serious confrontations, and was responsible for the growing instability and eventual collapse of the political system.

Like most observers of Uruguyan politics, Weinstein traces the present dilemmas to José Batlle, who succeeded during the first three decades of the twentieth century in stabilizing the nation after seventy-five years of turbulence and civil war. Batlle tried to impose on the nation a visionary ideology of nationalism *(Batllismo)* which advocated the values of economic development, nationalization, social welfare and equality, and political stability, and which relied for its political support upon the rapidly expanding urban population of Montevideo. Weinstein points out that from the very outset Batlle was unable to crush the rural counterculture and its powerful elites, whose traditional exports were paying the cost of national transformation. His compromise solution was coparticipation, through which economic and political power was divided so as to guarantee a minimum share for minority interests. The author cites how the opposition party (Blancos) was given hegemony over those departments in the nation where its interests were strongest, and how minority opposition to Batlle even within his own party (Colorado) was recognized and protected through an unusual and carefully devised electoral system. There were experiments with dividing formal power between the parties in the legislature, and from 1952 to 1967, a collegial executive extended coparticipation to that branch of government.

Weinstein raises important and difficult themes, and his exploration of the corporatist tradition in Uruguay is often provocative. Unfortunately the book suffers from extraneous material and digressions, problems aggravated by its brevity. Some of the discussion of the revolutionary Tupamaro movement and the discussion of social classes is inadequately related to the general theme, and the diffuse theoretical framework provided at the outset is labored and unconvincing. However, the analysis does contain an alternative interpretation of Uruguayan politics which is both persuasive and timely.

RONALD H. MCDONALD

Syracuse University

The Chinese Calculus of Deterrence: India and Indochina. By Allen S. Whiting. (Ann Arbor: University of Michigan Press, 1975. Pp. 299. $15.00.)

This is clearly a major book, the most important one on Chinese foreign policy since Jay Taylor's *China and Southeast Asia.* The author, who is well known in China-watching circles, has had a distinguished official and academic career and has published a number of significant works, notably a standard book on China's entry into the Korean War. The study of Chinese crisis behavior and of the ways in which foreign powers, especially the United States, should behave in order to avoid war with China has been probably his main professional concern. Although one can of course legitimately differ with Professor Whiting's methodology (which stresses content analysis), his interpretations, and his conclusions, one can only respect the high level of factual knowledge and analytical insight that is displayed in his work.

The book under review, the author tells us, was written in lieu of a memoir of his years in governmental service. He is to be congratulated on avoiding reminiscence in favor of analysis. The book deals mainly with China's border war with India in late 1962 and Peking's more indirect role in the heyday of the Vietnam War (1965–68). For comparative purposes, some brief reflections on China's role in other crisis situations, including the Korean War and the Taiwan Strait crisis of 1958, are also included.

The first major section, which deals with the Sino-Indian border war, is generally excellent. To be sure, it makes a bit too much use of content analysis, tends to take Peking's propaganda statements at face value, attaches too much causative importance to Peking's concern over American subversive activities in Tibet, and relies unduly on the excessively anti-Indian account of Neville Maxwell, but these are minor flaws compared with the high overall level of the analysis. Peking's attack on India is skillfully related to the Cuban missile crisis and to Chinese concern over other developments of that period, including tension in the Taiwan Strait, in Southeast Asia, and along the Sino-Soviet border.

The account of China's role in Vietnam is briefer and less satisfying, but it is undoubtedly right in concluding that Peking's actual role, which was designed with great care and skill, and still more the fear of what Peking might do if further provoked, exerted a significant restraining influence on the United States, for example by deterring an invasion of North Vietnam.

From a purely substantive point of view, it is unfortunate that Professor Whiting does not deal at length with Peking's role in the Sino-Soviet border crisis of 1969. Presumably he considered that his resignation from the government shortly before would deprive his analysis of the authority that he believes, with considerable justification, is conferred on his treatment of the Sino-Indian border and Vietnamese crises by his official position at those times.

The author's conclusion that Peking's external behavior is essentially rational and nonaggressive (at the overt level, at any rate), if sometimes activist, is correct, as is his warning that when Peking acquires a reliable second-strike nuclear capability its caution may not be so dependable. He perhaps does not see clearly enough that Peking has tended to exaggerate (i.e., both misperceive and misstate) external threats to itself, and that this tendency ought to decrease with the passage of time. In any event, the Chinese response generally begins with counterthreats ("... will not stand idly by," etc.), which may of course be bluff. On the other hand, they may not be, since they have sometimes led to what Peking considered a pre-emptive or forestalling attack, notably in Korea.

Professor Whiting's study is fully documented and is in every way a valuable guide to some important aspects of Peking's external behavior. Its main defect is its style, which is rather cumbersome and hard to read.

HAROLD C. HINTON

The George Washington University

Captured German and Related Records: A National Archives Conference. Edited by Robert Wolfe. (Athens: Ohio University Press, 1975. Pp. 299. $10.00.)

Death and fire are proverbially reputed to be the best forces working on behalf of social scientists threatened with ever-accumulating records of past political events. Mr. Wolfe and his colleagues have ably presented their case of a contrary phenomenon: their efforts made from 1943 to 1968 to preserve and systematize access to the documentation retrieved by the Western allies from the disintegrating Third Reich. It records the losses by war and postwar stupidies or wanton destruction. But the contributions made at this National Archives Conference should elicit professional wonder and appreciation that so much was done under often very difficult military, physical, and political circumstances. Beyond the fascinating human-interest accounts of discovery, restoration, and preservation of records is a basic fact: this book is indispensable for any social scientist working with source materials on the Weimar Republic, Nazi Germany at home, and Nazi-occupied Europe. It contains the most complete English-language guide to the present location and dimensions of all documentary collections on twentieth-century German history to 1945 that are readily accessible to Western researchers. Considerable collections, of course, remain in East Germany and other Russian satellite nations. No comparable compilation of resources there exists, and access for Western scholars can be affected by changes in political wind. For the immediate future, however, this excellent combination of archival history, records of documentation, and professional discussion can keep most of us fully occupied.

HENRY CORD MEYER

University of California, Irvine

Essays on the Study of Urban Politics. Edited by Ken Young. Foreword by Edward C. Banfield. (Hamden, Conn.: Archon Books, 1975. Pp. 208. $15.00.)

The adjective "urban" suffers from overuse. As an illustration, I spend most of my professional life working in an office on the fourth floor of a ten-story building in a gigantic multiversity. Starting from the bottom and proceeding heavenward, there are offered courses – depending on the floor – in the following: "urban anthropology," "urban geography," "urban politics," "urban studies" (a program, not a department), "urban sociology," and "urban history." For all I know, there may be courses labeled "urban psychology," but psychology has its building half a mile away and I am not privy to their undertakings. But I do know that the College of Education has a whole department called "Urban Education."

This massive fragmentation of efforts to study urban phenomena has resulted in duplication, sloppy thinking, and enormous confusion. It is an open secret that this confusion has spread to the ranks of political scientists. In his Preface to this collection of essays by a group of British and American scholars, Edward C. Banfield blames our relative ineffectualness on four general factors. "Firstly, the urban political scientist cannot satisfactorily specify the object he is investigating" (p. viii). "Secondly, the urban political scientist, because he studies a rag-bag of matters, is obliged to employ a rag-bag of concepts and theories" (p. ix). "Thirdly, the urban political scientist cannot settle upon a 'one best way' of doing empirical research" (p. ix). And, finally, ". . . our studies do not build upon another: they do not add up to a body of knowledge" (p. x).

Essays on the Study of Urban Politics was originally conceived of as a special edition of the British journal, *Policy and Politics.* All of the seven essayists are editors or contributors to that important journal, and each is distinguished in his or her own specialty. Since they all agree with Banfield's general verdict, they are united in their endeavor to raise the level of urban political studies, mostly by making use of comparative British and American materials, both old and new. As the editor, Ken Young, puts it, the book has two principal objectives: first, contributors were expected to delineate their separate areas within the field; and, second, "the overall collection was intended to achieve a clearer prospect of the field of urban political inquiry, to demonstrate the relationship between its several aspects, and to point up alternative avenues of exploration" (p. xiv).

Kenneth Newton begins with a review of issues in the American community power studies and their lessons for British research. His finding is that the American studies have little relevance to Britain. Jeffrey Stanyer, noted for his studies of English local authorities, discusses the state of urban electoral analysis, and offers a complex model for the examination of urban elections. Janet Lewis argues for the recognition of a wide range of causally significant and often elusive variables in her chapter, "Variations in Service Provision: Politics at the Lay-Professional Interface."

This is followed, quite logically, by an essay by Bleddyn Davies, senior editor of *Policy and Politics,* who outlines an approach to the variables identified by Lewis through multivariate analysis. Oliver Williams, whose writings on the subject are well known on both sides of the Atlantic, argues for the centrality of the spatial dimension in urban political analysis. Editor Ken Young, drawing heavily upon American studies, explores metropolitan reorganization on a comparative basis. Finally, in a most interesting essay Stephen L. Elkin discusses comparative urban politics and interorganizational behavior. The analysis is imaginative and suggestive of further important research.

How does the editor himself view the final product of this collaboration of experts? He considers the essays to represent a potential step toward the common objective: the development of an urban political theory. In one way or another, he says in his concluding chapter, all of the contributors are working toward this objective by touching on the question of values. Drawing on the research of Clyde and Florence Kluckhohn, Young asserts that the three key concepts within the field of urban political analysis that have a significant value content are urban imagery, localism, and professionalism. Each of these requires systematic exploration by political scientists, and Young suggests how this may be done.

Considered as a whole, how well does the book measure up to the goals delineated by the editor? My own judgment is that the work is a significant contribution to the growing field of urban politics. It takes a long and heady stride forward in achieving a "clearer prospect of the field of urban political inquiry," to use Young's own words. Urbanists are not only collecting their own materials; they are also building systematically on the findings of their colleagues. This book deserves a serious and widespread readership, and it should serve to stimulate and to encourage urbanists wherever they happen to reside.

Whatever directions future research in urban politics may take, the Young collection of essays makes it clear that urban politics has become firmly established as a legitimate subfield of comparative politics. Widespread recognition of this relationship may in itself help urban political scientists to develop hypotheses and theories that urban politics so assuredly needs.

MURRAY S. STEDMAN, JR.

Temple University

Liberalism i Kris. Folkpartiet 1939–1945 (Liberalism in Crisis: The Liberal Party 1939–1945). By Kent Zetterberg. (Stockholm: Liber Förlag, 1975. Pp. 428. Sw. Kr. 46:50.) In Swedish with summary in English.

Since 1966, a team of Swedish scholars has been engaged in a large-scale research project on the subject of Sweden during World War II. The present volume, a doctoral dissertation at Stockholm University, is part of a subproject designed to examine the behavior of the major Swedish political parties during the war years.

On December 13, 1939, in response to a conflict that was steadily drawing nearer to neutral Sweden, four major parties (excluding the Communists) announced the formation of a wartime coalition government. In the newly formed cabinet, the Social Democrats assumed five seats, and the Opposition parties (Conservatives, Farmers, and Liberals) were allotted two seats each. The coalition survived three wartime elections – in 1940, 1942, and 1944 – and was not dissolved until July, 1945, when the circumstances behind its creation no longer obtained.

During the life of the coalition, the fundamental problem for each of the participating parties was how to maximize its contribution to national unity without sacrificing its own distinctive political profile in the process.

This study of the Liberals is divided into two parts. The first deals with the behavior of the party's parliamentary delegation with respect to three wartime issue-areas: national defense, economic policy, and civil rights. The second part proceeds from a more comprehensive perspective, and is devoted to an analysis of how the party's actions related to party strategy.

Although the Liberals agreed on the immediate need for a greatly strengthened defense effort, they were not converted to the traditional conservative policy of a permanently strong defense. They remained instead faithful to their prewar policy of "elasticity" – a cyclical policy of adjusting defense preparedness to the fluctuations of the international environment.

The major economic issues of the war turned on how the burdens of retrenchment, defense expenditures, inflation, and taxes were to be allocated among the different income groups. It is not surprising that the Liberals, with their diverse class constituency, adopted a stance that was situational, pragmatic, and "social liberal" (an acceptance of some government economic intervention, but not *too* much).

The section on civil liberties is one of the most interesting in the book. All the government parties accepted the general principle that if one were forced to choose between the survival of the nation and the maintenance of democratic values, the former would have priority. The real problem was where to draw the line in imposing restrictions on speech, press, or party organizatons. The reaction of the Liberals here was both complex and subtle.

In essence the party implicitly agreed to pursue different policies at different levels of its own organization. Those levels farthest from power (extraparliamentary women's and youth groups) were the most persistent in their pressures against censorship. Although the Liberal members of the government dutifully supported the government restrictions, a majority of Liberals in the Riksdag refused to follow suit.

While the first three parts of this book may be illuminating chiefly for students of Scandinavian affairs, it is the final part that contains the kind of systematic analysis most likely to interest political scientists. The author applies to his study of party activity a general framework of political party norms developed by Gunnar Sjöblom in his Party *Strategies in a Multiparty System,* one of the better theoretical works to come out of Scandinavia in recent years. Thus, in the early years of the war (1939–1942), when external pressures were greatest, parties pursued largely what could be characterized as "unity norms." As the danger receded in 1943, more and more party activity was motivated by "party norms" (maximization of parliamentary influence, votes, and party cohesion).

The study demonstrates amply that the war, with its concomitant quest for national unity, did not entirely eliminate partisan political activity, but instead caused a redefinition of the boundaries of acceptable controversy. To the world outside Sweden, the appearance of an almost monolithic unity prevailed, but to a sharp eye, the differences between parties were still there, although expressed in subtle and indirect forms which became sharper and more explicit as peace approached. For anyone interested in how democratic coalitions behave under conditions of considerable but temporary stress, and in the problem of reconciling democratic values with wartime exigencies, there is much to be learned from the Swedish example.

JOSEPH B. BOARD

Union College

Afghanistan's Foreign Affairs to the Mid-Twentieth Century: Relations with the U.S.S.R., Germany, and Britain. By Ludwig W. Adamec. (Tucson: University of Arizona Press, 1974. Pp. 324. $13.00, cloth; $7.95, paper.)

Political scientists tend to look askance at diplomatic histories because they feel that they often record events in chronological order without any coherent or relevant theory to explain the political processes involved. To a certain extent Professor Adamec's account of the foreign relations of Afghanistan could be subjected to that criticism. But in fact a careful reader will perceive that Adamec does have a hypothesis which he develops and documents even though it remains largely implicit throughout the book.

He elaborates and demonstrates two important principles of international politics: (1) that a small power, geographically located in a strategically sensitive area of rivalry between major powers, is in an unfortunate position and constantly under great pressures. Twist and turn as it may, it cannot altogether escape them and must find ways to preserve its freedom of action and its very independence; (2) that apparently trapped in such a situation, the Afghans developed to a very high degree the techniques of the artful dodger and eventually turned the pressures to their own advantage by developing their country at the expense of the great-power antagonists. All this before the onset of the cold war. By implication, other small countries in the same fix should take note.

Of course, as Adamec explains, Afghan rulers have had much longer experience at playing major powers off against each other than most Third World nations. Nonalignment was almost invented in Afghanistan, at least in its modern form. Afghans even have their own term for it – *bi-tarafi,* a balancing of two sides. As Adamec points out, this was not a cold war invention but a practice Afghan rulers have followed since the eighteenth century in their attempt to preserve Afghan independence from the encroachments of Russian and British imperialistic expansion in Asia.

One of the subtle techniques of *bi-tarafi* pioneered by the Afghans is that of using a third power, preferably one geographically remote and more interested in commercial advantage than political penetration, to soften the asperities of bipolar rivalry and to cushion the neutrality and independence of the small power. In this book, Adamec shows how and why the Afghans chose Germany to play this third-power role. As World War II approached, the Afghan choice of Germany to play this neutralizing role seemed unfortunate because Germany, the presumably "benevolent neutral," attempted to convert its position of commercial partner and source of technical assistance into political leverage to persuade Afghanistan to enter the war on the Axis side. Germany's purpose was to tie down British arms on the Indian northwest frontier, which in conjunction with the Japanese threat to India's northeast border was designed to inhibit effective Indian-British participation in the European war effort.

For a time early in the war it seemed that Germany might succeed in persuading Afghanistan to abandon its traditional neutrality. Lavish promises of territorial gains giving Afghanistan access to the sea were made and, more importantly, the opportunity to settle old scores with Britain over the treatment of the Pushtun tribesmen of the Indian frontier was offered by Germany to the Pushtun ruling elite of Afghanistan. In the end, Adamec tells us, Afghanistan maintained its balance especially after Germany invaded the USSR and was stopped at Stalingrad. Later under pressure from Britain and the USSR Afghanistan reluctantly expelled all nonofficial Axis nationals, fearing that a denial of this demand would precipitate an allied takeover as had happened slightly earlier in Persia.

Adamec's account stops shortly after World War II so that he does not cover the success of Afghan diplomacy in enlisting the United States, first as a successor to Germany as the distant third power and later, using cold war leverage, as a successor to Britain to counterbalance the USSR. But Adamec has laid a well-documented foundation for the fascinating story of how Afghanistan turned its geopolitical disadvantage into an asset by stimulating economic competition within Afghanistan between the cold war rivals.

Viewed correctly then, as a case history of the role and problems of a strategically placed neutral small nation threading its way through a bipolar environment, this book is worth the full attention of political scientists interested in international politics.

LEON B. POULLADA

Northern Arizona University

Oil, Politics and Seapower: The Indian Ocean Vortex. By Ian W. A. C. Adie. (New York: Crane, Russak & Co., 1975. Pp. 98. $5.45, cloth; $2.95, paper.)

Adie's title is confusing and misleading; the monograph should have been called "Indian Ocean Megapolitics." Too ambitious for its size, the study nonetheless informs and prescribes in a fashion familiar to strategists. Adie's point of view is lucid and will seem rational to neo-Hobbesians: sovereignty requires arms, for the implacable forces of other sovereigns invariably devour the weak. So, although no clear and present threat exists for Australia (his host during this research), Mr. Adie urges an arms buildup by Australia in alliance with the United States because long range threats are certain – from the Soviet Union, from China, from wherever!

Why misleading? While oil is the catalyst for discussion, most of the book deals with the politics of economic change among the powers involved in the Indian Ocean theater. Although seapower is a topic of comment, the author properly views naval capability as a tool, not an intent. Indeed, China has no naval presence in the area, yet its policies receive a dozen pages of description throughout the volume.

This raises the question of the book's organization. I found the material confusing because the seven chapters each include comment on Soviet, U.S., and Chinese policies: whether the focus be "Arabia, the Persian Gulf, and the Subcontinent," "Franconesia and the African States," or "Australia and Japan." The reason is understandable, for Great Power policies, by definition, are not regionally confined; yet the book purports to be organized along regional lines.

The author is knowledgeable about details of the Indian Ocean area. From the French base at Crozet, to the current condition (1974) of Diego Garcia, and on to Pine Gap in central Australia, the reader is escorted through the fantasies of strategists in planning rooms of Canberra, Delhi, Moscow, Peking, Tehran, Tokyo, and Washington. The mystery of it all deepens as one reflects that while merely two decades ago the area was a British domain, now the Union Jack's presence receives a scant paragraph from one of her ex-civil servants. Not since the Napoleonic Wars has the Indian Ocean scene changed so rapidly. That change is judiciously comprehended by Adie, who describes the varied momentum of the several powers as they extend their naval capabilities – and rhetoric.

Accurate foresight usually comes at the cost of scientific evidence in the "futures game." In this study one finds the argument persuasive, if uneven, and it is clear that the author knows more than he has written. Is he lazy, was he constrained by the editorial policy of the National Strategy Information Center (funding agent for the publisher), or is he simply so accustomed to the ideological mind-set of the Foreign Office that he ignores the need for deeper political analysis required by a critical scholarly audience? While Adie has insight into the strategic issues that drive small, medium, and great powers into an Indian Ocean imbroglio, he establishes little linkage between foreign policy and domestic politics. A weakness in the monograph is this lack of connective tissue. Example: Japan has a vital interest in open sea lines, especially through the Malacca Straits, but Adie never tells us just what domestic forces keep the Conservative government from following the U.S.'s, China's, or the U.S.S.R.'s route to alliances or deep political ties. Likewise, the ungainly steps by the Soviet Union are unfavorably compared to the astute "two-track" policy of Maoists, but no effort is spent analyzing why the Soviets grope so.

This is No. 24 in a series on strategic problems funded by the National Strategy Information Center, a rather conservative group. Change in general is viewed skeptically by the authors they sponsor, and planned change by Communist or Socialist powers is particularly suspect. Conversely, U.S. defense postures are usually found wanting. With Vietnam behind them it is understandable why the Indian Ocean should loom so large in their vision of threat to Free World security. Adie assists in portraying that vision, the fourth such study on the Indian ocean sponsored by the Center. I believe that Adie comes nearer the mark than his three predecessors in helping the uninformed grasp the essentials of megapolitics in this enormous area, the Indian Ocean.

JOHN BADGLEY

Institute of the Rockies

West German Reparations to Israel. By Nicholas Balabkins. (New Brunswick, N.J.: Rutgers University Press, 1971. Pp. 384. $12.50.)

It is an old lament in academia that a good many intellectual efforts could have done better as tightly condensed articles than as full-blown tomes. Professor Nicholas Balabkins's *West German Reparations to Israel* suffers almost from the opposite problem. Most of its chapters could easily stand on their own as article-length monographs, yet as result, they may not quite add up to a book. Lucidly written and exhaustively researched, the book represents a veritable grab-bag of information and analysis on such wide-ranging topics as the economics of the transfer problem, economic history of Israel and West Germany, international law and ethics, and the politics of diplomatic bargaining.

The heterogeneity of the narrative stems from its ambitious scope. Its centerpiece is the Luxemburg Agreement, concluded between Israel and the Federal Republic in 1952. Under its terms, the Federal Republic bound itself to pay close to 3.5 billion deutschmarks (then $840 million) in money and kind as collective reparation to the Jewish people as represented by the fledgling state of Israel. The author set out to address two basic questions: How did this agreement come about, and what effects, both political and economic, did it have on either side and their evolving relationship? In his attempt to answer these questions, the author has spared neither effort nor disciplinary boundaries, traversing the realms of economics, history, and international law with scholarly thoroughness and analytical skill.

Thus the Reparations Agreement itself turns almost into a secondary concern, serving as take-off point for excursions into various related fields of inquiry. In legal terms, for instance, the Luxemburg Treaty was strictly *res nova:* one would be hard-pressed to find a precedent in international law for reparations paid *voluntarily* by one political community to another. Moreover, since they were both born in the aftermath of the Holocaust, Israel and the FRG were not even, strictly speaking, ex-belligerents. There was no basis in international law for a claim by one state (Israel) on behalf of individuals (the murdered and dispossessed Jews of Europe) who had not been members of its own citizenry. Accordingly, these legal conundrums prompted the author to look backward in history for antecedents and analogies. These are surveyed in "Germany's Road into Industrial Genocide" (chap. 1) and "Reparations in Perspective: The Victor-Vanquished Relationship in Interstate Wars" (chap. 2).

Ultimately, West Germany's debt and Israel's claim could only be based on some idea of international morality, historically the most unreliable guide to political intercourse among nations. It was one of Adenauer's (West Germany's Chancellor from 1949 to 1963) greatest political achievements that he not only recognized West Germany's moral debt (as East Germany did not) but also goaded his country into concrete financial amends. Given the dismal state of the West German economy after 1945, restitution to Israel initially looked like a serious drain on the FRG's meager resources and hence not exactly like a profitable domestic strategy. Yet Adenauer's task was soon lightened by the fortunate confluence between moral imperatives and economic plenty. By 1952, the FRG was well on the way toward the "economic miracle" which quickly dwarfed the magnitude of the "transfer problem." (Both are discussed at length in chapters 2 and 3.) Moreover, considerations of morality and *Realpolitik* nicely reinforced each other at this point. Clearly, as Adenauer understood better than his domestic critics, financial amends to Israel and World Jewry would not only ease

Germany's excruciating moral burden but also contribute to its rehabilitation in the community of nations and expecially by the three Western victor powers which were about to grant sovereignty to the FRG in 1952.

For Israel, on the other hand, morality was at odds with economic necessity. How could Israel possibly accept money from a successor to the Third Reich which had systematically murdered six million Jews and by this acceptance appear to forgive something which could not be forgiven? Balabkins juxtaposes an account of the painful domestic debate (chap. 4) with a skillful rendering of Israel's miserable economic condition in the early years of its existence (chap. 5). In the end, Israel agreed to face-to-face negotiations with Germany at the Wassenaar Conference (chap. 6). Its outcome was the Luxemburg Treaty whose financial and technological benefits were instrumental in launching Israel's economy onto a path of sustained economic growth (as analyzed in chapters 8, 10 and 11).

As an economist, Balabkins has devoted particular attention to the economics of the West German-Israeli relationship. His chapters on the Israeli and West German economies offer instructive case studies in economic history and development which might make profitable reading even for those who are primarily interested in the politics of the relationship. Indeed, concentrating as heavily as he does on economics, Balabkins has gone a long way in heeding the often-heard exhortation to integrate economic and domestic factors in the study of foreign policy. Balabkin's book is an analytical narrative but not a theoretical study. Given its diverse foci, it may strike many as a very heterogeneous contribution, and as such, it may not satisfy the "specialists" of either economics, political science or history. It is a veritable one-man interdisciplinary show and, depending on the biases of the reader, it might demonstrate either the perils or the promises of interdisciplinary approaches to the problems of the social sciences.

JOSEF JOFFE

Harvard University

Agreement on Berlin: A Study of the 1970–72 Quadripartite Negotiations. By Dennis L. Bark. (Washington, D.C.: American Enterprise Institute for Public Policy Research, and Stanford: Hoover Institution on War, Revolution and Peace, 1974. Pp. 131. $3.00, paper.)

This short monograph covers the historical background, the preliminary jousting, the issues at stake in the negotiations, and the final outcome of the bargaining over the status and access rights to West Berlin. It is a useful overview for the educated layman and undergraduate student, but contains little grist for the specialist.

It is necessarily cast within the framework of the other negotiations associated with the broad context of the *Ostpolitik,* or the mutual efforts to reach a *modus vivendi* between the Federal Republic of Germany and its East European neighbors. One of the central themes presented is that both the Soviet and East German positions on the status of access rights to West Berlin have varied significantly. The Western Allies abrogated Article 23 and 144(2) of the West German Basic Law (constitution) at the time of the creation of the FRG, which denied West Berlin equal status with the other *Laender;* it was to retain a special status unique under German law and allied authority. Bark, Research Fellow, Hoover Institution, recalls that in his 14 January 1970 State of the Nation Speech, Chancellor Brandt restated the FRG position: *Zugang,* unrestricted access to and from West Berlin; *Zuordnung,* preservation and strengthening of political and economic and cultural ties between West Berlin and the FRG; and *Zutritt,* entry by West Berliners to East Berlin and East Germany.

Five days later Walter Ulbricht held an unusual press conference to reinforce the DDR's stance against both the West and the Soviets. He stated that West Berlin was an independent political entity within East Germany and that "the capital city of the German Democratic Republic, Berlin . . . is not subject to four-power control." The author rightly points out that the final Berlin Protocol, which traded Soviet presence in the Western sectors and assured ties with Bonn for formal Soviet acceptance of the ultimate responsibility for Western access, severely undermined the DDR claims.

The monograph ends in 1972 and was published two years later, a serious gap when events moved so fast. One wishes the author had addressed a number of substantive questions anticipated during the negotiations or that have emerged since. For example, what are the legal and political implications of the Soviets' legal acceptance of access responsibilities and their delegation of the right to the DDR to determine when abuses of Western access rights occur? Further, how much have the negotiations reduced the burden of partition for the West Berliners and the West Germans?

The author's style and methodology mar the overall product. The monograph is wordy and redundant and could have been cut by one-third. Roughly 75 per cent of the references are West German newspapers, half of which are citations from the right-wing *Die Welt.* This led to several instances of recitation of press speculation. The virtual omission of the vast primary and secondary literature in German, part of which is available in English, weakens the depth of research and analysis.

LAWRENCE L. WHETTEN

University of Southern California

The Handling of International Disputes by Means of Inquiry. By Nissim Bar-Yaacov. (New York: Oxford University Press, 1974. Pp. 370. $24.00.)

The study of conflict has recently emerged as a major area in the field of international politics. Although contemporary research has focused primarily on the nature, incidence, and causes of international conflict – rather than on conflict management and resolution – the ways in which conflicts are resolved have also attracted the attention of scholars in the behavioral sciences, including political scientists. In his book, published under the auspices of the Royal Institute of International Affairs, Dr. Bar-Yaacov, Associate Professor of International Relations at the Hebrew University of Jerusalem, brings his analytical skills to bear, rigorously and comprehensively, upon inquiry as a means of settling international disputes. The institution of international commissions of inquiry, established by the Convention for the Pacific Settlement of International Disputes of 1899 and reaffirmed by the Convention of 1907, was designed to "facilitate a solution to these disputes by elucidating the facts by means of an impartial and conscientious investigation" (Article IX of the Convention, 1907). In subsequent evolution, however, the term "inquiry" was often used in various other meanings and – as shown by Bar-Yaacov – the method itself assumed many different forms, thus losing its original identity as envisaged by the Hague Convention. The purpose of Bar-Yaacov's study is to clarify the notion of inquiry and to find out whether and to what extent inquiry remains an independent method of settling international disputes.

In the Introduction the author analyzes the different categories of inquiry employed in international practice and various theoretical conceptions of inquiry which culminated in the two Reports of the Secretary General of the United Nations. Bar-Yaacov perceives two basic dichotomies in the area of inquiry: first, inquiry confined exclusively to the ascertainment of facts versus inquiry intended to clarify all aspects of the dispute, whether factual, legal, or political; and second, inquiry under bilateral arrangement as opposed to inquiry under the auspices of international organization. These major themes are explored thoroughly in all their variations in the historical exposition of inquiry included in Chapters 2 through 8.

Chapter 2 covers the political and legal issues as they emerged from various texts and statements at the Hague Convention of 1899. The famous Dogger Bank incident, which was the first test of the Hague institution, is examined in detail in Chapter 3. The author proves that despite certain departures from the Hague model, the commission acted as a genuine inquiry body and by preventing escalation contributed to international peace. After reviewing the institution of inquiry under the Hague Convention of 1907, the author follows up the main theme of how inquiry was used in international practice after 1907. The discussion is divided into four parts: inquiry under treaties, disputes handled by commissions of inquiry, inquiry in the work of commissions of conciliation, and inquiry in the framework of the UN. Chapter 5 deals with various treaty arrangements such as, for example, the treaty between the United States and Canada, 1909, the Bryan treaties, 1913–1940; and the UN Charter. The functions of the commissions set up by treaties are analyzed not on the basis of the formal name or title of the commissions, but according to their actual terms of reference. The examination revealed "a definite tendency to submerge inquiry into the facts in favor of the enlarged inquiry, and subsequently to submerge the enlarged inquiry in the process of conciliation" (p. 138). The disputes covered in Chapter 6 include the cases of the *Tavignano,* the *Tiger,* the *Tubantia,* and the *Red Crusader,* all dealing with incidents at sea. The author argues that despite limited resort to genuine inquiry, the system proved its value in disputes of moderate character, involving claims over what actually happened, especially incidents at sea. The author analyzes the present conception of inquiry in the process of conciliation, based on the work of one "commission of inquiry and conciliation" (concerning the Chaco dispute of 1929) and three "commissions of conciliation." These case studies, together with his examination of the doctrine, lead the author to conclude that conciliation is a process that has two stages (1) the examination of facts and (2) conciliation proper. Bar-Yaacov emphasizes, however, that a conciliation commission may decide not to elucidate facts likely to be prejudicial to the conciliatory effort and, in general, is guided by the principle that its inquiry must serve the purpose of conciliation.

The longest chapter is devoted to the study of inquiry in the framework of the United Nations. In general, Bar-Yaacov concludes that the concept of "inquiry" in the UN practice has been politicized and emptied of its original meaning, including, as it does, any routine preventive reporting system. Nevertheless, he is cautiously optimistic, believing that inquiry can still perform a useful function "in appropriate disputes and in favourable conditions of international cooperation and trust" (p. 329).

Bar-Yaacov's book is certainly not an interdisciplinary behavioral study in the area of conflict resolution. Quantitative precision, possibly in the form of tables summarizing various dimensions of inquiry, would add to the value of an otherwise rigorously conducted, solid piece of traditional research. Ample documentation, a number of maps and ocean charts as well as a general index and a treaty index make the book useful as a reference work. Several key Hague and UN documents are included in the Appendices. Substantively, the book is the best existing study of the Hague system of international inquiry, and as such it must be recognized as a major contribution to the growing literature on conflict resolution, of equal interest to students of international law and political scientists.

BOLESLAW A. BOCZEK

Kent State University

The Horn of Africa: Strategic Magnet in the Seventies. By J. Bowyer Bell, Jr. (New York: Crane, Russak and Co., 1974. Pp. vi, 55. $4.95, cloth; $2.45, paper.)

The recent appearance of Mordechai Abir's study of *Oil, Power and Politics: Conflict in Arabia, the Red Sea and the Gulf* (London: Frank Cass, 1974) has served to emphasize the need for a full-length study of the strategic significance of the Horn of Africa. For Dr. Bell, "The basic strategic importance of the Horn . . . is simple geography. The Horn commands the Red Sea and the northwestern Indian Ocean littoral" (pp. 8–9). He defines the Horn as comprising Ethiopia, the Somali Republic and the French Territory of the Afars and Issas (Djibouti), adding thereto somewhat arbitrarily the Sudan and, as arbitrarily, excluding Kenya. Events following the Six-Day War in 1967 have progressively enhanced the importance of this region. The closure of the Suez Canal and rupture of diplomatic relations between the United States and Egypt, Libya, and the Sudan together with the withdrawal of the British from Aden opened wide the door to expanded Soviet adventures in Somalia, Aden and the Indian Ocean. With Soviet support Syria stepped up her interventions in Eritrea on the western shores of the Red Sea at a time when the United States had decided upon a policy of withdrawal from that same area. At this writing, present wide-scale military operations in Eritrea and Ethiopian-Somali rivalry over the Djibouti territory soon to become independent have made the Horn a focus of international concern.

Much of the analysis in the book is concerned with internal economic, religious, and political instabilities and conflicts within the Horn rather than strategic considerations. The author nevertheless concludes that the area will remain essentially in equilibrium (p. 48); nor does he foresee a clash of rival protege regimes (p. 8). He takes the view that "the great ideas, particularly of revolution, out of the Western world have . . . often been fashioned to adorn old causes or warped to buttress ancient ambitions" (p. 34). While these assessments may be questioned in the light of recent events, there is the prediction that "In the fullness of time, a pro-Chinese Ethiopia might well face a pro-Russian Somalia, but the same old local political issues would be the reason for the alignment" (p. 8). The single greatest source of regional instability and confrontation, Somalia's designs for a Greater Somaliland, is viewed solely as a struggle for self-determination. Ignored is the far more critical and insoluble problem of the dependency of the nomadic Somalis on the grazing lands of Ethiopia.

More rewarding are the two chapters devoted to the roles of the United States, the Soviet Union, China, Israel, and France. The inveterate and bitter hostility of the Arab world, however, toward Ethiopia and in particular of Libya, Syria, and Somalia (now a member of the Arab League) is not brought into focus. Libya's campaign to remove the OAU Headquarters from Addis Ababa, Syria's leadership of the Eritrean revolt and power moves in the Red Sea itself have been ignored.

Inaccuracies flaw the work such as the assertion that the Somalis are partly of Mediterranean origin and their language unrelated to their neighbors (pp. 3–4), that the Gallas moved into the Horn 400 years ago (p. 4) and that Nimeiri and Qaddaffi came to power in 1967 (p. 45).

JOHN H. SPENCER

Tufts University

The Last Secret. By Nicholas Bethell. (New York: Basic Books, 1974. Pp. xiv, 224. $8.95.)

In this dramatic exposé of what has hitherto been one of the best-kept secrets of World War II, Lord Nicholas Bethell turns a spotlight on the forcible repatriation of some two million Russians and Ukrainians who found themselves in English and American hands in Europe between 1944 and 1947. The operations involved not only former Soviet citizens but also large numbers of men, women, and children who had never been citizens of the U.S.S.R.

Lord Bethell, a translator of Solzhenitzyn and the author of *The War Hitler Won,* examines the considerations that led the Western Allies to display such unwarranted zeal to please the Kremlin. His treatment is calm, dispassionate. His material is based on official archives and on the testimony of many eyewitnesses. Hugh Trevor-Roper, in his introduction, characterizes the work as one that calls not for judgment but rather for reflection.

The author tells us that, as a result of a secret protocol signed by the three powers at Yalta and another agreement reached in Leipzig in June 1945, the number of persons turned over to the Russians exceeded two million by the middle of 1945. The Americans specialized in tracking down Russians who had fought for Hitler and remained in Germany, and the British in locating and delivering those who had fought against the Red Army and surrendered in Austria. Combined with the Cossacks and Russians in Austria were some 200,000 Croats, Ustashas and Domobrans and their families, whom the British returned to Tito at the end of May 1945.

The rounding-up process, according to Lord Bethell, was invariably accompanied by a wave of mass hysteria and many suicides. The records show heads of families killing their wives and children and then themselves rather than return. In the end, however, except for a few who had either fled or had been allowed by their British and American guards to slip away, all who surrendered or captured were deported.

Through May 1947 the deliveries continued, the last of them labeled Operations Dachau, Plattling, Keelhaul, and Eastwind. In these instances, again, the deportations were accompanied by the same pattern of desperation, suicides, separation of families, and the frenzy of prisoners reacting to the use of force by U.S. or British military police. By late spring 1947, Stalin had violated every clause in the Yalta Agreement, and the cold war had set in. Very few victims remained to be delivered, the author insists, and the Allies felt under no further obligations to Stalin. The forcible repatriation ground to a halt in June 1947.

One may well ask why this tremendous activity escaped attention in the Western press. Why there was no public outcry? What were the overriding considerations that underlay the Western powers' zeal to carry out a policy that was "abhorrent to their peoples, traditions, and moral values"; They felt, says Lord Bethell by way of summary, bound by the solemn commitment undertaken at Yalta and at Leipzig. Bethell finds some validity in the practical reasons for the deliveries. There was first a need to protect Western prisoners formerly held in German prisoner-of-war camps and now in Soviet hands. Then the Allies could not afford to arouse the suspicion of Stalin for fear of impeding the war efforts. Finally, there was great concern over the logistical difficulty of absorbing and resettling the large number of anti-Communists in the West.

Bethell feels that the steady barrage of pro-Stalin propaganda to which the Western public had been exposed year after year by the British and American press had led people to believe that to oppose Stalin was tantamount to opposing the Allied cause. Who at the end of the war could have believed that a person might be violently anti-Stalin and yet neither pro-Hitler nor opposed to the Western democracies? The public "knew nothing of the internal affairs of the Soviet Union. Why should they?" (p. 208).

In assessing the responsibility of the British and American military men in the whole process, each of the officers in command, according to the author, places the blame on his superiors, and each defends his action on the grounds that he was carrying out orders. Bethell finds an analogy between the ready explanations of all those involved that they were only carrying out orders and the same plea entered by the German generals on trial at Nuremburg. Though obedience is one of the cornerstones of the soldier's craft, the judges at Nuremburg refused outright to acquit those German generals whose defense for committing cruel acts was that they were only obeying their superiors. The accused were convicted and many were sentenced to death.

Bethell argues that though the prisoners had no claim on Allied loyalty, they were nevertheless human beings. He raises the question of whether we may have arrived at the point of accepting a new standard: that a prisoner of war – whether civilian or military, innocent or guilty – once having been captured or having surrendered, is at the mercy of his captor.

MILOS MARTIC

University of California, Berkeley

Cyprus: The Vulnerable Republic. By Dimitri S. Bitsios. (Thessaloniki, Greece: Institute for Balkan Studies, 1975. Pp. 223. No price indicated.)

Published originally in Greek in 1973 under the title *Crucial Hours,* this translation is the latest addition to the growing literature on the Cyprus question. I read this book with great anticipation because the author, currently the Greek Foreign Minister, has been a career diplomat actively involved in the problem of Cyprus since the early 1950s. Mr. Bitsios's book concentrates on the period 1951 to 1965, with the addition of a brief postscript consisting of his October 31, 1974, address to the U.N. General Assembly on the Turkish invasion of Cyprus.

Despite the author's qualifications and knowledge of the problem, this book unfortunately fails to live up to the claims of its publisher and the expectations of the reader. In contrast, other current works on the subject, such as those by C. Christides, (*Kypriako Kai Hellinotourkika* [Athens: n.p., 1967]), P. Terlexis, (*Diplomatia Kai Politike Tou Kypriakou*[Athens: Rappas, 1971]), and S. G. Xydis, (*Cyprus–Reluctant Republic* [The Hague: Mouton, 1973]), and (*Cyprus: Conflict and Conciliation, 1954–1958* [Columbus: The Ohio State University Press, 1967]) to name a few, provide far better insights into the problem of Cyprus even though these authors never held posts comparable to those of Mr. Bitsios. Certainly, the author brings out in passing some little-known aspects of the diplomatic background of the Cyprus question. For example, the British High Commissioner in Cyprus seems to have encouraged Makarios to formally present his 1963 constitutional revision proposals, which became the catalyst of the postindependence phase of the dispute. Similarly, at the London Conference (January 15, 1964), Duncan Sandys presented a British scheme for the geographical separation of the two Cypriot communities. Finally Mr. Bitsios indirectly mentions what is already known, i.e., that the Greek-Greek Cypriot relationship was frequently tense even in the early stages of the anticolonial struggle. Unfortunately, the reader is left with the task of evaluating the significance of these facts, especially in the context of the developments in and around Cyprus of the last few years.

What disturbs and puzzles me the most, though, is Mr. Bitsios's superficial examination of the Cyprus question. Is the Foreign Minister concerned about embarrassing his associates, who today, as well as during the 1950s, handle the Cyprus problem? Are his ideological convictions such that he finds it necessary to downplay the role of the United States and NATO in this dispute? Note, for example, his implied criticism of the Greek diplomatic tactics during the colonial phase of Cyprus. Yet, instead of presenting alternatives, or showing where and why Greek diplomacy went wrong, he gives the reader sympathetic accounts of Prime Minister Karamanlis and Foreign Minister Averoff. In discussing the crucial 1955 London Tripartite Conference the author tells the reader that his purpose is not to enter in the details of the conference. Why not? Wasn't, after all, this conference one of the most important turning points in the Cyprus question?

The reader is also told that John Foster Dulles sent Greece a message of sympathy in the aftermath of the 1955 riots against the Greeks living in Turkey. Somehow the author has forgotten the content of the identical note sent to both Greece and Turkey by Dulles in the aftermath of this tragic event. Moreover, having defined the concern of the United States in the Cyprus question on strategic grounds, Mr. Bitsios provides no discussion of the aims and methods of American overt and covert diplomacy during the critical 1964–1974 period. It was during this period that the United States actively pursued solutions, through direct mediation or secret Greco-Turkish talks, which proposed to end Cypriot independence through the partition of the island between Greece and Turkey.

When the Greek edition of this book first appeared, Greece was under military rule and censorship. This may explain the author's decision to end his discussion of the Cyprus problem with the events of 1965. There was no reason, however, for the failure of the English edition of this book, published well after the restoration of democratic rule in Greece, to deal with the 1967–1974 period unless Mr. Bitsios wanted to avoid embarrassing Greece and the United States because of their role in the dispute. With the expertise born of his active service in the Foreign Office (through 1972), Mr. Bitsios could have shed some light on this dark and crucial phase of the dispute. He chose not to do so, and therefore this book – most noteworthy in what it does *not* say about the diplomacy of the Cyprus question — is a disappointment.

VAN COUFOUDAKIS

Indiana University at Fort Wayne

In Search of a Responsible World Society: The Social Teachings of the World Council of Churches. By Paul Bock. (Philadelphia: The Westminster Press, 1974. Pp. 251. $10.00.)

The study of international organization often appears to be little more than that of the United Nations, its specialized agencies, and a few European organizations. It is as if the scholarly, functionalist dictum of an initial, low-key involvement has succeeded so well that the very existence of the hundreds of other international organizations (governmental and nongovernmental) is virtually an arcanum to all but a few devotees. The costs for the development of general theory are obvious. Paul Bock's analysis of the social teachings of the World Council of Churches (WCC) offers a small palliative for this scholarly ailment, and, incidentally, an updating of an earlier study by Edward Duff, *The Social Thought of the World Council of Churches* (New York: Association Press, 1956).

The general object of Professor Bock's attention, the WCC, is a "supranational Christian body" (p. 151) consisting of some 263 Protestant, Anglican, Old Catholic, and Orthodox churches in ninety different countries and territories. The Organization includes, therefore, representatives from the Communist and Third World states, as well as the West. Founded in 1948 (although its immediate antecedents hark back to the Stockholm Conference of 1925 and earlier) and headquartered in Geneva, Switzerland, the WCC has proved to be an important NGO actor in international and transnational politics. The present study offers some idea of why this is so.

Although Bock's numerous and lengthy citations from various ecumenical pronouncements become at times burdensome to his reader, they do illustrate well the WCC's concept of a "responsible" international society: The synthesis of Western values emphasizing freedom with those of much of the remainder of mankind stressing economic and social justice and security. Individual chapters are devoted first to a survey of ecumenical social thought in general and then to specific analyses of the Council's statements concerning different political and economic orders, war and peace, communism, racism, and economic and social development. One theme stressed persistently is that Christianity should neither avoid social involvement nor become identified exclusively with any particular system or bloc.

The analysis is descriptive and chronological. Perhaps because of the overlap in the subjects covered, there are numerous tedious repetitions throughout the text. The reader is told several times, for example, that SODEPAX is an agency through which the WCC and the Vatican leaders cooperate on international social, development, and political matters, and that Tambaram, the site of an ecumenical meeting in 1938, is near Madras, India. One might also note here that the United States exploded its first hydrogen bomb in 1952, not 1950 (p. 102).

Most of the chapters end with a succinct summary followed by a comparison between Vatican and WCC thought on the issues in question. The study contains a short bibliography and is indexed.

If Kant's definition of justice as that behavior which treats human beings as ends in themselves be accepted, one should come away from a reading of Bock with an admiration for the social teachings of the WCC. This is a work that can be useful for students of religion and political theory, as well as international organization.

MICHAEL M. GUNTER

Tennessee Technological University

Decisions in Israel's Foreign Policy. By Michael Brecher. (New Haven, Conn.: Yale University Press, 1975. Pp. xiv, 639. $25.00.)

This sequel to *The Foreign Policy of Israel* is more ambitious and less successful than the earlier volume. While the first was an exhaustive though flawed clarification of a significant actor in world politics, here the same diligence is applied to several of its major decisions for much less gain to the study of foreign policy than is claimed.

General readers will this time find the going much harder, although there are rewards in the form of thoroughly documented accounts of Israeli decisions on Jerusalem, relations with Germany and China, Jordan waters, and three instances of war in the Sinai. Those who have kept up with the literature will find these accounts familiar, though more meticulously and massively presented than in other places. Its deliberately esoteric side apart, the book's encyclopedic quality is often vitiated by excess. When well-known propositions are established anew by reference to still more evidence, the line between scholarship and pedantry fades.

Political scientists without serious background in Middle East study are likely to be impressed with this volume, for Brecher mirrors particularly well a trend that both excites and troubles the present state of the discipline. Another was more explicit in the precursor.

One is a fascination with tools of inquiry productive mainly of jargon, besetting much contemporary social science, against which Lewis Coser recently warned sociologists meeting in San Francisco. "He spoke of 'embarrassing triviality' and 'enormous ballyhoo' for results 'mostly dross or interminable methodological disquisitions and polemics,'" (*NYT*, 8/30/75, p. 17). All of these are evident in this volume, much of which is given to announcements of the grand designs accomplished therein, the building of a novel general theory of foreign policy behavior.

Claiming that intellectual inquiry has heretofore been either untheoretical or unrelated to empirical data, the author proposes a synthesis in a "structured empiricism." What follows looks rather like an unrestrained hoarding of data presuming to be scientific. While no datum, however trivial, seems omitted, the "structuring" of the mass is mainly by codification and repeated reprocessing according to an elaborate research design. The codification accomplishes the opposite of simplification and economy. Thus: "The universal actor in the *global* system (G) was omnipresent. UN Secretary-General Hammarskjöld [repeatedly] visited the area of conflict. . ." (p. 226). When not just piling the esoteric atop the commonsensical, the author directly translates ordinary, if not trivial, propositions into pretentious language. Thus the notion that Israel had to face relations with China once it had to face relations with China: "The initial stimulus to Israel's first major decision on China was the proclamation of the PRC on 1 October 1949 and its request for recognition from all states" (p. 127). Much of the narrative given initially in each case is further reprocessed under different headings, with many tables still further repeating already articulated propositions in graphic or statistical form.

What the author really produces is a rich but not well-written encyclopedic treatise that is quite useful as such. Anyone interested in, say, who of the top Israeli leaders was most concerned with the Soviet factor in a particular decision, or a similar matter, will find satisfaction in this volume. But I doubt that anyone at all familiar with the literature would really appreciate much material, such as content analyses and tables trying to establish the not very startling proposition that Egypt was uppermost in the minds of Israelis when that most weighty of Arab nations was most directly involved in a crisis with Israel, along with other equally unsurprising discoveries.

Students of comparative foreign policy will, however, appreciate Brecher's attempt at testing a number of hypotheses current in their literature in the light of his seven cases. This more genuine contribution to theory building elicits two observations. One concerns the nature of the hypotheses, for several seem banal or simply tautological. The other concerns the several strategic and tactical decisions used for testing, which are not necessarily produced by the author's elaborate scheme, but rather are commonsensical derivations from thoroughly familiar events.

Brecher's work also mirrors the tendency in the discipline to presume global validity for parochial, Western conceptualization. Students of non-Western civilizations are invariably bemused by the inclination of political scientists trained in international relations to explicate phenomena in societies about which they know next to nothing in terms that would make sense only in their own. They thus identify "moderates" or "rightists" in wholly inappropriate circumstances or take for granted rationality as they understand it, where neither motivation, values, or basic mentality is essentially the same, or even remotely similar. Attitudes of leaders in violent, pre-industrial, rigidly-sectarian societies are almost invariably interpreted as if they were consensus politicians in Western countries. Just as Alexander Woodside has recently marveled at the massive treatment of Vietnamese affairs by analysts who disdained a knowledge of Vietnamese history, so one discovers also that a lack of serious study of the Middle East characterizes the authors of what prestigious foreign policy journals usually publish on that area. Being informed, in this genre, means accepting the current journalistic and diplomatic lore, often just cliche conceptions of reality.

If Israeli foreign relations could be understood by studying only Israelis, Brecher's work could be faulted mainly for misplaced methodological zeal, for he has been exceedingly diligent in researching *that* side. But one cannot really study the Arab omnipresence in such a problem by researching Israelis. Accepting the guidance of Israeli diplomats (as Brecher indicated in an article in *The New Middle East*) is not an adequate substitute for serious study of the Arab factor. That he and other scholars should follow diplomats rather than appropriate area scholars, is in the case of the Arab-Israeli conflict further motivated by the preference they share with diplomats for rationality, bargaining, and compromise. International affairs generalists and diplomats persist in viewing that conflict as indeed amenable to such means, if only the elusive formula were discovered, while the consensus of Orientalists is hardly favorable. Their inclination to exaggerate greatly the variability in attitudes toward the Jewish state in the Arab world is mirrored in Brecher's mocking reference to "the Arabs" throughout. This is curious behavior, especially where particular insistence is voiced on the importance of sober perceptions in international relations.

GIL CARL ALROY

Hunter College,
City University of New York

La Coexistence Pacifique. By Philippe Bretton and Jean-Pierre Chaudet. (Paris: Armand Colin, 1971. Pp. 328. No price indicated.)

The authors, both French jurists, have described the history of diplomatic relations between the Soviet Union and the United States with exactitude, constantly striving to give, in a small volume, the greatest possible amount of information (even, for example, references to internal jurisprudences).

They do not employ any of the fashionable concepts adopted today by the science of international relations, notably American, but abide by a very traditional, very "French," idea of the study of international phenomena which they describe in the most unbiased manner possible. Moreover, the book's bibliography is almost exclusively French; English literature is completely ignored, with, for example, only one book in English cited on the conflict in Vietnam.

This very factual presentation has, however, a positive aspect: the events described in plain terms are neither falsified by theoretical discussions, nor obscured by a complicated and tedious vocabulary. This attitude also has the advantage of not tempting Professor Bretton and Professor Chaudet to speculate on the future. By purely and simply describing the events (often recent), they act as historians and history goes out of fashion less quickly than international political science.

Nevertheless, their book does not avoid the inconveniences inherent in all studies concerning constantly developing news: it is no sooner written than it is out of date. Think what has transpired since the publication of this book in 1971. The signature of the S.A.L.T. agreements, the C.S.C.E. and the M.B.F.R., the Sino-American reconciliation, the Yom Kippur War, and, of course, the petroleum crisis have greatly contributed to the increase in the ambiguity of the meaning of coexistence, to which little consideration have been given recently.

In general, the work lacks internal cohesion since the events described are juxtaposed without any overall interpretation. This important weakness stems from the limited attempt to explore in depth the theory of the notion of peaceful coexistence (pp. 25–44) and, therefore, that of the cold war. Although no precise definition is given, the authors appear to consider that peaceful coexistence refers to the evident or latent desire of the Soviet Union and of the United States not to come to armed conflict.

Such a view appears to be very limited. On the one hand, it implies a lack of knowledge of the field of application of this notion. Adopted by popular consensus by the nonaligned countries, coexistence not only refers to the two superpowers but it actually represents the present goal of international relations in general. On the other hand, at no point do the authors attempt to trace the roots of the doctrine. It is extremely arbitrary to fix its origin as they do, at the beginning of the 1950s; the decree on peace made by Lenin in 1917 could no doubt be considered its first manifestation, and the division of the world into two main sociopolitical systems may be considered concomitant with peaceful coexistence, notably its economic implications (p. 20).

The book is divided into three parts. The first is devoted to the aftermath of the Second World War and, curiously enough, of the cold war. The second deals with the limitation imposed by the major powers on nuclear armament by international society. The third treats the "unarmed" conflict between the United States and the Soviet Union.

The comparison of the first and the third parts leaves the reader perplexed. Any classification of this nature is extremely arbitrary. However, taking into account the pragmatic policy adopted by the authors, a purely chronological presentation and a classification by type of problem or crisis would, no doubt, have been more appropriate, and their goal would have been more easily attained.

In conclusion, the specialist will no doubt find certain topics for reflection but will regret the weakness of certain analyses, notably those concerning Berlin and China. On this latter subject, the authors state that the People's Republic of China "rejects" the idea of peaceful coexistence (p. 34). This virtually unsupported statement is rather surprising: China, at Panch Sila in 1954 and Bandung in 1955, was one of the most ardent defenders of this doctrine and has on many occasions since confirmed its nominal position.

In spite of its weakness and some debatable aspects, the book presents, for the neophyte, a clear and serious introduction to a study of Soviet-American relations and is a quite useful reference book for a specialist. One should not expect anything more than this, and readers who are looking for theoretical explanations, original ideas, or intellectual stimulation will be disappointed.

ALAIN PELLET

Agrégé des Facultés de Droit
(Paris; Constantine)

Diplomats in Crisis: United States — Chinese — Japanese Relations, 1919—1941. Edited by Richard Dean Burns and Edward M. Bennett. (Santa Barbara, Calif.: Clio Press, 1974. Pp. 346. $15.00, cloth; $4.95, paper.)

This is the story of thirteen men — five Americans (Cameron Forbes, Joseph Grew, Stanley Hornbeck, Nelson Johnson, John MacMurray), five Japanese (Hirota Kōki, Matsuoka Yōsuke, Nomura Kichisaborō, Shidehara Kijūrō, Shigemitsu Mamoru), and three Chinese (Chou Fo-hai, Hu Shih, Wellington Koo) — in search of solutions to the mounting crisis in East Asia in the period between the wars. They were generally second-echelon diplomats, not top-level decision makers, although Shidehara and Matsuoka might be viewed as exceptions. "Well-meaning and competent," as Professor Norman Graebner describes them in his useful introduction, dedicated to the national interest as they saw it, they sought peaceful solutions, not war. Since the Sino-Japanese war and Pearl Habor were the end product of the period, their efforts ended in failure. Why the failure and to what extent were they implicated in it? These are two of the major questions raised in the Introduction. Were these men too imperceptive of the forces operating in the world about them? Did they fail to communicate effectively? Were they largely messengers, or did they seek to make an imprint on the policies their governments pursued? Were they, finally, overwhelmed by forces which they were powerless to control?

While the essays do not address themselves directly to all of these questions, some generalizations can be gleaned from the text. A failure to transcend parochial viewpoints and to comprehend the motivation of others is certainly one. Hornbeck's insistence, to the very end, that sanctions against Japan would not produce war because all rational calculation showed she must lose, is a case in point. His misconception, however, was no greater than that of, say, Matsuoka, the self-proclaimed expert on American psychology, who insisted that a tough policy, which included adherence to the Tripartite Pact, was the key to reaching accommodation with the United States.

To what extent should the diplomat be more than a message conveyer? From Tokyo, Grew (like Forbes before him) was increasingly apprehensive about the collision course on which U.S.-Japanese relations appeared to be headed. His warnings and suggested alternatives not only fell on unreceptive ears in Washington, they evoked criticism from Grew's superiors which led him to pursue a more cautious approach. Could — or *should* he have tried to speak out more forcefully? One raises the question in the age of the Pentagon Papers; Grew's course was to look to the historical record for vindication.

On the other hand, the example of Admiral Nomura could be taken as an illustration of an overly personal role. Convinced that neither government truly wanted war, and dedicated to preventing the final rupture, he occasionally allowed his own objectives to temper what he communicated between Washington and Tokyo. While the essay devoted to him does not stress the point, his actions produced a rebuke from his own government and, at times, appear to have furthered misunderstanding rather than to have reduced it.

Compared to the foregoing, Ambassador Hu Shih's experience reads like a success story. Here is the diplomat as "communicator" in another sense. Leaving more traditional embassy matters to others, he spent his time cultivating support for the Chinese cause in American public opinion, which he saw as an important ingredient in American policy. Not all diplomats, however, can enjoy the favorable juxtaposition of circumstances which existed for him — his own personality and background, the relative accessibility of the American public, the pro-Chinese predisposition of American opinion.

Professor Hilary Conroy, in his essay on Nomura, raises a fundamental question. Did these men, regardless of their views, make any difference? Did they in any perceptible way influence the course of events or were they helplessly swept along by them? His answer, rather tentatively put, is yes (as he rightly points out, a contrary answer would make this book a futile undertaking). Had there been a more favorable response at the right time, the Nomura mission might have succeeded. One can disagree with the example and still reject a deterministic approach. But just how did these men interact with circumstances? Herein lies a problem with these essays: the approach is not truly "transnational" (p. viii) but remains largely one-dimensional, and, if as the Introduction would have it, domestic factors were the major determinant of policy, the individuals too frequently appear in a vacuum, remote from the context of events in which they were involved.

Interpretative and critical, the essays are well written and maintain a uniformly high quality unusual in collections of this kind. Brief sketches of the social background and attitudes of the subjects are often highly perceptive. A political scientist interested in studies of elites, in bureaucratic behavior, or even in psychological approaches to politics should find suggestive material here.

WILLARD H. ELSBREE

Ohio University

Kto i kak delaet politiku SShA (Who Makes Policy in the USA and How it is Made). By Sergei B. Chetverikov. (Moscow: Izdatelstvo Mezhdunarodnye Otnosheniia, 1974. Pp. 329. Rubles .98.)

This is a landmark volume in the Soviet study of American politics because it utilizes Watergate to introduce some long overdue corrections into the conventional Soviet interpretation of the American political system.

For many decades Soviet analysts have maintained that as economic power becomes more concentrated in fewer hands in the United States, so does political power. Institutionally, that is reflected in the decline of Congress and the hypertrophy of the presidency. The fatal inevitability with which these processes transpire has been the subject of numerous Soviet studies.

Chetverikov, a specialist on executive branch organization and politics who earlier wrote under the pseudonym Sergei B. Marinin, undertakes to explain how the inevitable was *temporarily* reversed when an increasingly assertive Congress and a series of negative court decisions effected the resignation of a president attempting to expand his powers.

The author's often implied but never explicitly stated conclusion is that power is more diffused in the American political system than the conventional Soviet wisdom allows. Certain legal or formal aspects of American government which Soviet analysts had considered to be unimportant or subterfuges concealing the real concentration of power now must be taken more seriously. The principle of the separation of powers, for example, was used with telling effect against President Nixon by the Congress, and the system of checks and balances really worked. Whereas Soviet writers had largely ignored the role of the independent judiciary, Watergate demonstrated its importance. Even the Soviet tendency to underrate the options made possible by the two party system has to be reconsidered since that system created obstacles to the growth of presidential power.

Chetverikov says, in effect, that the Soviets must treat as really significant the very structures and processes of American government that most American analysts would consider to be its strengths. On the other hand, he identifies what he thinks will be the basis for an eventually much greater concentration of power in the presidency.

Before the increased power can be attained, however, the president must overcome one great obstacle, and the analysis of this process is Chetverikov's unique contribution to the study of the presidency. The president must contend with the "triple alliance," a gradual coalescing or fusing of the congressional staffs, the middle-level executive branch bureaucracy, and the bureaucracies of the large corporations. He responds by trying to impose his control over the federal bureaucracy through increasing the coordinating powers of his executive office, a phenomenon Chetverikov dubs "bureaucratic centralism." Simultaneously, the large corporations attempt to control the executive branch through increasing their representation on the numerous consultative and coordinating committees. Whether this brings big business into conflict with the president, as seems apparent, the author does not say.

Even though Chetverikov does not trace the concrete steps in resolving the conflicts, he does project the president's prevailing in them, and so comes to the conclusion characteristic of Soviet Marxist futurology: power will be concentrated in the hands of the executive.

Chetverikov has given an only very partial answer to the questions posed in his title. But he has also raised some intriguing, challenging, or fanciful (depending upon one's tastes in these matters) possibilities in the study of American politics – or, more particularly, politics of the bureaucracies, by the bureaucracies and for the bureaucracies.

RICHARD M. MILLS

Fordham University

East Asia and U.S. Security. By Ralph N. Clough. (Washington, D.C.: The Brookings Institution, 1975. Pp. 248. $8.95, cloth; $3.50, paper.)

This is an extremely well-written, thoughtful, and informative study that fills a large gap in the literature on the post-Indochina War context for U.S. policy in Asia. The book benefits from Clough's many years in the Foreign Service, his meticulous mind, his careful pen, and his provocative probing into the assumptions that have underlaid U.S. policy for the past two decades. Its contribution at various levels of understanding makes the study suitable for the undergraduate and the senior specialist as well.

The major portion of the book deals with U.S. relations among the Big Three of East Asia: China, the Soviet Union, and Japan. These relations are re-examined in each of their bilateral sets and in specific problem contexts such as Korea and Southeast Asia. The historical background of each interaction is concise, and the future perspective is dispassionate. Clough sees Japan as central to U.S. security interests in East Asia but does not expand this to advocate Japanese rearmanent, much less advise Tokyo to acquire nuclear weapons. On the contrary, he believes that the joint security arrangements should continue with whatever modifications are necessary to sustain Japanese political participation but argues against a qualititative or quantitative change in Japan's military participation because of the effect on Chinese and Southeast Asian perceptions.

During 1975 the Ford administration worked persistently to repair the damage of "Nixon shocks" to Japanese-American relations, with some success. Clough thus overstates the degree to which the alliance is threatened from within. Less visible but no less significant is the more subtle change in Japanese attitudes toward Korea since his writing. My conversations among informed officials and intellectuals in November 1975 contrasted with those of a few years previous in that assurances were now forthcoming of sufficient political support to permit U.S. military use of bases in Japan on behalf of the Republic of Korea should it be attacked from the North. Such opposition as might still occur would probably not be as serious as Clough states.

In view of his extensive China background perhaps the most disappointing discussion concerns Taiwan. Clough asserts:

> Should the United States break relations with Taipei, abrogate the defense commitment, and set Taiwan adrift, the people of Taiwan could be compelled to accept control by Peking.... As the Chinese thrust military forces into the Pacific close to territory disputed between Japan and China, and the United States in the face of military threat abandoned fifteen million people whom it had pledged for over twenty years to help defend, the shift in the power balance could not fail to make a deep impression in Japan ... [and] immediately raise grave questions about the reliability of the U.S. defense commitment to South Korea and increase the risk of conflict there (p. 140).

This "worst case" possibility lacks a detailed, credible scenario for Peking's attainment of Taiwan, collapses the time frame so as to juxtapose a U.S. disengagement with a "military threat," and adds Korea, where – unlike Taiwan – the U.S. maintains a military presence and has fought one war for its interests there. Moreover, while Clough suggests there is little likelihood that the People's Republic will resort to force, he fails to consider how this might be rendered even less likely through bilateral understandings or unilateral moves by the United States.

Despite the book's title, Clough includes Southeast Asia in his focus, on the proper premise that the Indochina War expanded to include the U.S., and to a lesser extent China and the Soviet Union, because of the prior interaction of the larger powers in East Asia. Although this portion of the book is minor, it makes a major contribution in systematically exploring the degree of future Chinese threat, which Clough discounts, and the consequent level of American interest and recommended involvement, which he reduces to nil. Except for a few outdated portions written before the total collapse of the U.S. position in South Vietnam and the subsequent emergence of Hanoi's disputes with Peking over influence in Cambodia and islands in the South China Sea, these two chapters make salutary and trenchant reading.

Any effort to summarize the political situation in Asia during so dynamic a period as 1973–74 runs the risk of rapid obsolescence. The only serious instance of this phenomenon concerns China's sudden emergence as a producer and exporter of oil, particularly to Japan. This development weakens Clough's negative assessment of Peking's prospects for earning foreign exchange as well as for strengthening its relations with Tokyo at the expense of Moscow. In all other respects, however, the book is likely to stand the test of time extremely well, at least until Washington completes the normalization of relations with Peking and thereby terminates its political-military relations with Taipei.

ALLEN S. WHITING

University of Michigan

The United States and Latin America: An Historical Analysis of Inter-American Relations. By Gordon Connell-Smith. (New York: Halsted Press, 1974. Pp. 302. $16.75.)

Connell-Smith holds the Chair of Contemporary History at the University of Hull in England. His earlier book on *The Inter-American System* (1966), with Jerome Slater's *The OAS and United States Foreign Policy* (1967), remains one of the two best studies of the emergence and evolution of inter-American organization before the substantial eroding of the "special relationship" between the United States and Latin America.

In this new volume Connell-Smith has broadened his scope to general inter-American relations from the eighteenth century through 1973. He tells his readers that he has explored further sources of information, including documentary materials that relate primarily to Mexico's participation in the Organization of American States, and that he has made "fresh visits to the United States and countries of Latin America, during which [he] was able to discuss [his] subject anew with leading scholars and men prominent in public life" (p. ix).

The resulting "contemporary history," at least to this reviewer, is somewhat disappointing. Connell-Smith comments: "I have tried to view the events of [the 1960s and early 1970s] in a wider historical perspective, and at the same time – as a complementary part of my historical analysis – to view earlier events in the light of the new perspectives which more recent events have induced. Unremitting comparison between historical phenomena occurring at different points in time is an essential feature of a contemporary approach" (p. xvi). Unfortunately, the product of this approach in this instance is not entirely satisfying either as history or as current analysis.

As history, the study is at best a thoughtful survey of the major events of some two hundred years of inter-American relations. Forty pages are devoted to the thirteen years since the Cuban Revolution, which is some indication of the detail that the author has had to sacrifice simply because of his extended time frame. Had he focused on a single era, as did Bryce Wood in his two classic books on the period of the Good Neighbor Policy, Connell-Smith might have had a greater opportunity to do pathbreaking historical research.

Nevertheless, Connell-Smith argues, his study does blaze new trails in that his "approach is more critical – and realistic – than that customarily adopted by United States historians towards their country's policies" (p. xii). In his view, his work offers an important corrective to the assumptions of S. F. Bemis and other traditional U.S. historians that "there exists an essential harmony of interests between the two Americas" and that "in spite of many – even serious – mistakes, their country's intentions have been good" (p. xiii). Indeed, he maintains, "the need is apparent for a reappraisal of [United States] Latin American policy" and "to such a reappraisal, [he] would like this study to contribute, in however modest a degree" (p. xvii).

Yet Connell-Smith advances no real prescriptions for "improved" U.S. policies and his "critical" perspective offers few insights that have not been common to most of the recent literature on inter-American relations. The most successful chapter in his book (Chapter I on "The Background"), in fact, neatly summarizes prevailing academic wisdom about the divergent interests of the United States and Latin America and capsules the increasingly familiar debate as to whether Washington has a unified Latin American policy or one fragmented by bureaucratic politics. In a sense, Connell-Smith has sought to characterize his study as revisionist by setting up a straw man – the traditional historian – as his target. He acknowledges that "in recent years, many North American scholars – social scientists ominously more prominent among them than historians – have written very critically on various particular aspects of inter-American relations and on United States policies" and that he has "quoted extensively from their work" (p. xiii). Whatever he intended, he has not added much to what has already been said.

Broad in scope and largely sound in its interpretations, this volume would make a good text. But the hefty price of the sole hardback edition will preclude widespread use in the classroom.

YALE H. FERGUSON

Rutgers University-Newark

Vienna Broadcasts to Slovakia, 1938–1939: A Case Study in Subversion. By Henry Delfiner. (New York: Columbia University Press, 1974. Pp. 142. $10.00.)

The book contains translations of 51 of the estimated 170 radio broadcasts in the Slovak language transmitted by Radio Vienna under the Nazi control in the period immediately preceding Hitler's destruction of what was left of Czechoslovakia after Munich. The purpose of these Slovak broadcasts was to lure the Slovaks away from the Czechs and toward the Germans.

The modest merit of this book is to assemble some valuable raw materials on the subject of an episode in the Nazi psychological warfare; the translations of the Vienna broadcasts constitute the "bulk" of the slender book. These materials may be perhaps of some use to some future scholar devoted to content analysis or communication theory neither of which seems to be Professor Henry Delfiner's forte.

The initial assumption of this case study was that the Vienna broadcasts had had a significant impact on their Slovak listeners. The opposite seems to be the case: there was no "wholesale defection by Slovaks of the endangered nation" (p. 6); "the Vienna Slovak broadcasts and other German propaganda ventures did not succeed in setting in motion a genuine Slovak independence movement" (p. 47). Discarding an original assumption under the weight of contrary evidence is certainly not a drawback of any case study; in fact, it happens all the time. In this case, however, the author seems reluctant to admit and explain the very limited role the Vienna broadcasts (so laboriously assembled and translated from Slovak to German to English) had finally played. It seems that their sheer existence (as proof of the Nazi determination) rather than their contents might have helped to convince the Slovak leaders, who were anti-Czech but not pro-German, that their resistance to the Nazi policy of divide and conquer was simply useless.

The methodological weaknesses of the book are obvious. The author has not attempted to assess the quantity and quality of the Slovak responses to the Vienna broadcasts; this might have been done by interviewing the surviving listener-witnesses either in Slovakia or among the Slovak exiles in the United States or the author's native Vienna. Rather than intuitively reading between the lines of the Vienna broadcasts and so only guessing, which is always dangerous, about the psychological warrior's assumptions, the author should have attempted a rigorous content analysis. If, for instance, one makes a very rough count of the three major themes in the broadcasts, as stressed by Henry Delfiner, namely (1) anti-Czech, (2) Slovak pride-and-right, and (3) anti-Semitic themes, there are forty-nine anti-Czech references, forty-three exclusively Slovak concerns, and eighteen anti-Semitic ones. Professor Delfiner claims that the Vienna broadcasts merit our special attention on account of their use of anti-Semitism for the first time to win a non-German people over to the Nazi side; this reviewer wonders whether a comparative analysis might not show that, long before the Vienna broadcasts, anti-Semitic themes had figured prominently both in the Nazi broadcasts beamed to West Europe and in the financial support of the right-wing press in France (*Gringoire* and its infamous "rather Hitler than Léon Blum"), Belgium (Rex movement), and England (Sir Oswald Mosley's fascists). Without a comparative perspective, or quantitative/qualititative assessment of the Slovak listeners' reaction, the study is of little utility to a specialist and too narrow in its focus to appeal even to those historians of World War II and who still delight in the discovery of the various footnotes to that era.

IVO D. DUCHAČEK

The City College of New York

Advancing and Contending Approaches to the Study of Chinese Foreign Policy. Edited by Roger L. Dial. (Halifax, Nova Scotia: Centre for Foreign Policy Studies, Dalhousie University, 1974. Pp. 412. No price indicated.)

A group of scholars of various nationalities has produced here a collection of essays proceeding from the assumption that theory and methodology have made considerable progress in the social sciences in general but not yet in the field of "sinology" (a term used in this book to denote the study of contemporary China, although it is more properly applied to the study of traditional China), and that it would be desirable to eliminate this "China differential" – to employ a term devised in another context. The alternative and plausible assumption – that a great deal of theory and methodology in the social sciences, political science included, may be an inappropriate importation from the physical sciences, and one that physical scientists themselves regard as unscientific neoscholasticism, so that what emerges is neither politics nor science – seems not to have been seriously entertained.

The result is a study replete with references to and quotations from other writings on Chinese foreign policy and with formulas and tables designed to yield models, but with only a modest amount of substantive analysis. Although it is debatable whether this approach "advances" the study of the subject very much, the book deserves to be taken seriously because the subject is important and interesting and because the approach is fashionable.

Robert Boardman (Chapter 1), after an interesting analysis of the various principal views of the subject adopted by Chinawatchers ("sinologists"), concludes sensibly that "no unique conceptual framework is needed" (p. 35) and that students of international relations theory and the foreign policies of other states would benefit from paying more attention to Chinese foreign policy.

James Oliver, Robert Boardman, and Roger Dial (Chapter 2) perform content analysis on the interpretations of Chinese foreign policy of a number of American and British Commonwealth writers and conclude that the former are the more prone of the two groups to infer causation of the phenomena described and the less prone to attribute significant causal force to internal developments.

Sang-woo Rhee (Chapter 3) defends the need, in the study of Chinese foreign policy, for theory, the alternative to which in his eyes is "common sense." This is a false dichotomy, since specialized knowledge of the subject, whether accompanied by theoretical generalization or not, cannot be reasonably considered or termed common sense unless the words are twisted to mean something quite different.

Another chapter by Rhee (4) attempts to apply Rummel's field theory to the subject at hand. With the aid of many formulas, it arrives somehow at the shattering error that over the period from 1950 to 1965 there was "no systematic change in any kind of Chinese foreign policy behavior. . ." (p. 147). If this is science, give me "common sense."

Charles W. Kegley, Jr. (Chapter 5), attacks the view (held by no one so far as I am aware) that the foreign policies of the Asian states are completely different from one another and tends toward the opposite error – i.e., the view that they are all virtually alike.

Jan H. Kalicki (Chapter 6) discusses the behavior of the People's Republic of China and the United States during their crises with one another (none of which happily has occurred for about a decade) and concludes sensibly, after considerable theorizing, that each side has improved the sophistication of its performance over time.

Robert R. Simmons (Chapter 7) applies alliance theory – the term alliance being used in a broad, nontechnical sense – to the interaction among North Korea, the Soviet Union, and China during the Korean War, which he correctly views as a difficult process for those concerned. He also develops the interesting hypothesis that the Soviet Union behaved at the United Nations in early 1950 in a way calculated to reduce the chances of Peking's admission.

Roger L. Dial (Chapter 8) applies organization theory to the study of the formulation of Chinese foreign policy in opposition to the "totalitarian" or "monolithic" approach, which he rightly says is discredited and which in fact almost no one holds to.

Robert Boardman (Chapter 9) concludes that "studies of perception and cognition at the individual, interpersonal, and organizational levels can throw a good deal of light on Chinese foreign policy" (p. 343). Perhaps, but his essay asserts the proposition rather than demonstrating it.

Roger L. Dial (Chapter 10) discusses the interest group approach to the study of Chinese foreign policy, with a good sense both of its utility and of its limitations. The same comment applies to Douglas Johnston's essay (Chapter 11) on Chinese treaty behavior.

Geoffrey Lambert (Chapter 12) deals inconclusively with the relevance of elite theory and studies to the analysis of Chinese foreign policy.

One of the useful features of this book is that some of the contributions include notes and bibliographies that taken together form a helpful, although by no means complete, guide to the secondary (almost never the primary) sources on Chinese foreign policy.

HAROLD C. HINTON

The George Washington University

A Global Approach to National Policy. By Richard A. Falk. (Cambridge, Mass.: Harvard University Press, 1975. Pp. 320. $12.50.)

The scholarly work of Richard A. Falk, Milbank Professor of International Law at Princeton University, is distinguished both by originality and by the use of legal theory as an adaptive tool in the solution of contemporary political problems. *A Global Approach to International Politics,* however, is an uneven collection of essays, only one or two of which represent Falk at his illustrious best.

The book consists of four parts. The first, the framework, treats the state system under siege, lawyers in the conduct of foreign policy, and problems of denuclearizing global politics. Part Two deals with what he calls "wider lessons of Indochina." Part Three, "Analytical Modes," which deals with the United Nations and geopolitics and world order, is by far the best section in the book and should be read by all political science students interested in law and international relations. Part Four, "A

Vision for the Future," apart from being excessively dependent on the mistaken conclusions of the Club of Rome, is an interesting but inconclusive schema for studying the future.

Part Two of the book presents Falk at his worst. Many distinguished scholars share Falk's high valuation of the Nuremberg trials. My own view is closer to that of Justice Stone, who had only contempt for Justice Jackson after his participation in the trials. Rustem Vambery, the distinguished Hungarian jurist, pointed out that the rules at Nuremberg had as much in common with customary international law as the sign of a professor at the University of Texas, who having learned that by custom only dons are permitted to walk on the greens of Oxford, posted a sign in Texas stating that, henceforth, by custom only professors would be permitted to walk on the grass. The reasonable ends of wartime diplomacy could have been achieved by application of the laws of war, which were well recognized in international law, by political acts of the victorious powers as part of their war aims, and by the appropriate use of German law, including, although it was probably not necessary even if constitutional, ex-post-facto laws. Instead, the Russians who had slaughtered 10,000 Poles at Katyn and the Americans who had engaged in the fire bombings of Hamburg and Tokyo found the Germans guilty of violating international law and capped the "legality" of the trials by exculpating the Germans for their violations of the prewar naval protocols because the British had also violated them. The principle really established was that in large wars victors try the vanquished. The danger in this, of course, is that it may make it much more difficult to limit and to terminate wars. In a nuclear age, this could produce catastrophe.

Things get even worse when Professor Falk seeks to apply the Nuremberg precedent to the trial of Karl Armstrong, who bombed the Army Mathematics Research Center at the University of Wisconsin, thus causing the death of Robert Fassnacht. Professor Falk tries to be reasonable. He points out that acts of violence will lead to acts of counterviolence; and, therefore, he believes that Armstrong should not have been entirely exculpated. However, Falk believes that the sentence should have been mitigated because Armstrong believed that he was protesting a violent and illegal war that could not effectively be opposed otherwise (a real howler in terms of democratic theory). Surely Professor Falk knows that many people believe that it is simple nonsense to argue that the war was illegal. Indeed, if any state violated international law, it was North Vietnam for, at the time of the incident in Wisconsin, the organized armies of North Vietnam were waging the war against a South Vietnam that clearly had legal personality under international law as well as a right to support from its allies. Moreover, even newspaper reporters now understand that the National Liberation Front was ruled from Hanoi by agents who were officials of the northern party.

Should the judge, prosecutor, and jury have taken this into account and perhaps have convicted Mr. Armstrong of first-degree murder under the reasonable man doctrine? Even if Armstrong believed the building to have been empty, he knew that people sometimes did work late at night and intended an explosion that could kill a person in that event.

Juries and judges do properly take into account ordinary character and motivation in arriving at sentences. To extend this principle to crucial political motivation is dangerous. Factually such extensions do occur, as in the case of the juries in Ohio that refused to convict those responsible for the atrocities at Kent State. Think how much it would damage the system of domestic law were we to write into jurisprudential philosophy the legitimacy of political sympathy in the imposition of penalties at law. It would politicize the law and legitimate its repressive use. Law may need to be tempered by moral concern, but Professor Falk's partisanship sometimes leads him to deplorable conclusions.

MORTON A. KAPLAN

University of Chicago

Neither Peace Nor Honor: The Politics of American Military Policy in Viet-Nam. By Robert L. Gallucci. (Baltimore: Johns Hopkins University Press, 1975. Pp. 187. $10.00, cloth; $2.95, paper.)

The appearance of Robert Gallucci's book marks a watershed in the American literature on the Vietnam War. With it we pass from accounts by polemicists, participants, and journalists to relatively detached reappraisals by young scholars who never observed it directly. *Neither Peace Nor Honor* is, I believe, the first full book about the war derived from a doctoral dissertation by such a student. Also, as Gallucci himself indicates (pp. 1–7), we have passed from the conventional wisdom of the "quagmire" (Arthur Schlesinger's *Bitter Heritage*) to the revisionist conventional wisdom of the "stalemate machine" (Daniel Ellsberg and Leslie Gelb), both of which were forged in and for the heat of home-front battle, and we are now more calmly ready to borrow the new conventional wisdom of "bureaucratic politics" (Graham Allison et al.) to explain America's 1960s Vietnam involvement. No doubt we will soon have a stream of Vietnam books that are as much or more concerned to advance empirical political theory as they are to wave policy flags of any color; such is the nature of academe and the succession of generations. Gallucci's version starts the flow unevenly, but the example he gives is on the whole auspicious, considering the stage in his development and that of the field.

Gallucci's difficulty, I think, is that he, like some others trying to use Allison's *Essence of Decision* for various purposes, has got himself tangled between "Model II" and "Model III." In effect, Gallucci gives an elegant Allison "Model II" interpretation of American military organizational interests and routines as dictating the 1965–68 course of the air war over North Vietnam and of the ground war in the South; but Gallucci is much less successful, it seems to me, in adapting Allison's "Model III" to explain top war-policy making in Washington 1961–68 as it shifted from the Kennedy to the Johnson administrations. Part of the problem is that Gallucci does not even try to distinguish explicitly between Allison's Model II and Model III, lumping them together as "bureaucratic politics," whereas Allison had at least tried to separate "organizational process" (Model II) from "governmental (bureaucratic) politics" (Model III). To be sure, the boundary grew fuzzy in Allison's work as he moved from organizational *routine* (clearly Model II) toward organizational *interest*, which could overlap *"players'"* interests (Model III); Gallucci may have felt that avoiding Allison's fuzzy distinction was a desirable simplification. But the effect, in my judgment, is to diminish somewhat the conceptual clarity of Gallucci's excellent account of field operations (mainly Model II) and to mire him in Allison's other chief area of conceptual muddiness: how *primus inter pares* is the Model III President?

This trap is the most threatening to the "bureaucratic politics" school. Morton Halperin *(Bureaucratic Politics and Foreign Policy)* struggled to escape it by first writing twelve president-centered chapters to show how decisions get made and then writing three bureaucracy-centered chapters to show how they get evaded; despite the artificiality the impact on balance was probably "presidential" (quite *primus*). Allison himself wavered from chapter to chapter, perhaps not entirely consciously; but I would say that the overall impact was "bureaucratic" (not very *primus*). Gallucci, it seems, would like to rate the importance of the President's personal "intellectual baggage" even lower. Gallucci, however, is dealing with two strong-willed presidents, Kennedy and Johnson; and he is too responsible a scholar to distort the evidence. His solution is to write an account which repeatedly veers toward an overemphasis on the autonomous importance of the president's advisers (the Vietnam war "principals"), yet as often checks itself in a kind of informal dialectic, and finally reaches a loose synthesis in the proposition that Kennedy's relative moderation about Vietnam was both *cause and effect* of his keeping some doves in his entourage to balance the hawks, and that Johnson's immoderateness was both cause and effect of his not doing so.

The difficulty here is that Gallucci wants to put the emphasis too heavily on the word "effect" in that proposition. Thus he veers: "In fact, a credible argument could be made that by not accommodating views markedly different from those put forth by his military advisers, President Johnson gradually put himself in the position of a leader without significant options – not a position he or any other president is likely to have contrived to attain" (p. 57). But then he checks himself:

> When decisions are most difficult because the consequences are so grave, it is human for the man who has the ultimate responsibility to seek as much united support as he can gather, rather than to cultivate views that can sound like heresy before a decision and become a political liability after it. So common sense may make us less optimistic about the possibilities of preventing the isolation of future presidents, but the Vietnam experience should convince us that even partial success would make the effort worth while (pp. 134–135).

Amen to that normative conclusion. But the empirical case remains moot since Gallucci has avoided "the game of trying to figure out when President Lyndon Johnson really decided to bomb North Vietnam. That particular moment of truth will be left to others to ferret out" (p. 41). By another (not well documented) Gallucci argument, the president's decision to bomb made so as to forestall domestic opposition to a *prior* private decision to send ground troops. So without having even tried to determine just when Johnson resolved upon this general escalation, Gallucci was in no position to judge whether "hunkering down in a bunker," with those who would and would fit there, was or was not a conscious presidential choice, one that might have persisted from very early in Johnson's administration.

Without at least confronting this historical puzzle, Gallucci could scarcely have expected that a bureaucratic politics model would lead to crisp conclusions about the differences between the Kennedy and Johnson administrations about Vietnam. And it is to Gallucci's credit that he is not procrustean. He knows he has taken us only part of the way in empirical political theorizing about the Vietnam war. It is, on balance, a good start. The next useful step may be to focus on the presidents themselves, perhaps adapting John Steinbruner's cognitive processing paradigm from *The Cybernetic Theory of Decision.*

H. BRADFORD WESTERFIELD

Yale University

The United States Against the Third World: Antinationalism and Intervention. By Melvin Gurtov. (New York: Praeger Publishers, 1974. Pp. 288. $9.50, cloth; $3.95, paper.)

If the 1960s witnessed the traditionalist/behavioralist split, the 1970s are facing the behavioralist/radicalist "great debate." In French social science literature, contributions of Amin, Emmanuel, Palloix . . . tip the balance in favor of the radicals, but in English the battle is undecided. Hence the importance of Gurtov's book.

Gurtov begins by explicating his objective: to add "to the respectability of the critical and radical literature" (p. ix). How? Through the study of four American administrations' fifteen cases of intervention in the Third World, the author analyzes the conflict between "imperial foreign policy objectives" and ". . . the official sympathy with nationalism . . . and political change" (p. 202).

Chapter I discusses three interrelated axioms of U.S. interventionist credo: 1) America's domestic tranquility depends on security and stability abroad; 2) security and stability abroad for the forces of freedom depend on America's willingness to carry out its mission; and 3) fulfillment of America's mission and security depends on a willingness and ability to intervene in the domestic affairs of other peoples (pp. 4–9). The four subsequent chapters substantiate this by treating the Eisenhower administration's interventionism in the Middle East, Kennedy's in Africa, Johnson's in Latin America and Nixon's in Asia. The last chapter sums up "a number of commonalities in the American perception": e.g., 1) political and social instability in the Third World is a breeding ground for radical leftist forces; 2) history's lessons amply justify an imperial foreign policy. When does U.S. intervention move from a perceptual to a behavioral datum? Two primary and three secondary conditions are explicated: i.e., 1) necessity of a revolutionary milieu; 2) a milieu perceived as destabilizing in its policies or programs to other countries; 3) low risk of direct Soviet or Chinese counterintervention; 4) domestic (U.S.), regional, or international authority sanctions U.S. intervention, and 5) a "representative" of local political authority invites it.

Contrary to Gurtov's statement (p. 1), the study's organization is chronological and regional, but his thematic thesis assures continuity. Recent conceptual/methodological issues are ignored (p. 206), yet a global systematic framework (with Third-World regions as its subsystems) coupled with an explicit comparative perspective would be more reflective of U.S. policymakers' perception and values, help identify similarities/differences among intervention cases, increase the study's conceptual unity and consolidate the persuasiveness of the global interventionism thesis.

For instance, the 1954 Czech arms deal with Guatemala justified U.S. action to topple Arbenz. Nasser's 1955 Czech arms deal was certainly more "destabilizing," and yet he continued in power. Why did not U.S. policymakers do in the Middle East what they did in Latin America? Is it geographical proximity as Gurtov seems to imply (p. 82)? But since Gurtov mentions a combination of military, economic, and political reasons and since "cold war attitudes" condition U.S. global behavior, readers would have been better off with a comparative elaboration. Instead the two subsystem cases are not even cross-referenced.

Similarly, Gurtov's solid part on U.S. intervention in Vietnam discusses the "bureaucratic dimension" (pp. 155–158); but because of the absence of a "bureaucratic politics" approach or a conceptual alternative, the reader faces noncumulative statements, and is unable to see the bureaucracy's precise influence or its specific frontiers. Since Gurtov and – for instance – Allison discussed both the bureaucracy and intervention in Cuba, the reader would be interested in knowing Gurtov's views on Allison's findings. Allison is not mentioned even in a footnote. Equally, a basis of the study is U.S. "national interest," and even though the concept is defined perceptually (p. 206), Gurtov does not discuss, for instance, George's "operational code" approach or Holsti's findings of its application to J. F. Dulles.

This aversion to established (establishment?) conceptualization, nondiscussion of its pertinent findings (throwing away the baby with the bath water?) and the weakness of counter-conceptualization lead to predominantly descriptive parts, e.g., two pages on U.S. policy in South Africa and Rhodesia (pp. 79–81).

These limitations, however, should not detract from the study's strong points. 1) The main thematic thesis is pursued systematically, and the supporting evidence varies from tabulated data to telling quotations. The result is that if some area specialists would not find much new about their own area issues, analysis of similar issues in other areas is still rewarding. Precisely, a conscious comparative perspective would have given the book something more than the mere sum of its parts. 2) And because Gurtov writes such good prose, he condenses much material into limited space (e.g., p. 145). 3) The research itself is solid. Computation based on a random sample of the book's footnotes indicates that the ratio of primary to secondary sources is approximately 3:1. Moreover, because Gurtov intends his analysis to be policy relevant, policymakers would find his study enriching and thought-provoking. Students tired sometimes of methodological-conceptual 'hair-splitting' would find Gurtov's book an enjoyable change. Involved political scientists convinced of the soundness of Gurtov's main theses, would have liked this book to be proof that "radical" political analysis and relevant conceptual-methodological refinements are not mutually exclusive. Hopefully, we are at the threshold of this "radical"/"scientific" symbiosis that should characterize the post-behavioral era.

BAHGAT KORANY

Université de Montréal

The National Archives and Foreign Relations Research. National Archives Conferences, Volume 4. Edited by Milton O. Gustafson. (Athens: Ohio University Press, 1974. Pp. xvii, 292. $10.00.)

This volume contains papers prepared for and proceedings of the Conference on "The National Archives and Foreign Relations Research," sponsored by the National Archives and Records Service on June 16–17, 1969, and is part of a series designed to "increase contacts and improve relations between the scholarly community and the National Archives." Altogether, twenty-three short papers are presented, along with brief excerpts from conference discussions.

The uninitiated researcher studying U.S. diplomatic history will get from this collection some sense of the organization of relevant archival collections of the government, their availability, the problems associated with making the best use of them, and to a degree, indications of which areas and historical periods seem most fruitful for future research. Although it suffers from several limitations and deficiencies which reduce its utility, this volume does belong in the reference collections of serious research institutions.

As is almost always true for any conference or symposium, the papers are of uneven quality, and of varying relevance for the intended topic, which in this instance was "the problem of finding a relevant topic and the resources needed to study that topic." The first seven papers, each presented by a member of the National Archives staff, are descriptive commentaries on the holdings of the Archives which bear on U.S. foreign relations, and may be the most helpful part of the volume for those who have not previously used these materials. Included are discussions of document collections from the State, War, and Navy Departments; of captured foreign records; of materials in presidential libraries; of nontextual records; and of neglected materials from non-foreign-affairs agencies which are nevertheless important for some types of foreign relations research. These presentations are hardly exhaustive, but they do supplement usefully the general and specialized guides prepared by the National Archives (listed in "General Information Leaflet No. 3, Publications of the National Archives and Records Service," which can be obtained from the Publications Sales Branch, National Archives, Washington, D.C. 20408).

A second section deals with publication of records on United States foreign relations, specifically the microfilm program of the National Archives, and the *Foreign Relations* series prepared by the Historical Office of the Department of State. William M. Franklin's paper on the latter is of particular interest, providing insights into problems which will be encountered as the time period covered by the series reaches the postwar period, and into the relationship between documents published in the series and the much larger number which are not, but which become available through the Archives when the companion *Foreign Relations* volume is published.

The remainder of the volume is concerned with archival materials bearing on substantive research questions, including domestic influences on foreign policy, the diplomacy of war and peace, United States relations with Europe, with Latin America, with East Asia, and with the Middle East and Africa, and the administrative history of the Department of State. Elmer Plischke, in an extensively documented piece, argues that the latter topic is deserving of more attention than it typically receives, even when the researcher's primary topic is substantive policy and not institutions and processes per se.

Unfortunately, a number of these papers make only passing reference to how the holdings of the Archives can best be employed for research on the topics or geographic areas covered, or to the types of questions for which these materials are particularly important, choosing instead to present brief surveys of existing research, or samples of research drawing upon archival sources. This is frequently interesting, but it reduces the value of these contributions as general guides for researchers working in the topical areas in question.

Several of these papers, however, deserve specific mention. In a lucid essay entitled "Rewriting the History of American Diplomacy during the Second World War," Gaddis Smith asserts the necessity of using multiple sources in a needed effort to add subtlety to interpretations of the American role before, during, and after World War II. Norman A. Graebner draws heavily upon official records to qualify substantially the frequent conclusion that American foreign policy in the nineteenth century was essentially isolationist. Robert Freeman Smith presents a review of the major themes in recent archival research relations between the United States and Latin America in the twentieth century, accompanied by an extensive discussion of Latin American materials in the archives, and of problems encountered in archival research. These include the locating of documents and the need to correct for the perspective gained if only one nation's records are employed in studies of relations between it and another state. Finally, Morris Rieger's survey of sources in the Archives relating to African-American relations should facilitate better use of materials which are among those most neglected. Because a number of useful general points are made in the course of discussing specific topics, the researcher contemplating a project drawing upon the National Archives should range widely through this volume, rather than confining his attention to those papers which seem most germane on the surface.

A final section of the book contains the conference keynote address of Foy D. Kohler, "Reflections of a Professional Diplomat," on the relationship between history, historians, and diplomats.

This volume leaves the reader with the conclusion that although there are many problems in the use of archival materials, they are absolutely essential for the writing of good history. Indeed, one purpose of the conference was to solicit the assistance of scholars in helping to improve the usefulness of the Archives' collections for students of foreign relations. Sadly, at least judging from suggestions contained in the rather sketchy summaries of discussions at the various sessions, little concrete help was forthcoming. This undoubtedly attests to the difficulties inherent in finding solutions to problems of indexing, classifications, and access, particularly when resources are limited, but it may also suggest that there was a natural tendency for each of those attending the conference to focus on irritations encountered in his own research rather than on the more general problem of how the Archives can be structured to be of greatest use to the widest range of scholars.

The staff of the National Archives should be commended on its efforts to improve the dialogue with users of its materials, in the series of conferences similar to the one described here. Four other volumes in this series, reflecting four other conferences, have been previously published, and at least four others are to follow. But it is unfortunate that since five years elapsed between the conference and the publication of this record, much of the information about current research and about procedures for access to materials in the Archives is outdated. This difficulty is only partially corrected by the editor's brief updating. And although a number of the papers make reference to guides prepared to aid the scholar in using these collections, a current brief status report on both the general *National Archives Guide* and available specialized finding aids should have been included, along with information about how they can be obtained.

This volume represents a good idea flawed in execution. It will heighten the interest of many scholars in using archival records, but it will provide them with only a limited amount of assistance in doing so.

WILLIAM I. BACCHUS

Washington, D.C.

Cultures in Conflict: The Four Faces of Indian Bureaucracy. By Stanley J. Heginbotham. (New York: Columbia University Press, 1975. Pp. xii, 236. $12.50.)

Explanations of bureaucratic behavior primarily in terms of cultural variables are rare in the literature of political science. Fred Riggs (*Administration in Developing Countries,* 1964) attempted to get things going by relating general patterns of organizational behavior to different abstract models of social and economic complexity, defined primarily in terms of cultural characteristics. But the book was not easy to digest, and very few followed up his suggestions. At the same time, although unrelated to Riggs's effort, Michel Crozier (*The Bureaucratic Phenomenon* [Chicago: University of Chicago Press], 1964) attempted to explain aspects of French bureaucratic behavior by reference to cultural patterns in France. James Abegglen (*The Japanese Factory* [New York: Free Press, 1958]) did the same sort of thing for Japan. There hasn't been much else. Now, Professor Heginbotham has done it for India. His book is important because it is one of the rare attempts in the literature to treat aspects of culture as independent variables in the analysis of bureaucratic structure and dynamics.

The venue for the analysis was the Indian State of Tamil Nadu (formerly Madras State), and more particularly within that State, the North Arcot District. The evidence for the study was collected during field research in 1967 and 1968. The specific focus was on that part of the development administration of Tamil Nadu which was responsible for implementing a program to popularize the use of high-yielding varieties of rice in the villages. The descriptions of the patterns of bureaucratic behavior in Tamil Nadu are enlivened and made most convincing by numerous accounts of the author's personal research experience in Tamil Nadu while he was collecting his material.

In order to explain the behavioral patterns he observed, the author constructs four different models which he calls cognitive models of organization. Each model attempts to catch a different constellation of norms and values about three interrelated sets of cognitive orientations: first, perceptions of the broad goals and dynamics of an organization; second, perceptions of the motivations, loyalty, and responsiveness to various incentives by people working at different levels within the organization; third, perceptions related to the clients the bureaucracy is supposed to serve. The four models are identified as traditional Indian ("dharmic"), traditional Western ("British colonial"), modern Indian ("Gandhian"), and modern western ("community development"). The differences and similarities among these cognitive models are used to explain some of the problems involved in implementing the development program in Tamil Nadu. For example, one of the major problems was the reluctance to allow the extension workers in the villages sufficient flexibility to be able to respond effectively to the individual requirements of different villages. This problem is explained in terms of the cognitive models. In both the dharmic and British colonial models, the general goal orientation is the maintenance of balance and order in the system, whereas in the community development and Gandhian models the goal is to bring about change. In consequence, in the dharmic model duties undergo little change, they can be specified in detail, and little initiative is required in their performance. The British colonial tradition's elaborate rules and regulations laying down precise procedures to be followed are the functional equivalent of these dharmic prescriptions. Where such cognitive models reign, community development programs requiring flexibility are difficult to implement. Thus, ". . . the community development movement was swamped by the pervasive domination of colonial and dharmic models of compliance systems. The result was not simply a routinization of procedure, but a severe and progressive deterioration of performance, integrity and morale" (p. 172). Frequently throughout the book, cultural variables are used in this way to explain bureaucratic behavior and performance.

Much of the analysis hinges on the four cognitive models; yet it is precisely here that the author will perhaps seem least convincing to both the India expert and the student of comparative politics. The India expert will no doubt focus on his dharmic model. Heginbotham devotes considerable attention to its salient features, and leans heavily on it in his analysis. But there are severe problems in attempting to press the entire range of traditional Hindu values into a single cognitive model. Nor is the notion of *dharma* sufficient in and of itself to incorporate all that is important in the Hindu tradition; after all, *dharma* is only one of a conventional fourfold classification: *artha, kama, dharma, moksha.* Furthermore, the Indian cultural tradition is not only relevant in the villages, as is suggested. One could go on, but the point is that a single model for Indian traditional culture is probably too blunt an instrument for analysis. Indeed, one of the problems with such cultural explanations is to get agreement on how to conceptualize what the culture is. For the student of comparative politics, critical attention will focus on whether or not the author has made headway in overcoming the enormous problems in establishing categories that permit the comparison of similar aspects of two cultures without grossly distorting the cultural reality in any one. The author is alive to the complexities involved, and candidly reviews the difficulties that have been encountered in comparative cultural studies. The construction of four cognitive models of culture (rather than just one) and the way the contradictions and reinforcements between them are used in the analysis are valuable and suggestive, but I fear that the author does not take us much further with the problems of comparison across cultures.

These problems, however, should not obscure the fact that this book is a major achievement. It provides one of the best general descriptions of bureaucratic behavior in India that I have seen. The framework of analysis is highly original. It is also relevant to important issues in comparative politics.

DAVID C. POTTER

The Open University
Milton Keynes, England

Arab and Israeli Elite Perceptions. By Daniel Heradstveit. (Oslo: Universitetsforlaget Publishers and New York: Humanities Press, 1974. Pp. 146. $9.50, paper.)

Professor Heradstveit's principal goals are to arrive at an estimation of "the possibilities of a settlement of the Arab-Israeli conflict" and to contribute to the understanding of Middle Eastern conflict and conflict in general. To accomplish these tasks, ninety-four representatives of Egyptian, Jordanian, Syrian, Lebanese, Palestinian, and Israeli "elites" were interviewed by the author in order to generate data on the perceptions, goals, problems, strategies, and minimum negotiating conditions of the main parties involved in the conflict.

Invariably, assessments of the perceptions of parties involved in serious conflict tend to yield depressing catalogues of misperceptions. The Arab-Israeli conflict is no exception. Among the classical misperceptions revealed in Heradstveit's data are: (1) cultural and ideological stereotypes; (2) underestimation of the adversary's sense of threat; (3) reference to past events referred to as evidence that both sides will eventually reach their goals (at the same time, each party also gives a very optimistic evaluation of its own situation with a correspondingly pessimistic evaluation of that of the adversary); (4) strong tendencies to ignore information which does not fit with the established image of the adversary – in addition, the regarding of extremist groups or extremist signals that have very low credibility as indicating the intentions of the adversary as a whole; and (5) the tendency to describe the intentions of one side's Great Power backers as being much more sympathetic than the intentions of the adversary's backers, while both sides maintain exaggerated notions about the dependency of the adversary on their respective backer states.

Vis-a-vis the possibilities for negotiations, Heradstveit finds internal struggles among the competing strategies of political solutions, military solutions, and status quo (neither war nor peace) maintenance. The author concludes that within the Arab world, both the political and the military strategies have strong followings and that the relative strength of the two alternatives fluctuates with the general political situation, the strength of the guerrillas, and the calculations concerning the impact of negotiations with Israel on internal political stability. Further complicating the Arab side of the conflict are the different and contradictory interests (summarized by the author as territorial retrieval vs. reduction of the conflict level) perceived by different Arab actors. For the Israelis, the parallel struggle is between negotiated settlement and status quo maintenance, with the latter appearing more attractive since the expected gains from negotiation appeared to be slight. In sum, Heradstveit observes that some potential for negotiation has been present and that the outlook for negotiation will probably depend on changes in the distribution of Arab-Israeli capabilities and the development of a widespread feeling that the political and military costs of maintaining the status quo are too high.

The book achieves some success in realizing its goals by presenting data-based information on the cognitive dimension of an important conflict which has not been known for producing an abundance of objective and empirical studies. Nevertheless, the book definitely has its liabilities. Perhaps most fundamental is the "elite" foundation upon which the interviews were based. Very little attention is given to who belongs to the elite sector in the Middle East or for that matter, what role elite opinion plays in the Middle East. Instead, Heradstveit interviewed a "purposive sampling" (outlined in the table below), which often seems to represent a euphemism for talking to whomever one can. Given the difficulties of doing field research in the Middle East, particularly on this sensitive topic, this result may be about as much as can be realistically expected. Still, the reader is left with the distinctly uncomfortable feeling that while the conclusions seem fairly accurate, it is not clear to what extent the ninety-four respondents represent elite opinion in their respective countries nor whether the respondents are cross-nationally comparable. Add to this the following problems: (a) the Arab interviews were conducted in March, 1970 (before the Jordanian suppression of the Palestinian guerrillas) and the Israeli interviews on March, 1972; (b) different respondents were apparently asked different questions; and (c) nowhere in the study are the data summarized in tabular form (the reader is given only a list of the questions and the author's interpretations and conclusions). The end result is an intriguing study that has restricted value because of the several problems in the research design and the reliability and validity of the data obtained. Furthermore, the study is only very weakly connected to the more general literature on conflict misperceptions and not at all to other studies of the Arab-Israeli conflict.

Finally, Heradstveit's book may be partially dated by the impact of the 1973 October War on elite perceptions and the possibilities for negotiations. This is an empirical question which requires further analysis. It is especially unfortunate, however, that the various sins of omission and commission associated with this study probably foreclose what might have been an exceptional opportunity to create a panel for follow-up surveys.

WILLIAM R. THOMPSON

Florida State University

National Monetary Policies and International Monetary Cooperation. By Donald R. Hodgman. (Boston: Little, Brown & Co., 1974. Pp. 266. $12.95.)

Problems of a World Monetary Order. By Gerald M. Meier. (New York: Oxford University Press, 1974. Pp. 305. $4.95, paper.)

A reviewer of books on monetary matters may be tempted to consider publications prior to 1973 as of historical interest. The year exuded a sense of before and after. Before the Yom Kippur War of October and the oil price hike of November, Finance Ministers could assemble at Nairobi to discuss the IMF Committee of Twenty's "First Outline of Reform" and set a deadline of July 31, 1974, for agreement on the issues still outstanding. Since the war, Finance Ministers have either hedged on deadlines or concluded that the present system's resilience has been proved in practice.

To judge by Professor Hodgman's *National Monetary Policies and International Monetary Cooperation,* and Professor Meier's *Problems of a World Monetary Order,* the two approaches were latent in the old regime. "Mystery," "maze," and "muddle," writes Meier in the Preface, "are the words that most frequently follow 'international monetary.' But international monetary problems are continually being resolved one way or another" (p. vii). More soberly, Hodgman concludes that "no system of regulation that applies uniformly to the entire (European) Community can remove the problem of reconciling diverse interests among individual members. Systems applied by individual countries are inconsistent with the goal of a unified Community-wide capital market as a main feature of monetary and economic integration" (p. 237).

Perhaps the nuances of difference between the two authors derive from differing foci and objectives. Meier's book is aptly conceived for teaching in the classroom and provides a broad brush introduction to international monetary experience since Bretton Woods. Hodgman's book specializes on the structural variety of Western European capital markets and monetary policies. Understandably, his judgments in respect of solutions are more reserved.

One thread common to both authors is the increasing subordination of domestic policies to the exigencies of world trade and capital flows. To some, the Bretton Woods system's failure to provide adequate liquidity restrained economic expansion in major industrial nations and impeded development in the poor. Fixed exchange rates were untenable as capital flows expanded and growth or inflation differentials induced changes in the international hierarchy of states. To others, the system was never

Heradstveit's Purposive Sample

	Active Politicians		Civil Servants		Editors/ Journalists		Professional Students		Guerrilla Leaders		Others		Total	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
Egypt	2	7.4	4	14.8	12	44.4	3	11.1	6	22.2	0	–	27	28.7
Jordan	1	9.1	1	9.1	0	–	4	36.4	3	27.3	2	18.2	11	11.7
Syria	0	–	1	33.3	0	–	0	–	0	–	2	66.7	3	3.2
Lebanon	2	13.3	0	–	2	13.3	9	60.0	1	6.7	1	6.7	15	16.0
Israel	7	18.4	3	7.9	9	23.7	15	39.5	NA	–	4	10.5	38	40.4
Total	12	12.8	9	9.6	23	24.5	31	33.0	10	10.6	9	9.6	94	100.0

Note: This table is constructed from information given by the author (pp. 135–137) in his list of respondents. Palestinian respondents are apparently attributed to the country in which they happened to be interviewed.

implemented. The United States played banker to the world economy and the dollar became the international medium of exchange. By the time that President Nixon declared the dollar's inconvertibility into gold in August 1971, foreign states were so implicated in the dollar's fate that they had no alternative to accepting the consequences of America's new economic policy.

An international approach to the monetary history of the 1960s – interestingly documented by Meier – starts with the rapid change from dollar gap to glut as capital movements were liberalized and the deficit on American capital account mounted. As dollars accumulated in European central banks, doubts grew – with President de Gaulle's active encouragement – at the United States ability to honor its commitments. Each attempt to shore up confidence in the dollar aggravated Atlantic relations. Department of Defense arguments on liberal access to European arms markets were contrasted to its nationalist procurement policies. The success of interest equalization tax in discouraging foreign borrowers on to United States capital markets was set against the limited success of voluntary or direct controls on direct overseas investment by American corporations. With the institution of the two-tier gold market in March 1968, and the first discussions on the Special Drawing Right in the IMF as the new unit of account, speculative pressures began to build up.

As one financial crisis succeeded another learned discussions were held over whether the United States should adjust to the growing lack of confidence in the dollar by devaluation against gold, or whether the dollar "hemorrhage" was not providing the world with needed liquidity. The first school of thought tended to stress the asymmetrical advantage that the United States derived from maintaining an overvalued currency through the purchase of foreign companies or the expansion of American overseas holdings. The second school of thought pointed to the benefits in trade which the United States was foregoing by maintaining an overvalued dollar and to the necessary burden of leadership in a rudderless world. Power politics and finance fostered many a mixed metaphor.

Imperceptibly, though, the Western world slid into a regime of floating exchange rates as no government – and especially the American – could stem capital movements in response to interest rate differentials. Only skeptics fastened on the fact that fluctuating exchange rates accentuated asymmetries between trading nations according to the degree of national production engaged directly in the pattern of world exchanges. Meier's book fairly presents both viewpoints.

Hodgman's originality lies in tracing decreased government control over domestic economic environments from the viewpoint of the mixed economies of Western Europe. He studies in fascinating manner the various monetary and credit structures of Western European states, and illuminates detail with reference to the theoretical underpinnings of government objectives. He places himself squarely on the ground of the contribution of government monetary management to the attainment of the four major goals of national policy: full employment, growth, price stability, and balance of payments equilibrium.

A primary distinction drawn by Hodgman is between the implications for policy structures and instruments of monetary and credit management. Where policy emphasis is on the control of monetary aggregates, government direct intervention in the processes of credit markets is minimal. "By contrast, concern by the authorities with the structure of interest rates and credit flows always involves forms of administrative intervention into credit markets to modify the structure of interest rates and the allocation of credit that otherwise would result from market processes" (p. 199).

The paradigm of success in aggregate monetary management is obviously West Germany where the central bank has sought with increasing success to regulate liquidity of commercial banks and other credit institutions. Inflation rates are the lowest in Western Europe, as closer government controls since 1967 over budgetary policy has resulted in a freer hand for more vigorous monetary policy. The challenge to effective monetary policy from international capital flows was finally met in 1973 by the imposition of stiff disincentives to foreign speculators on the DM's revaluation and of draconian deflationary measures.

Hodgman provides a quarry of information and fresh insights into the difficulties confronting national monetary management in Western Europe. He carefully explains how Italian efforts to allocate credit have been thwarted by the growing need for central government finance. His account of France's giving priority to credit allocation and control of interest rates for privileged circuits illuminates the growing inflationary pressures in the French economy and the insistence of Paris on a Europe of the Fatherlands where each nation is responsible to its own development. His devastating critique of British capital policy is summarized in the pithy sentence: "The complexity of this

(United Kindgom) network of markets and institutions led the Radcliffe Committee to refer to the British financial system as sophisticated" (p. 206).

I cannot resist the temptation to recommend Professor Hodgman's book as indispensable reading for my (British) compatriots. He places the problem of Britain's national debt – accumulated over two world wars from 656 million pounds in 1913 to 27,000 million pounds in 1960 – at the center of British monetary dilemmas. He doubts the efficacy of the 1971 Credit Control "reform" by asking whether the government will be prepared to finance the government debt including new deficits by noninflationary means, accepting in this process the interest rates on government debt determined by market forces. "Attempts to stabilize the market price of long-term government debt hampered the execution of monetary policy in the United Kingdom throughout much of the past two decades... By contrast, repudiation of government debt inherited from the years of World War II in Germany has as one result the removal of a comparable constraint on the vigor of monetary policy in that country" (p. 214).

Curiously, Hodgman ends on a plea for comprehension for new forms of supranational organization. Yet he remains skeptical at European efforts to "harmonize" their varied national monetary and credit policies. Perhaps his distinguished career as teacher, author, and consultant to the Federal Reserve Board of Governors and to the United States Treasury has led him to verbal circumspection. That is not least of the attributes of this excellent book.

JONATHAN STORY

Institut Européen d'Administration des Affaires

The Middle East and North Africa in World Politics: A Documentary Record. Volume I: European Expansion, 1535–1914. Second Edition, Revised and Enlarged. Edited by J. C. Hurewitz. (New Haven, Conn.: Yale University Press, 1975. Pp. 616. $30.00.)

This volume of Professor Hurewitz's enlarged documentary collection is twice as big as volume one of its predecessor, *Diplomacy in the Near and Middle East* (Princeton: Van Nostrand, 1956), and twice as useful. Of the seventy-odd new documents, about fifty concern Morocco, Algiers, Tunis, and Tripoli; nine are on Afghanistan. These areas are added to the original coverage of "non-Soviet southwest Asia," including Iran, plus Egypt and the Sudan. There are also new documents on Iran and the Ottoman Empire. Of the 184 entries (some of two or more documents) all but 40 are post-1800.

The major problem in this edition remains the same as in the first – the general exclusion of the Balkans. That peninsula, like most of the real estate Hurewitz covers, was once within the Ottoman Empire, and was a focus of contention among the great powers; its omission means that the Eastern Question cannot be seen whole. Hurewitz does restore articles of the treaty of Küçük Kaynarca on the Balkans, and adds the treaty of Bucharest (1812). But the treaty of Arianople (1829) is missing, and the treaties of Paris (1856) and Berlin (1878) appear in part only. This principle of selection, emphasizing European imperialism in Islamic lands, is the editor's considered choice.

Each document is preceded, as in the first edition, by a concise and well-written note outlining the historical context and referring to scholarly bibliography. Many notes have been extensively revised. For instance, the French-Ottoman treaty of 1535, accepted in the first edition as an accomplished fact, is here correctly called a draft, lacking the Sultan's ratification. The bibliographical references are also updated, and a new thirty-page bibliography appended. There is no index, but a good table of contents. Unfortunately some editorial gremlin, which let a good many typographical errors slip by, also allowed incongruent page listings in the table for documents 38 through 41.

The documents are principally treaties of peace, commerce, navigation, alliance, or other formal agreements between governments, both Middle Eastern and European. There are also texts of capitulations, economic concessions, fermans, and statements of policy (mostly British), plus some other tidbits like first-hand French and Ottoman reports on the Algerian fly-whisk incident of 1827. All in all, this is a well-edited and most valuable compilation.

RODERIC H. DAVISON

George Washington University

Vision Accomplished? The Enigma of Ho Chi Minh. By N. Khac Huyen. (New York: Collier Books, 1971. Pp. 377. $8.95, cloth; $2.95, paper.)

The incredible Ho Chi Minh, as he was called by his best-known alias, completely blended cosmopolitan and local roles over a long epoch greatly influenced by his own achievements. Inevitably his extremely varied dramatic experiences engendered many myths. Professor Nguyen Khac Huyen, chairman of the Department of Political Science at the College of St. Catherine in St. Paul, Minnesota and a Vietnamese who witnessed the early stages of the First Indochina War, diligently attempts to explicate the enigma of Ho's life history as it was intertwined with modern Vietnam's tragedies, triumphs, and myths.

Other biographies – e.g., those by Jean Lacouture, David Halberstam, Reinhold Neumann-Hoditz – and public relations tracts from Hanoi have provided details of Ho's life despite his famous comment to Bernard Fall that an old man should be allowed to have a few secrets. Huyen provides greater detail, though some facts remain secret and others remain debatable. He supplements the description of historic events with accounts of incidents he witnessed personally as a child – such as the 1946 National Assembly election – thereby amplifying important developments other biographies have treated sparsely.

Huyen stresses Ho's perseverance in pursuing a consistent plan from 1911 to 1969. Ho survived decades of Communist ideological conflicts, Hong Kong and Chinese prisons, frequent illness, the Mao-Chiang break, brutal policies of Chinese warlordism and French colonialism, the Tonkin maquis, and party Vietnamese debacles in 1930–31 and 1940–41 for which Huyen assumes Ho was responsible. Ho's 1945–46 cooperation with the French could have resulted in his assassination by Vietnamese non-Communist forces, by the Chinese, and particularly by dissident Viet Minh. Ruthlessness was a factor in Ho's survival. According to Huyen's account, Ho sold Phan Boi Chau to the French and had countless village elders assassinated, a generation of opposing Vietnamese leaders eliminated, and untold numbers of highlanders hunted down after 1954. Ho's strategy in both Indochina wars emphasized the tactical advantages of civilian casualties. This strategy led to deliberate actions by the Viet Minh to maximize the numbers of civilian casualties and to remake society by destroying the existing social and economic order.

As Huyen's biographical information proves, Ho, despite the enormous suffering in Vietnam resulting from his strategy, was unquestionably the most renowned hero in Vietnamese history. As a result of Ho's revolutionary leadership throughout Asia and later specifically in Indochina, his image took on legendary proportions. Independence *(doc lap)* was the foremost Vietnamese goal, and Ho, the incorruptible revolutionary, epitomized it. Nonetheless, invariably a major question carefully analyzed by Huyen has been presented frequently as the principal enigma of Ho's life: was he primarily a nationalist or an international Communist? The enigma is analyzed in detail for each major phase of Ho's life in Huyen's biographical account. Huyen documents information on Ho's leadership of the entire Southern Bureau of the Comintern's South Asia Section, on his active role in numerous third International Asian and European leagues, on his actions as representative to the Comintern and Kresintern and as an advisor to Borodin, and as a student at the University of Toilers of the East. Huyen traces Ho's career as a founding member of the French Communist party and the Indochina Communist party and subsequent fronts and as a recruiter for revolutionaries and espionage agents in Siam, Hong Kong, China, and Europe. Huyen's theme is that although Ho's Communist credentials were unquestionable they were always questioned by many French leaders, Vietnamese nationalists, and scholarly Western accounts until Ho's death in 1969 because many observers refused to believe he could be simultaneously a nationalist and a Communist.

Huyen carefully documents the fact Ho was also a thorough-going Vietnamese chauvinist and expansionist. In fact Huyen reinforces my own observations it was Ho and not Ngo Dinh Diem who was in Denis Warner's terms the last Confucian. As Huyen shows, Ho, the son of a disappointed mandarin, received the mantle of the emperor in the popular view. He did this by being the father and sage possessing superb recognizable *tuong* (proper facial features) and by being the stern disciplinarian who sacrificed all for the classic Vietnamese ideal of a struggle for a united nation always advancing south and west. Huyen proves Ho retained this respect despite serious government failures of the Hanoi regime between 1954 and 1969 described in the biography.

Perhaps what Huyen proves more conclusively than other biographers is that Ho never doubted his vision would be accomplished. Huyen's title question seems to have been answered. Yet in fact probably the vision was broader than Vietnam, a theme Huyen alludes to often, and Ho's successors will in time fulfill that broader vision as Vietnamese push outward under leadership of hosts of cadres of new mandarins utilizing interlocking associations common both to Communists and to Vietnamese. That may be the answer to the enigma.

CHARLES A. JOINER

Temple University

China, Pakistan and Bangladesh. By J. P. Jain. (New Delhi: Radiant Publishers, 1974. Distributed by South Asia Books. Pp. v, 264. $14.00.)

Indian and Pakistani authors writing about Sino-Bangladesh, Indo-Pakistan, or Bangladesh-Pakistan relations can hardly overcome the built-in biases which permeate the political atmospheres in their respective countries. Both Pakistani and Indian writers usually display a lack of scholarly detachment and objectivity, their works being heavily loaded with patriotic partisanship. This tendency is even more evident in the recent literature provoked by the Bangladesh issue. Thus, the reading of "an objective and scholarly study of Chinese relations with Pakistan and Bangladesh" (as the flap of the book under review rightly claims) by an Indian scholar is a refreshing experience.

In the first half of the book, Dr. Jain delineates how the pressure of ,events rather than cool-headed calculation brought mid-'sixties China and Pakistan closer. From the very beginning, he tells us, Pakistan made overtures to China to develop some sort of understanding. Although alignment with the West was the sheet anchor of Pakistan's foreign policy in the 'fifties, the rulers of Pakistan were successful in convincing China that the Western alliances were not in any way directed against China; and, as a result, Peking adopted an attitude of noninvolvement in the crucial Kashmir issue. Dr. Jain notes that Pakistan became disillusioned with the Western allies after the Sino-Indian border war in 1962 when, ignoring Pakistan's protest, both the U.S.A. and the U.K. together with the U.S.S.R. rushed massive military aid to India. This response led Pakistan rulers to a fundamental rethinking of Pakistan's whole pattern of relations with others. The Chinese got alarmed and wanted to have close allies in the immediate neighborhood to avoid complete encirclement. According to Dr. Jain, the interests of Pakistan and China converged, leading to the Sino-Pakistan boundary agreement in March 1963, to the China-Pakistan accord on air-service in August 1963, and to Peking's all-out support of Pakistan's stand on Kashmir in February 1964.

Dr. Jain then argues that Chinese support encouraged Pakistan to send infiltrators to India-held Kashmir leading to the Indo-Pakistan conflict of 1965. China's threatening posture, however, prompted the two superpowers – the U.S. and U.S.S.R. – to take joint action in bringing about the ceasefire.

Dr. Jain again argues persuasively that Russian proposal for "Asian collective security" exacerbated Peking's fear of encirclement and led Peking to entice Pakistan again to her side and that new relationship developed among Peking-Islamabad-Washington following Dr. Kissinger's secret visit to Peking via Islamabad emboldened Yahya Khan to use military force in East Pakistan. Dr. Jain, however, takes pains to dig deep into the Chinese official pronouncements in order to prove that the Chinese tirades were mainly directed against Russia. He points out that "in emphasizing a reasonable settlement . . . the attitude of China was not very much different from other superpowers – the United States and the Soviet Union" (p. 175) and that "China has always cherished profound friendly sentiments for the people of Bangladesh" (p. 201).

Dr. Jain rightly concludes that China would continue to cultivate Pakistan because "without a foothold in Pakistan, China would be facing the solid phalanx of the U.S.S.R.-Afghanistan-India single-handed" (p. 231), but China would also keep her options open toward Bangladesh, a strategically important area, bordering near the two fingers of "the Chinese Tibetan palm."

Dr. Jain does not have the elegant style of Maxwell (*India's China War* [London: Jonathan Cape, 1971]) or Rowland (*A History of Sino-India Relations: Hostile Co-existence* [London: D. Van Nostrand Co., 1967]). The format, printing, and paper of the book are also inferior to those of the treatises on similar subjects published in the U.S.A. and U.K. These limitations of the book, however, do not detract from the substantive point that it is one of the few in-depth scholarly studies of Chinese involvement in the South Asian scene.

TALUKDER MANIRUZZAMAN

University of Dacca, Bangladesh

India and Disarmament: Volume I, Nehru Era, An Analytical Study. By J. P. Jain. (New Delhi: Radiant Publishers, 1974. Pp. 211. $10.00.)

Universal and complete disarmament, regular run-of-the mill disarmament, arms control, arms limitation and nuclear test ban and nonproliferation, were the various captions used to describe the international negotiations dealing with the arms race since Hiroshima and Nagasaki. As these very names given to the successive efforts clearly suggest, there has been a steady decline in "idealism" and a corresponding elevation of "realism" in the disarmament goals the world community set for itself. Mutual rivalries and suspicions among nations, especially between the erstwhile Communist camp and the Western bloc, militated against disarmament. Neither the U.S. nor the U.S.S.R. was willing to "lower the guard" unless the other took the first move, and there was not much chance for progress till the atmosphere of suspicion and hostility was itself contained to manageable proportions. The tortuous and complex disarmament negotiations reveal any number of reversals and contradictions in the posture adopted by nations from time to time. J. P. Jain, Visiting Associate Professor, Department of Disarmament Studies, Jawaharlal Nehru University, seeks to tell the intricate and often exasperating story from the vantage point of recounting the "role" of one nation. Although not a big power herself, India under the leadership of Nehru took an active part in the world efforts to reduce the threats to peace and to live with the bomb. India's nonalignment helped her to play the role of an intermediary and a catalyst in the East-West negotiations during the cold war years.

As Jain points out, India did not hesitate to support one side or the other when it suited its interests. India worked for the avoidance of nuclear confrontation during the postwar era because the noncombatants were not immune from the "fallout" and other consequences of a nuclear war. It was India's hope and belief that if the arms race were curtailed, more resources would be available for economic development – the single most crucial goal of the nation.

From the very beginning, India consistently held the view that progress in arms reduction and disarmament would be impossible unless the nations that had the atomic weapons – the Big Powers – were willing and ready to act. India also insisted that no meaningful progress could be achieved unless China were drawn into and actively involved with the disarmament negotiations, as Jain documents carefully. The two key elements of these negotiations have been the chicken-and-egg duo: which should come first, disarmament or international inspection and control? This fundamental impasse disrupted many disarmament negotiating sessions. What is ironic is that the West and the East reversed themselves on this issue more than once.

India was all in favor of international control to the extent necessary to ensure peaceful uses of atomic energy. But, as Jain points out, India was emphatically opposed to any restriction on her sovereign right to utilize peacefully her own atomic energy resources. The study clearly indicates that in response to the Chinese bomb and the determined bid to develop delivery vehicles, India began to look upon the nuclear option more favorably. Developing its own bomb was then regarded as the only possible course open to India to balance the might of China, unless the Big Two were prepared to offer suitable joint guarantees. This part of the story, Jain says, belongs to his forthcoming study. One hopes that it will be equally well documented and also somewhat more readable. The laborious presentation of an endless chain of quotations from the Indian leaders or delegates, and summaries of this or that relevant UN document need to be modified. This shortcoming, however, should not detract us from the utility of the present study as a source of ready reference for future scholars of disarmament in general and of India's role in the negotiations in particular.

B. RAMESH BABU

University of Bombay

The Lin Piao Affair: Power Politics and Military Coup. Edited by Michael Y. M. Kau. (New York: International Arts and Sciences Press, 1975. Pp. 591. $20.00, cloth; $8.95, paper.)

The Cultural Revolution and Lin Piao's abortive military coup, the editor of this book remarks, have probably been the most significant and dramatic developments in the political system of China since the Communists seized power in 1949. I would agree with this evaluation, and add that the Lin Piao affair should be viewed against the background of the Cultural Revolution, of which it was a consequence. During that revolution Mao Tse-tung had violated the rule that the party must always command the gun, and that the gun must never be allowed to command the party. Backed by the military establishment which Lin Piao led, in his capacity of defense minister, Mao demolished a party which had become resistant to his will. At the conclusion of the Cultural Revolution, with Mao's reconstruction of the party structure underway, the time came to force the military genie back into the bottle of party control. It was that endeavor which touched off the Lin Piao affair.

If the spectacle of Chairman Mao leading an uprising against his own party had bemused outside observers, its sequel, the Lin Piao affair, seemed almost equally strange. After all, the aged Mao Tse-tung whom Lin Piao allegedly attempted to assassinate had but recently arranged Lin's elevation to the position of sole vice chairman of the party, and his designation by name, in a revised party constitution, as the Chairman's "close comrade-in-arms and successor." Casual consideration might lead one to dismiss such affairs as too mysterious for comprehension. But in actuality it is occurrences like the Lin Piao affair – occurrences too important and too eruptive to be kept entirely out of sight – which are susceptible of at least partial and tentative reconstruction, and of providing us with useful insights into what would otherwise be the hidden workings of upper-level Chinese politics.

Michael Y. M. Kau, in *The Lin Piao Affair: Power Politics and Military Coup,* provides upwards of 600 pages of translated source materials relating to that affair, together with an introduction of some 58 pages presenting his own reconstruction and analysis of it. The most important of the source materials are the translations, comprising some 68 pages, of eleven official documents issued by the party center in Peking. Included among them are a coup planning paper prepared by a group of plotters led by Lin Piao's son, Lin Li-kuo; an account of explanatory talks held by Mao Tse-tung with key regional commanders, in preparation for his intended showdown with Lin Piao; and excerpts from the report of an ad-hoc investigation team, set up by the party center in the wake of Lin Piao's flight and death. While all but one of these documents had been labeled "Top Secret," and though most of them first became available through Taiwan, they have generally, and I think correctly, come to be accepted as authentic – though not necessarily reliable in terms of content.

The case against Lin Piao, basically made in the official documents, was elaborated in speeches by officials and articles in the printed media. The volume under review contains about 135 pages of such materials. They are followed by a concluding section, comprising over half the translated source materials, devoted to Lin Piao's speeches, writings, and instructions from the 1965–1970 years which spanned the period of the Cultural Revolution. It is difficult to regard many of the more extreme accusations leveled against Lin Piao, recorded in the speeches of officials and the published articles, as representing anything but a campaign designed to reverse his long-held reputation of revolutionary hero and loyal supporter of Mao Tse-tung. The real Lin Piao tends to emerge, however, from the speeches, writings, and instructions which comprise the book's final section.

I know of no single reconstruction and analysis of the Lin Piao affair which is more illuminating than Mr. Kau's in his introduction; and at least two of the items contained in the section devoted to the "Top Secret" documents – the Lin coup plan outline and the summary of Mao's talks with the regional commanders – make fascinating reading. The remainder of the book, in my opinion, will be of interest chiefly to those readers who wish to make their own reconstruction and analysis of the Lin Piao affair, or who are willing to sift through Lin's speeches, writings and instructions of pre-coup years in order to obtain the occasional nuggets of interesting information which they contain. Such readers will want to make up their own minds about whether Lin Piao actually ordered his coup plan put into effect, as the editor appears to assume (pages li, lii and lxv), or whether contrary statements elsewhere attributed to Chou En-lai may be more reliable; and they may ponder the question of whether the coup plotters' map of *Tiao-yü t'ai* refers to the island of that name (as a footnote on page 104 suggests) or to the similarly named official guesthouse compound in Peking. But they will be grateful to find in one volume so large a selection of translated source materials and so stimulating and generally reliable a body of editorial comments.

EDWARD E. RICE

University of California, Berkeley

Soviet-United States Naval Balance. By Robert A. Kilmarx. (Washington, D.C.: Georgetown University Center for Strategic and International Studies, 1975. Pp. 181. $5.00, paper.)

During the first twenty-five years after World War II there were few published, English-language works on the Soviet Navy and only two could be considered "significant contributions" – the collection of essays published as *The Soviet Navy* (New York: Praeger, 1958) and Robert L. Herrick's excellent *Soviet Naval Strategy* (Annapolis, Md.: U.S. Naval Institute, 1968).

In the last six years there has been a profusion of books and monographs on the Soviet Navy few of which, in my opinion, have added significantly to our knowledge or understanding of the subject. This paperbound monograph is both useful and readable.

Dr. Kilmarx astutely notes that "the game of manipulating ship numbers, age, and capabilities can be played convincingly to a predetermined outcome by any skilled practitioner of naval theory, by mathematical models or logical progressions, depending on his bias and assumptions" (p. 1). How true! The author then starts with the basics – why the United States and the Soviet Union have navies and how they seek to employ them – and he points out the importance of navies in peacetime – as Admiral Gorshkov claims – "to vividly demonstrate the economic and military power of a country beyond its borders" (p. 4).

In seeking a useful method of analyzing naval balance, Kilmarx considers four functions for general or multipurpose forces: (1) restrictive and preclusive operations, as when Soviet ships in Egyptian ports in October 1967 inhibited Israeli attacks and Soviet demands for port visiting rights in the Mediterranean; (2) supportive operations, as when the Soviets claimed that the presence of their ships near the Libyan coast alone prevented a Western-backed counter-coup when King Indris was overthrown in 1969; (3) protective operations, primarily protecting nationals overseas and commercial shipping, a "legitimate" rationale for the use of naval power; and (4) projection operations, both planned and improvised. The last function according to Kilmarx, reflects how "a sustained naval presence in an area can assure a major power a voice in the multilateral diplomacy that will shape the political future of the states in the area" (p. 9).

Whether or not one agrees completely with this analytical scheme, it does demonstrate the difficulty in understanding and categorizing modern naval operations, and their increasing importance.

After a comprehensive introductory chapter, Kilmarx provides separate chapters on the "strategic dimension" of the naval balance; on Europe's flanks; on the Indian Ocean; on the Pacific; and on the Caribbean. The strategic discussion is especially useful in the context of the SALT and Kissinger-versus-Schlesinger debates; also, the author explains why it probably would be impossible to "trail" Soviet missile submarines effectively should the USSR increase deployment to the level of U.S. Polaris/Poseidon operations (just over 50 per cent of the force at sea). This is significant because most Western writers on the subject cite "trail" as the most effective method of ASW against strategic missile submarines.

Unfortunately, there is only a brief summary and no chapter for Kilmarx's conclusions. Perhaps this omission represents his efforts to maintain a degree of objectivity, allowing readers to form their own conclusions. But the message is, painfully for the West, obvious: virtually all trends in the naval balance now favor the USSR. This is not to say that the Soviet Navy is "ahead" in all or even a majority of measurements. Rather, by almost all criteria, whereas a decade or two ago the U.S. Navy was preeminent in essentially all categories, we have today lost in some and are losing in others.

Kilmarx can be criticized for mentioning research, merchant, and fishing fleets, as well as other nonnaval elements of "sea power," without discussing them sufficiently. He has narrowed his margins, however, by use of the term "naval" rather than "maritime" in the title. Also, the book contains a disturbing number of small errors in the description of ships and weapons considering the plethora of unclassified material available. For example, by 1975, it was known that there were only thirty-three or thirty-four Yankee-class submarines, not thirty-eight (p. 67); the Krivak destroyer was then believed to carry the SS-N-10 missile (now the SS-N-14), but never the SS-N-9 (p. 70); the Baltic Fleet has no "ocean-going capability" on the basis of observed exercises and other indicators (p. 71); the U.S. mine warfare ships based in Japan were mine countermeasure units and not a "mining force," and it was disbanded in the early 1970s (p. 137).

Again, given the large number of words being published on Soviet naval matters, this small volume is both useful and readable. In a period when American use of the sea is probably more important than ever before in the nation's 200-year history, it is also highly relevant.

NORMAN POLMAR

Editor, United States Sections
Jane's Fighting Ships (1967–1976)
and *Ships and Aircraft of the U.S. Fleet*

A Nation Writ Large? Foreign Policy Problems Before the European Community. By Max Kohnstamm and Wolfgang Hager. (New York: Macmillan, 1975. Pp. 275. $17.00.)

When Carl J. Friedrich wrote a book under an almost identical title (*Europe: An Emergent Nation?* [New York: Harper & Row, 1969]), he addressed himself to the relevant question of the integrability of the communities which might form a future Federation, i.e., business, agricultural, labor, or elite communities. He did not merely limit the examination, as the book under review does to the potential foreign policy of the yet unborn and perhaps never-to-be-born European nation. Why this title? One surmises that because at the time this book was being written some European statesmen still visualized the purpose of the integration of Europe as the formation of an independent "Third Power," therefore the authors and editors saw as their first duty to reassert the iron laws of interdependence. This limited task the authors have accomplished with perspicacity.

Indeed, the danger of ephemerality inherent in all studies of policy making and especially of foreign-policy making has not affected the Kohnstamm-Hager book very much. On the other hand, in the case of the studies on "The Community and the Less Developed Countries" by Henry Perroy and "East-West Relations" by Michael Tatu, the integrationist aspects of the progress made since 1973 by the Community have proved greater, or speedier, than forecast by the authors. For instance the Lomé convention, too late (1974) to be dealt with in the first of these studies, broke much new ground. The success of the long negotiations was due to: (1) institutional imagination in setting up the intricate integrative mechanism, and (2) the practical ingenuity with which the new Treaty transformed the old relations between the individual Western Powers and individual decolonized territories into a global functional relation between the community and a group of "new states." Similarly, Michel Tatu's hopes, on the eve of the European Security Conference, that the Nine would agree on a common attitude toward the Warsaw Treaty organization were fully confirmed. Moreover, the unity of the Nine rendered the proposal to attend the conference more palatable to the United States. It also facilitated, in 1973–74, the transfer, as prescribed by the Treaty of Rome, of the sovereign right of the individual member states to sign bilateral trade agreements with individual "state trading" [Communist] countries to the Community. This was a significant *"acquis communautaire."*

The analyses by F. Duchêne of "The European Community and the Uncertainties of Independence," by Signor Gazzo of "The Relations between the European Community and the United States" and by Sir Bernard Burrows of "The European Security," share the same premise. The premise is that since the European Community will not become in the future either a superpower or a neutral state, it might be better visualized as an ongoing process conditioned by major relations of interdependence, above all with the United States. Moreover, in the conclusion, Professors Kohnstamm and Hager indicate that "at the present stage of European unity it is first necessary to build a working consensus on the role of the Community in the world" (pp. 254–255). Whereas the short-term collaboration between the U.S. and the European Community has greatly improved since the breakdown of 1973, the prospects of long-term collaboration still require a clear formulation of all the desiderata of an ideal-type European foreign policy, directed, as the Gaullist expression goes, *à tous azimuts.* This could afterward be adjusted and reframed within the possibilities of the indispensable relation with the U.S.A. But for the time being, nobody, not even European public opinion, knows what a "perfect" foreign policy could be for the E.C.

The conclusion also raises, though only at the very end of the book, and too tentatively to be sufficiently noticed, the two most important questions of 1977, especially for political scientists looking at the future of the European Community. When they state that "a broad public debate on the issues is essential. Such a debate would also help the Commission to reflect and interpret European public opinion" (p. 255), the editors touch on the supremely important question of the breakdown of communications between European institutions and European public opinion. The information provided by the Community is kept at the lowest common denominator or, to be precise, at that particularly low level at which all member states in the Council of Ministers and all political groups in the European Parliament can agree on the texts to be released. But then such texts are so innocuous and vague that they scarcely arouse the interest of national and European public opinion. What is needed is information, especially about the new European solutions to the overwhelming socioeconomic problems of Europe, as distinct from those experimented with by the national governments.

For a European public opinion concerned above all with massive socioeconomic problems, the apparent concentration both of the Community and of its information policy on mainly intergovernmental issues seems to be not only irrelevant but also futile. The conclusion rightly asserts that "the Community's capacity to act constructively in the world depends on the last resort on its success in establishing within its boundaries a more united and more just society" (p. 256). But insofar as this is said in, literally, the last line of the book, one can only conclude that what is needed is yet another book starting where this one has prematurely left off.

GHITA IONESCU

University of Manchester

The Forgotten Friendship: Israel and the Soviet Bloc 1947–1953. By Arnold Krammer. (Urbana: University of Illinois Press, 1974. Pp. 224. $10.00.)

Arnold Krammer's book *The Forgotten*

Friendship sets out to review the fascinating story of Soviet and East European relations with the state of Israel during the first years of its existence. In May 1947, the Soviet Union astonished the world by supporting the Zionist aspirations for the establishment and consolidation of a Jewish state in Palestine. Equally astonishing was its ideological about-face regarding the Zionist movement, until then a major focal point of Soviet distrust and criticism. For two years a unique *entente* existed between the Soviet Bloc and the infant state of Israel, with the former constituting the sole source of vital armaments and military training for the embattled Jews. In late 1949, a reverse trend became apparent, with relations deteriorating into what the author calls "a period of indifferent neutrality." Relations reached their lowest point during the East European purge trials, particularly during the 1952 Slansky trial in Czechoslovakia, which took on explicit anti-Semitic and anti-Zionist overtones. On February 12, 1953, after an exchange of mutual recriminations culminating in an explosion of a home-made bomb in the courtyard of the Soviet legation in Tel Aviv, the Soviet Union severed its diplomatic ties with Israel. Relations were resumed only on July 20, 1953, as part of a reappraisal of Soviet policy following Stalin's death.

The period examined in Professor Krammer's book is undoubtedly as important as it is interesting. For its exploration might well have provided meaningful insights into the goals and mechanics of Soviet foreign policy. Moreover, this period, devoid of much serious, comprehensive inquiry to date, is wide open to scholarly research. The need for a thorough investigation of the subject arises principally because a great many of the materials published so far are memoirs or personal accounts written by those (mainly Israelis) who actually participated in the events – recollections which, for all their appeal, tend to be subjective and discursive.

Professor Krammer obviously put considerable effort into compiling factual data. His survey encompasses a wide array of literature including Western, Soviet, East European, and Israeli press and public pronouncements. These, conveyed by the author in a flowing and lucid style, are complemented by three research trips undertaken by Professor Krammer to the Middle East and Eastern Europe in order to examine official and private archives, as well as to interview the surviving participants in the events set forth in the book.

It is perhaps ironic that the wealth of facts and information that stands out as the book's most impressive achievement should also become its major pitfall. Thus, the perceived need to incorporate the vast number of factual details into the discussion overshadows the clear definition and rigorous development of arguments and analysis. Too often, the dramatic and fascinating narrative (particularly so to readers unfamiliar with other accounts of the period) lapses into irrelevant digressions.

The book lacks a central thesis or analytical framework which might have produced a well-structured, comprehensive study that transcended a purely historical survey of events. The author presents the various determinants of Soviet foreign-policy behavior as independent, polarized factors (ideology versus pragmatism; domestic versus foreign-policy considerations; economic versus political factors, and so on). There is no attempt to analyze the interplay among these factors or to determine the relative impact of each factor as it affects the course of foreign policy within the case study. Hence, many conceptual contradictions arise. Moreover, some of the most significant ramifications of the events surveyed are left unexplored. Instead, Mr. Krammer prefers to repeat the standard observations about Soviet political behavior, observations which, for all their generality, are not always implicit in or relevant to the actual findings of his research.

In sum, the book presents us with an extremely rich ore, which, however, remains to be refined.

ILANA KASS

The Hebrew University, Israel

Japanese Radicals Revisited: Student Protest in Postwar Japan. By Ellis S. Krauss. (Berkeley: University of California Press, 1974. Pp. 210. $10.00.)

Several thousand Japanese students were directly involved in various activities related to the great Ampo struggle of 1960 which attracted international attention and was generally regarded as a severe domestic crisis in Japan. As Professor Krauss notes:

> Four years before the Free Speech Movement at Berkeley portended the waves of student protest in the United States, Japanese students had proved the potential power of students and student movements in opposition politics. In Japan itself, the experience of the 1960 treaty crisis was so salient as to create a generation of students who, to this day, are referred to as the 'Ampo generation' and whose leaders' names became household words in Japan (p. 5).

Krauss asks the obvious question: "What has become of these student activists of 1960?" In order to answer this and other related questions, Krauss personally interviewed fifty-three former students of the Ampo generation. In his analysis and interpretation of those interviews, he develops a number of interesting ideas and insights. Unfortunately, he also tends to drift into areas which strike me as being off-target.

More specifically, I find much of the discussion dealing with "political socialization" dreary, repetitious, and unnecessary. While many of the issues raised may be of interest to those who see the study of political socialization as a subfield of political science, I feel that it does not contribute positively to the study's goal of understanding what has become of student activists of the 1960s and the significance of student discontent and protest in the 1970s and beyond. In contemporary Japan, discontent and protest are widespread and the legitimacy of the "Regime of 1955" is being seriously challenged. Workers, farmers, intellectuals, doctors, urban dwellers, housewives, and various other social groups have contributed to the general turbulence of Japanese political life. Placing student discontent and protest in historical context is an important scholarly task, and political socialization can be a useful concept for understanding one aspect of that context. In choosing, however, to define political socialization as "the process by which political beliefs and behavior are acquired, maintained, and changed throughout the life cycle" (p. 16), Krauss makes it a conceptual catch-all which deflects attention from more to less relevant issues. Much of the book seems out-of-focus as a result.

The Japanese version of the ideological turnabout myth, *shūshoku tenkō*, or "employment conversion," has been perpetuated almost without challenge. As indicated by Krauss: "Conventional folk wisdom that student radicalism is merely a passing phase of youth, and the publicity given the dramatic *tenkō* of some former Zengakuren leaders, have together given rise to a widespread belief in Japan about the inevitable fate of the Ampo student activists" (p. 7), – i.e., that student radicals tend to enter large business concerns or government service and shed their radical ideas shortly thereafter. Krauss *does* challenge this myth persuasively but not conclusively. The problem is that he simply does not have the evidence (not that the evidence in support of the myth is any better).

As much as he attempts to ignore the fact, Krauss cannot get around the limitations of "the data" on which he chooses to base the major part of his analysis. One cannot help but admire the effort involved in tracking down and talking with fifty-three members of the Ampo generation. The problem is that fifty-three "anonymous men" (no women were included) cannot be taken as representative of a "generation" unless they are selected according to some generally accepted method. The method used by Krauss is arbitrary, and his sample cannot be taken as representative of anything but itself.

There are other troublesome methodological limitations. Some are inherent in survey research methods but others are a consequence of the unreflective and injudicious use of such methods. For example, in his analysis of the family role in the political socialization of his respondents, Krauss does not question the reliability of their recollections of family life. In other places, Krauss indicates that he has "tested" certain hypotheses. I am not persuaded, however, that his procedures constitute valid "tests."

I find most troubling Krauss's treatment of the Marxist aspects of political consciousness in Japan in general and among his respondents in particular. His juxtaposition of "Marxism" and "humanism," and his effort to reduce the former to four premises and to classify his respondents as "total Marxists," "eclectic Marxists," and "non-Marxists" in terms of their response to interview questions supposedly based on those premises is superficial The "revisit" of the Ampo generation students by Ellis S. Krauss is a fascinating study but seriously distorted by the simplistic treatment of Japanese Marxism.

WILLIAM E. STESLICKE

University of Michigan

A Nation Observed: Perspectives on America's World Role. Edited by Donald R. Lesh. (Washington, D.C.: Potomac Associates, Inc., 1974. Pp. 150. $8.95, cloth; $3.95, paper.)

It is important to recognize immediately the value of this relatively small publication, since the introduction not only does not promise the study to be academic but itself fails to capture the real depth evident in the selections following. The text proves to be a seasoned overview and taken as a unit becomes increasingly significant. The editor states an assumption that readers will have an understanding of U.S. foreign relations which itself recognizes and verifies the level of analysis evident.

The text incorporates six essays, offering the perspective of six experienced foreign observers of the United States, each representing a different view. The majority have had academic affiliations; all have been associated professionally as analysts of U.S. foreign policy. The publishers also commissioned a national opinion poll to test the attitudes of Americans on the strength and importance of the U.S., particularly as evident in sentiment regarding internationalism and isolationism. These findings are recorded in the final section.

A collection of this type has a potential to date itself. This would be underscored had Secretary Kissinger not remained as a central figure following Nixon's resignation. Reference to the Nixon-Kissinger Doctrine remains valid, yielding clarification regarding the dominant role of the Secretary of State. Donald Lesh, the editor, refers to him as the sixth superpower. John Holmes, director general of the Canadian Institute of International Affairs, in a concise analysis, concludes that the Nixon-Kissinger Doctrine is a design for a new era rather than for a single administration. The doctrine is credited with recognizing America's inescapable responsibility for leadership, combined with a plea for a more equitable sharing of burdens, policy making, and responsibilities.

Central to the selections is a recognition of a change in the international system that reflects a limit to U.S. power. The U.S. maintains a unique position, but its growing dependence on foreign resources has altered its bargaining position. While dependent upon the maintenance of a military balance, issues relating to security have made room for those focused on economics. The new international system reflects the U.S. as seeking a managing role, aware of a vulnerability evident in the constitutional crisis during Watergate and the balance-of-payments difficulties which lead to the collapse of the monetary system created at Bretton Woods.

Pierre Hassner, senior research associate at the Centre d'Etude des Relations Internationales in Paris, refers to a Europeanization of American policy in which the U.S. has moved from occupying a superior position in a multilateral structure to a vulnerable, yet sustained, position of initiative, conditioned by a Bismarckian alertness with Dr. Kissinger managing a revised capacity for U.S. leadership.

The dichotomy evident in the selections, confirmed in the opinion poll commissioned for this publication, is that "Middle America," while desiring to reassert a dominant position for the U.S., has moved toward an isolationist view. The U.S. public is increasingly preoccupied with domestic problems. Hans Herbert Götz, Brussels correspondent for *Frankfurter Allgemeine,* notes that the subject matter in foreign and domestic politics has itself undergone change, suggesting the evolution of a world domestic policy. John Holmes asserts further that most of the substance of current international negotiation is now concerned with economic and social issues, within which allies shift positions.

It is evident in these readings that the U.S. is confronted with a redefinition of its role in world affairs. The combination of perspectives presented yields an awareness essential to the contemporary analyst. The text offers more than an observation; it provides an assessment. Beginning with a careful reminder by Georgy A. Arbatov, director of the Institute for the Study of the U.S.A. in Moscow, that Americans are less often considering military might as a basis for foreign policy, the reader is reminded that our transition from empire to nation now involves more commitment to economic considerations. Yukio Matsuyama, political reporter for Japan's *Asahi Shimbun,* concludes that U.S. foreign policy has been dominated by too many lawyers, each staking out a ground of legitimacy giving emphasis to an outdated status quo. The contributors perceive instead a lack of a single appropriate description for the structure of contemporary affairs. Mariano C. Grondona, Argentine lecturer and political commentator, recognizes a new multipolar world in which the U.S. maintains a role as rector. He concludes that what counts now is the effective negotiating power of each country. Within this environment the U.S. is left with a capacity and challenge for leadership, while domestic support remains potentially thin. A text which verifies this is a must in our field. This presentation is an alert overview for foreign policy analysts engaged in assessing U.S. priorities and the significant impact of Secretary Kissinger.

HAROLD S. JOHNSON

Michigan State University

The Soviet Impact on World Politics. Edited by Kurt London. (New York: Hawthorn Books, Inc., 1974. Pp. 312. $13.95.)

This collective work purports to offer an analysis of Soviet policies in all areas of the world, except for Africa. Its several authors differ from each other regarding the basic Soviet motivation: is the USSR engaged in the pursuit of its national interests or is it primarily inspired by the eschatological image of a universal victory of Soviet-like socialism? The editor has no doubts on this score (Preface and Conclusions, pp. vii–viii and 271–300). The

USSR remains for him a revolutionary power committed to the goal of a world revolution; hence, no real detente is possible with it. Brandt's Eastern policy stands condemned in his eyes, because it allegedly opened the door to the Soviet penetration into Western Europe.

Professor Leonard Schapiro of the London School of Economics fully subscribes to the editor's views (pp. 3–21). He advances a debatable proposition that the Secretary General of the C.P.S.U., be he Khruschev or Brezhnev, has always enjoyed the monopolistic power to formulate foreign policy, thus discounting any important role of the Political Bureau in this respect (p. 14). He says rather peremptorily that: "The speculations over dissent on foreign policy inside the Politbureau ... are extravagant" (p. 4). The Soviet Union, committed to "the world of Communist power," pursues the policy of an "aggressive, chauvinistic, expansionist world power" (pp. 6–7). Schapiro advances another debatable proposition that the Party Secretary General may "ignore the advice of his military leaders" (p. 13). He believes that one may extract political concessions from the USSR and China in exchange for trade facilities and credits (p. 14). The failure of Senator Jackson to extract such concessions (free Jewish emigration) does not support this contention. Schapiro too lightly dismisses the studies by Soviet academic specialists on international problems as "nonsense" (p. 15), although he concedes an improvement during the past decade (p. 18).

Professor Adam B. Ulam provides a summary of the history of Soviet foreign policy (pp. 22–45), a sort of resume of his excellent book on this subject. He is inclined to share the editor's outlook, also believing that the Soviet policy of co-existence with the West is nothing more than the old posture of "neither war nor peace" (p. 44).

The subsequent chapters, devoted to regional studies, are more matter-of-fact and less prone to indulge in sweeping generalizations. Professor Hugh Seton-Watson of the University of London writes about Eastern Europe, an area difficult to generalize about because of the different historical and cultural backgrounds of its component countries and also because of notable differences among its Communist regimes. He notes the postwar achievements such as economic development, urbanization and mass education (with the resulting social mobility), which transformed "the peoples of Eastern Europe ... [into] modern European nations" (p. 69). He points out the examples of survival of old national discords such as the Romanian-Hungarian over Transylvania, the Yugoslav-Bulgarian over Macedonia, and the Yugoslav-Albanian hostility. The advent of Communist regimes has not altered the East European problems in this respect, although the alteration of frontiers and mass population shifts made of Poland a homogeneous state for the first time in its history.

Professor Alfred Grosser of the Institut d'Études Politiques competently writes about the French-Soviet and West-German-Soviet relations, although he makes one mistake in stating that West Berlin "is internationally represented by Bonn" (p. 82), this being true only regarding nonpolitical matters. Unlike the editor, he evaluates favorably Brandt's Eastern policy as contributing to the general detente as well as to the greater independence of the Federal Republic which needs no longer any intermediaries in its relations with the Soviet Union and Eastern Europe (p. 89).

Mr. Malcolm MacKintosh of the London International Institute for Strategic Studies offers a historical summary of Soviet policy toward the Middle East (pp. 94–112), stressing the two assets of that policy: the Arab-Israeli hostility, and the American commitment to Israel. He rightly points out the importance of the Suez Canal for the Soviet Navy and Merchant Marine. He is far from wrong in stating that "perhaps the least favorable situation for the Soviet Union would be a peace settlement between Israel and at least one of its neighbors [Egypt?], for this would reduce the indispensability of the Soviet Union for the Arab states" (p. 106).

Professor Harold C. Hinton of George Washington University devotes his attention to the Soviet policy in East Asia, pointing out the unfavorable factors such as the quarrel with Japan over the Kurile islands fishing rights, and the dispute with China. He notes the Soviet success in winning the Indian friendship due to the hostility of both countries to China and the Soviet support of India against Pakistan. He might be right in predicting "a trend toward accommodation [between China and the USSR following Mao's passing] since it is desirable on both sides on a number of practical grounds" (p. 137). One of those grounds of accommodation is the elbow room which China would gain in maneuvering among the three other Far-Eastern powers, the U.S., the USSR, and Japan.

Dr. Guy J. Parker of the RAND Corporation offers his views on Soviet policy in Southeast Asia (pp. 138–155). By the way, he rejects the editor's view that the Soviet main objective is the spread of communism as being "captive of an outlook shaped in the days of the Comintern" (p. 150). He is probably right in thinking that the Soviet ultimate objective in Indochina is Hanoi's supremacy as a counterweight to the Chinese influence (p. 151).

Mr. William J. Bernds of the Council on Foreign Relations concludes his study on South Asia by stating that the Soviet Union achieved in 1971 the status of the ranking power in that part of the world by its support of India against China and Pakistan (p. 169).

Dr. Leon Goure of the University of Miami analyzes Soviet policy toward Latin America (pp. 182–207), evaluating it as "pragmatic and unencumbered by heavy ideological baggage" (p. 193). Yet he thinks that the USSR has not so far achieved any spectacular successes in spite of the rise in Latin-American nationalism with its anti-U.S. sentiment and in spite of the Latin-American inclination to develop economic relations with the Soviet bloc.

Dr. Charles Burton Marshall of The Johns Hopkins University supplies a not very original historical survey of the American-Soviet relations (pp. 211–233). He too disagrees with the editor, saying that: "No one I ever knew or heard of in the U.S. policy nexus would [assume] . . . that the Soviet ruling group would be inclined to sacrifice national interests in deference to revolutionary purposes" (p. 219). He quotes approvingly Jacob Beam, former Ambassador to Moscow, who said that the Soviet leaders "are not revolutionaries any more. They are hard-headed practical people and know that the total Communist victory is not going to happen ever" (p. 231).

The chapter on Soviet military forces by Thomas W. Wolfe (pp. 237–268) supplies very valuable data not only on those forces but also on the Soviet military aid to non-Communist countries such as the Arab states or India.

All in all, the authors of this book have provided chapters of unequal value, the best being those which limit themselves to matter-of-fact expositions and are free of cold-war ideology.

W. W. KULSKI

Duke University

The Angels' Game: A Handbook of Modern Diplomacy. By William Macomber. (New York: Stein and Day, 1975. Pp. 225. $10.00.)

William Macomber, recently U.S. Ambassador to Turkey and formerly holder of top State Department executive posts, including that of Deputy Under Secretary for Administration, states the case in this book for the development of a new breed of Foreign Service Officer: the "manager-diplomat." As a champion of the "Young Turks" (the Washington, D.C., variety of the late 1960s) and the impresario of a notable State Department effort at "reform from within" (the "Macomber Task Force" reports published in *Diplomacy for the 70's: A Program of Management Reform for the Department of State* [1970]), he writes from unusual authority and experience.

The reader who expects to find in this book the details of this attempted "managerial evolution" will be disappointed. Neither diplomatic memoir nor technical treatise, *The Angels' Game* (whose title is derived from Harold Nicolson's observation that the first diplomats were angels – *angeloi,* diplomatic "shuttlers" between heaven and earth) might easily be dismissed as another diplomat's commonplace book, full of aphoristic rules-of-thumb and well-meaning exhortations. The diplomatic practitioner is, for example, told: "arrange things in such a way that the disputes which arise between nations are losable" (p. 70); "always seek to avoid disputes over principle" (p. 70); "no nation should be regarded, or treated, as a permanent enemy" (p. 72); "be certain that policy makers never lead the country into believing that major problems will fade after one last effort, one last battle" (p. 74). The scientific value of such counsel – which, incidentally, shows how easily traditional diplomacy lends itself to a policy of pragmatic détente – is not established by careful specification of the conditions under which it is applicable. The author, sensibly but uninstructively, states: "There are no principles in diplomacy, no matter how sound, that cannot be violated if circumstances warrant" (p. 74).

What makes this book worth reading are the description of the altered institutional environment in which the professional diplomat finds himself and the prescription of methods for him to survive in it. The professional outlook of most diplomats, he argues, has not evolved with the demands of changing circumstances. They "tend to have a far better understanding of what their job used to be than what it is today" (p. 7). Accustomed to "hunch playing" and "lone-wolf activities," which rely heavily on personal "feel" and experience, they become uncomfortable under the requirements of "systematically coordinating large-scale operations" and "managing and being managed by others" (p. 80).

For this they are not entirely to blame. Unlike officers elsewhere in government service, particularly in the military, they receive little training in management early in their careers. As a consequence, they find themselves in important managerial positions – ambassadorships and other high-level embassy jobs – "with almost no previous managerial experience to fall back on" (p. 82).

Paradoxically, Macomber points out, the "management lag" tends to be greatest during "the eras of strong secretaries of state" (p. 88) – one of the few implied *ad hominem* criticisms in the book. The reasoning here is interesting: It is not simply that Secretaries Acheson and Dulles (and presumably Kissinger) chose to rely on their own virtuosity and neglected to develop and utilize the skills of others in the Department. It is that the Department's lesser personnel, basking in the "reflected authority" of the high-flying immortals above them, have let the Seventh Floor's dominance of the Washington foreign policy-making scene substitute for genuine institutional superiority. Their feeling of confidence is thus illusory. Macomber warns his colleagues: "leadership, and the effective and sensible coordination of the whole range of a nation's foreign affairs, clearly require more than a high morale and a combative stance in intramural disputes" (p. 89).

How can the State Department, with its relatively small number of employees, small budget, and small domestic constituency, hope to provide leadership and coordination for all the departments and agencies now involved in foreign affairs? The answers Macomber gives in *The Angels' Game* are sound. The "key to leadership," he affirms, is "having the ideas" (p. 75). The flow of these should be increased by a vigorous "adversary/devil's advocate system" and by greater openness toward "outside ideas" from the academic and other communities. Coordination should be improved by operation of the Seventh Floor as a "collegium" (p. 100) and by expansion of the State Department's liaisons with other departments and with Congress.

If only those the angels were playing with were angels too. . . .

ALAN K. HENRIKSON

Fletcher School of Law and Diplomacy

Kenneth Kaunda of Zambia: The Times and The Man. By Fergus MacPherson. (New York: Oxford University Press, 1975. Pp. 478. $10.75.)

The lives of only a few leaders are completely intertwined in the evolution of their country from foreign domination to independence and in the early years of the new state's independence. Their lives record a significant part of the history of a country and a people. Such is the life of Kenneth David Kaunda, President of the Republic of Zambia. Fergus MacPherson, in his *Kenneth Kaunda of Zambia,* has recorded in one volume the colonial background of the period of Zambia which was the era when the Zambian president was born. He mixes this with facts about President Kaunda's parents and early family life not known to the general public. The book is at its best in recording in some detail the early political life of President Kaunda up to independence. The authors has captured the fast-moving trend that took Kaunda from the African National Congress to the State House in 1964. Professor MacPherson has also managed to catch the special sparkle of Kenneth Kaunda. The readers obtain a sense of Kaunda's personality from this book. While the focus of the book is naturally Kenneth Kaunda, I regret that the author was not able to give more attention to his wife, Betty. Some of us who have closely followed Kaunda's career believe that his wife has had more than routine influence on his political values.

This book is well documented and is valuable for students and others interested in pursuing specific matters in greater detail. It would have been more comprehensive if the author had been able to include a chapter on the relationships of this African leader with leaders in other parts of the world. Kaunda not only corresponds with leaders in all parts of the world but influences them.

In my opinion, history has already guaranteed a place in the hierarchy for Kaunda: in his short lifetime, he has been the founding father of a nonracial state in southern Africa! Now he faces even greater challenges as he uses his considerable skills to find just solutions for the problems of Rhodesia and South Africa. The author was not able to include these subjects in his book because Kaunda's role was just beginning to evolve when he wrote this book. The events in southern Africa in 1974, 1975 and 1976 give every indication that Kaunda may be able to play a historic role in resolving some of the tensions that have plagued this part of the world too long.

MacPherson's work is a first-rate book on a man of considerable nobility. It should be required reading not only for students of African affairs but for all interested in one of the most strategically located countries in Africa. Although several biographies of Kaunda have been published by now, MacPherson's is the first major comprehensive work on Kenneth David Kaunda of Zambia.

THOMAS PATRICK MELADY

Sacred Heart University, Bridgeport, Ct.

The Middle East in Soviet Policy. By R. D. McLaurin. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 206. $13.50.)

This work may best be described as a series of fact sheets about Soviet aims and activities in the Middle East since the mid-1950s, apparently intended, since the research was sponsored by the Defense Department, to introduce non-specialist government officials to the subject. As such, it is not likely to prove of much interest to specialists on the Soviet Union or the Middle East, because the material presented presupposes little more than minimal knowledge of the area, the key actors, and developments since 1945.

The approach is macrostrategic at its least impressive: the Middle East is viewed from a perspective so distant that differentiation, complexity, and the dilemmas faced by the Soviets are barely, if at all, discernible.

There are eight chapters: these introduce the general subject, provide some historical background, examine domestic constraints on Soviet policy, describe the political, economic, military, and cultural dimensions of Soviet activities in the Middle East, and conclude with some observations. Most of the material is rudimentary. For example, the chapter on Soviet domestic constraints on policy toward the Middle East contains short sections on the nationalities, the Party, industrial and defense industry groups, the military establishment, and Middle East government bureaucracy – all covered in four pages under the subheading, "Interest Groups"; also mentioned in the same sketchy, uninformative fashion are "bureaucratic politics," "public opinion," ideology, and regional and international constraints. The chapter on "Military Activities" provides the most solid factual fare, but its pre-1972 vintage makes it a bit stale. It relies on the data compiled by SIPRI (Stockholm International Peace Research Institute) and offers some interesting tables.

The conclusions are generally sound, though they seldom derive from the data that preceded them. Thus, the author notes that "Soviet influence in the Middle East is amorphous: it may be employed with great effect relative to international issues to the extent they are *not* regionally important, but in questions of significance to the Middle East all evidence indicates that local governments continue to make decisions on the basis of perceived self-interest" (p. 147); that the Soviet Union seeks to reduce Western influence; and that it has achieved most of its post-1945 security objectives: "The CENTO alliance is no longer a threat; Iran and Turkey enjoy relatively good relations with their northern neighbor; support for U.S. global initiatives from Middle East regimes is scarce; fewer and fewer ports welcome vessels of the Sixth Fleet; and other strategic forces considered hostile by the Soviet Union have diminished in size."

In light of Soviet policy during and after the October War, however, it is not accurate to say that "the USSR has not shown itself willing to court a risk in pursuit of its Middle East objectives" or that "in the supply of weapons, qualitative limitation has been a consistent element" (p. 148). Moscow is in the Middle East for the long haul and it is not averse to raising the stakes.

ALVIN Z. RUBINSTEIN

University of Pennsylvania

Malawi: Foreign Policy and Development. By Carolyn McMaster. (New York: St. Martin's Press, 1974. Pp. 246. $15.95.)

In a theoretical article still relevant for understanding the foreign policies of new states Robert C. Good argued that state-building is the principal determinant of their external relations. Good said:

> Foreign policy perpetuates the cohesive role of the revolution against colonialism; underscores the existence and integrity of the postcolonial state detached from the identity of its former metropole; enhances the prestige of the national leader at home while reducing the effectiveness of his opposition; and provides opportunities for diversifying the new state's reliance on external assistance. . . . At every point, then, the requirements of state-building impinge upon foreign affairs. For a new state, foreign policy is domestic policy pursued by other means; it is domestic policy carried beyond the boundaries of the state. (State-Building as a Determinant of Foreign Policy in the New States, in L. W. Martin (ed.), *Neutralism and Non-alignment,* N.Y.: Praeger, 1962, pp. 11–12.)

While Miss McMaster appears to be unaware of Good's penetrating insight (he is not cited in either the notes or references) her case study of Malawi's foreign policy represents an interesting, thorough, and particularly well-written "test" of Good's thesis.

Embedded by geography, colonial history, and economic relations within the white dominated southern African region, Malawi is of particular interest because it "was the first black African country heavily dependent upon southern Africa to become independent" (p. 53) and because of the exceptional character and personality of its Life President, Ngwazi ("Chief of Chiefs") Dr. H. Kamuzu Banda.

Miss McMaster's approach is first to describe the political evolution of Nyasaland up to its achievement of independence in 1964 in her first two chapters and then to present five topical chapters and a concluding assessment of Malawi's foreign policies from 1964 to 1973. Topics covered include: the 1964 cabinet crisis that challenged Dr. Banda's leadership; patterns of development assistance generated by Banda's policies; relations with South Africa, Rhodesia, and Portugal; and policies toward Black Africa. Appendices are provided on constitutional changes and economic relations. Detailed and often enlightening footnotes run to more than forty pages, and a useful bibliography and index complete this brief volume.

Malawi: Foreign Policy and Development is a model narrative and descriptive case study of one small, dependent state's foreign policies. Using interviews, newspapers, government publications, and secondary sources Miss McMaster tells her story well. She is particularly fair and sympathetic to Dr. Banda, a very controversial figure. Thus, although McMaster correctly regards Dr. Banda as "a despot" (p. 169), Banda's state-building goals (development and stability) and the foreign policies that flow from them are objectively described, leaving the reader with some degree of sympathy for the dilemmas facing any leader of government in Malawi, even Dr. Banda.

Of considerable theoretical interest is the question whether environmental factors or Dr. Banda's values and perceptions are the prime cause of his policies of increased cooperation with South Africa and its white-dominated allies. In an effective comparison with Zambia and Botswana, which share Malawi's southern African environment but differ from Malawi in external behavior, McMaster convincingly argues that environmental factors must make any leader of Malawi coexist with the neighboring racist regimes but that Dr. Banda's values and perceptions are responsible for the increased linkages with the white South that have been created since 1964.

Thus, this book clearly supports Good's hypothesis that state-building is the major determinant of new state foreign policy and the "pre-theoretical" prediction of James N. Rosenau that in small, poor, and authoritarian states individual and systemic (environmental) variables are most important in accounting for foreign policy behavior (*The Scientific Study of Foreign Policy*. New York: Free Press, 1971. p. 113).

One criticism must be directed at the publishers. St. Martin's Press: at $15.95 for a cloth version this book will not reach the audience its quality merits. A cheaper paperback edition is very much warranted.

PATRICK J. MCGOWAN

University of Southern California

Changing Perspectives in East-West Commerce. Edited by Carl H. McMillan. (Lexington, Mass.: D.C. Heath and Co., 1974. Pp. 207. $14.00.)

An overview of the factors which underlie East-West commercial exchanges, this book provides scholarly explanations of what happened in recent years, why it happened, and what these events meant for the policies of different countries. Edited by Professor Carl H. McMillan of Carleton University, the book includes thoughtful analyses by eleven eminently qualified contributors – eight of whom teach at Canadian and British universities, and three who are researchers in the United States. They focus mainly on changing policy perspectives of the principal trading entities; changing legal, commercial, and financial perspectives; and the impact on Western institutions. The analyses are clear, convincing, and stimulating.

In recent years, East-West commerce benefitted from the quantum jump in negotiating flexibility and in the kind of cooperation not practiced before. The two-way volume of East-West trade increased to a 1974 total of $43.5 billion, or about six times the 1963 level. While the overall U.S. share was relatively small, the growth of trade turnover between other Western countries and the socialist countries during the last decade was spectacular. Emphasizing the import of capital along with the import of commodities, the Eastern countries borrowed heavily in Western capital markets, using the money to help finance sizeable, widening deficits in trade with the West.

The ten chapters of the compendium assess the various Western perspectives on an orderly and fruitful increase in commercial relations with the socialist Eastern countries. They document the substantive change both at the "macro" level of governmental economic policy and at the "micro" level of participating firms and financial institutions.

The contributors show that the expansion in

East-West commerce has been more than a steady flow of export and import transactions between Western firms and the foreign trade organizations of the Eastern countries. Western governments have entered into an intricate network of bilateral agreements with Eastern governments and have created an elaborate structure of implementing machinery. Extending over virtually the entire spectrum of Western technology, more than 1,000 agreements were concluded between Western firms and socialist countries, binding them into longer term relationships known as "industrial cooperation arrangements," and leading to mutual economic penetration on a considerable scale. With business perspectives guiding the Western nations' policies, the large multinational enterprises became deeply involved in the trade transactions and in the cooperative undertakings with Eastern countries. The involvement in East-West commercial exchanges became part of the standard operating procedures for a steadily growing number of firms and organizations, both in the East and in the West. The impact of these measures on Western institutions is the subject of the last two chapters.

While it reflects the contributors' command of the controversial issues involved, the volume pays little attention to East-West trade in agricultural products – despite the fact that agricultural commodities dominate the United States' and Canada's commerce with Eastern countries, and inflamed passions fuel the vigorous public controversy surrounding the agricultural export issue in the United States. Large segments of American farmers perceive agricultural exports to Eastern countries as a significant factor in world commerce, and get upset when trading opportunities are tampered with. Perhaps agricultural trade issues are so complex that they require special treatment.

Anyone sincerely concerned about the changing perspectives in East-West commercial relations should welcome this valuable overview. Moreover, the erudite explanations are of importance to members of government and business communities, as well as to scholars.

JOSEPH HAJDA

Kansas State University

Labour in Irish Politics, 1890–1930. By Arthur Mitchell. (New York: Barnes and Noble Books, 1974. Pp. 317. $12.50.)

This study traces the rise and fortunes of the labor movement during a time of turbulence and in a location where constitutional issues have tended always to throw a long shadow over political beliefs and distort any rational pattern of voting among the working-class people. Dr. Mitchell offers an account which is always scholarly yet eminently readable. There are clear signs that the work was originally a thesis, but it has been updated and the many documentary sources cited are supported by reference to twenty interviews with prominent Irish personalities undertaken over a period of eight years. Ths study is lucidly presented, and the reader obtains a vivid impression of the role of Larkin, Connelly, Griffith, Thomas Johnson, and other key figures in the Irish labor movement.

One can, nevertheless, take issue with the author on certain points. The dates chosen seem, in a sense, almost arbitrary, in that many of the events leading up to the struggle described took place before 1890 – and indeed continued after 1930. As one who has lived for the past seven years in Northern Ireland, I consider that the North and its contribution has, in places, been somewhat scantily treated, especially because Belfast had become such a great industrial center at the end of the nineteenth century, as the author recognizes on p. 285. The conclusion, while adequate, is perhaps somewhat too concise and generally a little disappointing in view of the wealth of facts amassed and presented in Chapters 1–11. The alphabetization of names beginning with 'Mc' is unusual, both in the bibliography and the index.

The total study undoubtedly makes a contribution to the investigation of this crucial period in Irish history. Despite the date of publication, there is no direct reference to the fact that the Labour Party is now in power in the Republic of Ireland, albeit in a coalition with Fine Gael. If, when Dr. Mitchell refers to "the untried alternative of socialism," he means more radical labor policies and success than this or a party holding a majority in its own right, then he is likely to remain disappointed, for Ireland – despite some posters which greet one in Dublin – is basically (and in many senses of the word) a conservative state.

Notwithstanding these grumbles, this study is clearly the product of thorough research and will certainly be of value to lecturers and students in the field of twentieth-century Irish political history.

A. MALTBY

Queen's University, Belfast, Ireland

Democratic Campaigning in Venezuela: Caldera's Victory. By David J. Myers. (Caracas, Venezuela: Fundación La Salle de Ciencias Naturales, 1973. Pp. 259. 20 bolívares.)

It is easy to be superficially critical of this careful work, for its title misleads. It contains two different volumes, each short and experimental, with little linkage between them. The title describes only the first; many readers possibly will feel the second, which is simply titled "Epilogue," is the stronger. Myers has subordinated an impressive anecdotal knowledge of Venezuelan political history and current issues, either in the first volume's application of a strategic game framework that adapts work by Richard Snyder, or in an analysis of six cleavages in the second based on work suggested by Seymour Lipset and Stein Rokkan. As a whole, the book is not designed for a beginner; it contains little systematic instruction about the problems that have moved the country since the present system was established in 1958. Throughout, the impression is that Myers reined himself in. I wish he had not been so restrained, for eventually the restraint harms the work.

The first volume sets its material into an overarching frame, applying to the 1968 quinquennial general election a comparative analytical pattern that Myers feels has been given only limited use in the literature of comparative politics. Much of the construct is his own. He argues that the most important issues to be discussed are the institutional setting, the rules of the game, the nature of the participating units, strategies and tactics, stakes, and information. He examines each category in the light of its own subsets, each in its own chapter complete with illustrative material. A concluding chapter incorporates summaries of the votes cast and recapitulates the application of the matrix to the available information. In concluding, Myers suggests leads for research in a much too brief offering of implications for the future.

The second volume is subtitled "Structural Cleavages, Parties and Party Systems in Latin America: The Case of Venezuela." Myers again develops his own structure of operational theory, building his strongly quantitative analysis around six structural cleavages: center-periphery, clerical-anticlerical, traditional-modern, urban-rural, owner-worker, and poor-others. Each cleavage is described in general terms, and with some specific references to Latin America, and is then applied to the Venezuelan case. The census of 1961 supplies socioeconomic data by *municipio,* the smallest political subdivision; each of these 642 units is relatively homogeneous, although in the less-settled ones there may be some dichotomizing of attitudes. Setting up the materials for multiple regression analysis, Myers draws on the census data for independent variables, while the voting statistics become dependent variables. Separate regressions are run for each of the more important parties in the 1958 and 1963 elections. The analysis tests the validity of hypotheses that initially governed the drawing of proxies from the socioeconomic data. An extension of simpler calculations allows judgments to be made about the other smaller parties, and about the coalitions that were organized in these two elections.

The second volume is concluded by extended comments on the 1968 election (pp. 227–240). In some respects this is the most useful portion of the volume, for it applies the hypotheses to the 1968 voting data and offers some predictive suggestions for the 1973 election. But the conclusion also is frustrating on several counts. My earlier criticism, that the two sections are not knit together, here gains force because Myers pays so little attention to the issues that give shape to the structural data.

The 1973 election demonstrated that some of Myers' less likely possibilities were actually more likely. He had suggested that the most-voted parties (*Acción Democrática* received 48 per cent in 1973, the Social Christian COPEI received 37 per cent) are probably viewed by Venezuela's mobile and modernizing voters as old-fashioned and of decreasing relevance. He also suggested that the heated 1968 campaign may have cost the system legitimacy. The 1973 election outcome actually suggested the reverse. It seems logical to suggest that issue examination would have helped Myers' study; the 1973 results may have been perceived as a continuation of trends rather than a reversal.

Myers is descriptively very helpful. An appendix contains biographical notes on many principal actors. Maps and tables abound, and an early chapter sketches the institutional history of this century. Footnote citations, especially in the Epilogue, are exhaustive.

The careful balance of this work sets it in clear and advantageous contrast to several earlier volumes. For example, the heavily funded work by Frank Bonilla and Jesús Silva Michelena (appearing as two volumes of a three volume set, *A Strategy for Research on Social Policy* [Cambridge, Mass.: M.I.T. Press, 1967–70]) fell victim to their ideologies. Myers sets scholarly criteria for nearly all comparative analysis.

PHILIP B. TAYLOR, JR.

University of Houston

Collective Security and the United Nations: A Definition of the UN Security System. By M. V. Naidu. (New York: St. Martin's Press, 1975. Pp. xv, 164. $12.95.)

In this brief volume, Professor Naidu seeks to clarify the concept of collective security, to demonstrate the inappropriateness of that concept in discussing the theory and practice of the United Nations, and to suggest other terms which he feels are more applicable to the UN. Unfortunately, students already familiar with the existing work on collective security, especially that of Inis Claude and Ernst Haas, will find little of additional value in this new book.

The book begins with a familiar review of the varied, imprecise, and frequently contradictory uses of the term collective security. It then defines the "ideal" type, essentially following Cluade's definition; suggests the "analytical components" involved; and presents the "subjective" and "objective" conditions necessary for the successful operation of the ideal type as defined. While some of this discussion is useful, very little of it goes substantially beyond the existing literature.

The third and fourth chapters deal largely with the United Nations. The security system of the UN Charter is seen as an advancement over that of the League of Nations, but as falling short of collective security. The "most serious procedural limitations" of the UN system are "the voluntary nature of the Special Agreements through which the military forces for the United Nations are to be created, the right of individual and collective self-defense, the lack of a definition, and the right of veto given to the Permanent Members of the Security Council" (p. 62). Moreover, says Naidu, by failing also to meet the objective and subjective prerequisites, the actual practice of the UN has departed even further from the ideal type.

Professor Naidu suggests the term "collective measures" as most useful in characterizing the UN security system. This involves three kinds of activities: "collective military measures (the Korean type), collective police measures (the UNEF and ONUC type) and collective peacekeeping measures (the UNOGIL type)" (p. 76). All of this seems reasonable enough, although the definitions provided would not be easy to apply. Professor Naidu then goes on to distinguish these collective measures from preventive diplomacy, pacific settlement, and economic and diplomatic sanctions. His distinctions here, especially with regard to sanctions, are sometimes unclear. Also, the reader is left without an explanation of why collective measures are part of the security system of the UN, while these other instruments apparently are not.

The concluding portion of the text is essentially an extended summary, ending with an expression of the author's hope that acceptance of collective measures by the world community will lead to "an ultimate acceptance of the *ideal* type of collective security system" (p. 91). No attempt is made to suggest the processes by which the linkages here might come about.

The final summary is followed by forty pages of appendices, including excerpts from the League Covenant and the UN Charter, portions of two reports by Dag Hammarskjold and an address by U Thant, the texts of several General Assembly and Security Council Resolutions, and the text of the agreement between the UN and the Government of the Congo concerning ONUC. This is a limited but useful collection of documents.

While the book is generally accurate, it does contain occasional factual errors. At one point, for instance (p. 98), Naidu says that the Security Council declared China an aggressor in Korea. This action, of course, was taken by the General Assembly. A more glaring inaccuracy is the following statement: "In that tiny strip of a country, Vietnam, more persons have been killed, more land and property has been destroyed than in the entire Second World War" (p. 55). Such an extreme error should certainly have been caught before publication.

Overall, then, the book does not have a great deal to recommend it. Some worthwhile distinctions are made and some useful (but not unusual) documents are gathered in a single place. There is little, however, that is original and much that is obscure.

EDWARD THOMAS ROWE

University of Denver

The United States and Britain. By H. G. Nicholas. The United States in the World: Foreign Perspectives Series. (Chicago: University of Chicago Press, 1975. Pp. 195. $10.00.)

Publishers – especially, it seems, academic publishers – are incurable optimists. In recent years they have persuaded themselves that there is a real demand for "short" books on almost any aspect of American involvement in world affairs.

The books in this series propose to examine American relations with other countries through the perspective afforded by non-American scholars – in this instance the Rhodes Professor of American History and

Institutions at Oxford University. It is not a book for the specialist, and should not be judged as such. At the same time any author who accepts such a challenge wants to do more than merely provide undergraduates with a shortcut to "knowledge." And H. G. Nicholas clearly has such ambitions.

In places, he succeeds. Despite a tendency to write at relatively great length about relations during war crises at the expense of other aspects of the Anglo-American connection (ten pages for the Civil War, but only nine for the entire "imperial" era from 1895 to World War I), Nicholas covers the territory well, and even dots the landscape with new insights not found in either the "standard" works or in specialized studies. He is at his best in the sections that deal with Anglo-American relations from 1776 through the Civil War and in those that pick up the story from 1939 to the present. He is weakest in the middle section, where the reader is surprised by the absence of any reference to Anglo-American differences over Far Eastern policy or, stranger still, over the Mexican revolution. It is somewhat surprising, too, after all the recent monographic work on the Wilson Administrations to find the author of an up-to-date synthesis still treating Woodrow Wilson as a previous generation of scholars did – i.e., as a disembodied idealist who floated about two feet above the ground, dispensing justice to all the world without a concern for America's true national interests.

While seldom straying very far from what might be called low-church orthodoxy, Nicholas nevertheless poses some real teasers. He suggests, for example, that Churchill's decision to send aid to Soviet Russia after Hitler's attack may have tipped the balance in favor of similar aid from the United States. Nicholas also makes an interesting case for British leadership in meeting the Soviet "threat" in the early days of the cold war. A Bevin-Churchill combination in the United Nations and at Fulton, Missouri, he implies, helped to overcome a strong disposition, "especially in the State Department, to keep outright confrontation with Russia to the minimum" (p. 115). That assertion will no doubt raise many eyebrows. It was a different story altogether, however, when it came to the cold war in the Far East. There British and American interests diverged, right from the time of General MacArthur's shogunate to Johnson's War in Vietnam. "Britain's overwhelming concern, second only to that of not getting involved herself, was to prevent the U.S.A. from becoming bogged down in a hopeless and costly struggle that would divert her attention, her will, and her primary tasks, as Britain saw them, elsewhere (p. 167).

Trying to explain Anglo-American differences over Asian policy, Nicholas remarks that Washington's sense of mission began to resemble the self-conceived role of the British raj in India. But where Britain had remained flexible out of necessity, in order to protect real economic interests, America's "Pacific Firsters" did not have that advantage. Lacking any "strong occupational or interest group base," the "Pacific Firsters" attracted the "Frustrated and Disaffected, the adventurers and the also-rans of every kind" (p. 133). Isolationism, denied its traditional role in the postwar world, "found a substitute existence for itself in a concern for America's place in the Pacific and in Asia, an area where the tentacles of Europe did not reach and the demands of allies were less vocal or persistent" (*Ibid.*).

After Vietnam it is hard to respond to arguments that stress the irrational, yet Nicholas himself points out that there was more to the American raj than misplaced idealism. "By the 1960s the U.S.A. was providing directly about half the capital required for investment in the developing countries of the Commonwealth, apart from the substantial provision made by the United Nations and its related agencies, most of which was ultimately traceable to the American Treasury" (p. 162). Perhaps both explanations are correct. At any rate, it is a problem more for the reviewer than the intended audiences.

LLOYD C. GARDNER

Rutgers University

Deutschland und der Kalte Krieg. By Ernst Nolte. (Munich: Piper, 1974. Pp. 760. DM 64.)

This is a large book: large in size, large in conception, large in shortcomings. The strengths and weaknesses of this work stem in good measure from its being essentially an effort in the history of ideas. Professor Nolte, well known as the author of books on fascism, applies a phenomenology of ideas to the foreign policy conduct of the major actors he deals with in this study: the United States, the Soviet Union, Germany. This method is Nolte's answer to what he calls the question about the "basic character" of the cold war. In raising this question, Nolte suggests

> All other aspects can be pulled together, and the possible answers range between the narrowest possible – [the cold war seen as] a conflict, clothed in ideology, carried on from 1947 to 1953 between the only remaining world powers, the United States and the Soviet

> Union, over European territories not unambiguously located in their respective realms *(zugeordnet)* – and the widest possible – [the cold war seen as] an ideological and power-political struggle (begun in 1917, and incipiently in 1776, extending into the unforeseeable future) about the future shape of a unitary world among several militant universalisms, of which each is in the possession of at least one great state (p. 39).

Nolte leans toward the "widest possible" end of the spectrum: "The Cold War belongs to the species of struggles between states or groups of states that are totally or partially determined by ideology. The narrowest possible definition is, therefore, to be excluded" (p. 45). This ideological characterization of the cold war, qualified as it is by the term "partially," leads Nolte to an examination of the intellectual and political "prefigurations" of the eighteenth and nineteenth centuries that he sees as ultimately culminating in the cold war. Early chapters in the book deal with "The European System, Its Left and Right"; "The First State of the Left: The United States"; "The Most Successful Left: Marxism"; "Marxism as a State: The USSR"; the early encounters between the United States and the Soviet Union; fascism; the dealings among the Soviet Union, Germany and the United States prior to World War II. From there, Nolte turns to an overview of the uneasy wartime coalition between the Western Allies and the Soviet Union; the "preparatory" phase of the cold war in the immediate postwar years; the Berlin Blockade; the height of the cold war and the partition of Germany; the origins of the Federal Republic and the GDR; the Korean War and the debate within West Germany about rearmanent; the Twentieth Party Congress and its meaning for Soviet foreign policy; the two German states as dual focal points of the cold war; Kennedy's "new realism" and the Berlin Wall; the Cuban missile crisis; the Test Ban Treaty; and finally the "détente" phase of the cold war, as exemplified in the nonproliferation treaty, Bonn's *Ostpolitik*, and the Strategic Arms Limitation Talks. In addition, at more or less suitable junctures, Nolte interjects short chapters that deal with "illuminating parallels" (Japan, Austria, China, Korea); "parallels and contrasts" (Israel, Vietnam); "contrasts" (the Arab states, North Vietnam, the People's Republic of China); the renaissance of the Left in the West; John Kennedy, Lyndon Johnson and Vietnam; self-determination in the Third World; the potential for a "cold war" between the Third World and the industrialized states; and many other topics.

In order to deal successfully with such varied and intractable materials an author needs plausible organizing principles. For Nolte these principles derive, by and large, from the intellectual and ideological climate of the times. Since ideas, and especially ideological systems, develop and mature slowly, Nolte's examination reaches back to the Puritans and the Founding Fathers, the origins of Marxist thought, the predictions made in the nineteenth century about a future American-Russian conflict, and so forth. Some of these early chapters make for interesting and profitable reading. Nolte often succeeds in unearthing intellectual precedents and traces them along their (sometimes) tortuous paths until they illuminate a point he wishes to make; he condenses skillfully, he has an eye for the telling detail, and his style of writing is generally readable.

But one remains uneasy. The "prefigurations" and their modern ideological and political manifestations traced by Nolte are often murky and appear somewhat contrived: in dealing with ideas anything that is said tends to do injustice to things that are left unsaid. In the case of the United States, for example, it is very doubtful whether there exists an intellectual tradition (not to speak of an ideology) that extends over 200 years and that has demonstrably and continuously affected American foreign policy. Nolte's treatment of the United States prior to World War II appears to be an almost perfunctory exercise, shot through with unexplained contradictions, and seemingly uninformed about the work of such historians of ideas as Louis Hartz, Daniel Boorstin, David Potter, and many others who have dealt with the impact (or lack of it) of "ideology" on American foreign policy.

When Nolte turns to the cold war years, his eclectic methodology leads to even more serious omissions, and hence distortions. Despite the wide range of topics dealt with in the book, there is no sustained discussion of military-strategic matters; of economic and monetary arrangements and their imperial implications; or of European integration and its function for German recovery, for the cold war in general, and for shaping an alternative to existing political ideologies. Even in chapters that connect ideological "prefigurations" with cold war perceptions and attitudes, Nolte's argument tends to run parallel with historical reality and does not intersect with it, leaving unexplained how and why critical foreign policy decisions were made. In these parts of the book it isn't so much that one feels compelled to disagree with what is said; rather, one wishes to be shown if and where the world of thought and action meet and merge. (An example: Nolte devotes considerable space to what is generally a balanced assessment of the development and revisions of Marxist-Leninist thought, but he shies away from dealing concretely with some of the central elements of Marxist thinking – national and international economic processes and socioeconomic configurations – as they exist in the "real" world.) Nolte's analytical approach is somehow disarming because normal standards of criticism seem inapplicable: questions of "correct" or "false" statements, or even interpretations, cannot properly arise, since very often one plausible generality is being contradicted by another plausible generality.

There is no reason to argue with Nolte's basic contention that ideas do matter in the conduct of foreign policy. But an approach which denigrates so insistently the dictates of circumstance, of power politics, of power economics in favor of the dictates of thought and ideology, should be more responsive to its own demands and possibilities. Perhaps that would require turning a set of ideas into a paradox. For it is at least conceivable that the wielding of power develops its own ideology, impervious to correction and immune to any dialectic except its own; and that imperial (as well as bureaucratic) compulsions derive as much from opportunity as from design.

WOLFRAM F. HANRIEDER

University of California, Santa Barbara

South Africa in Africa: A Study of Ideology and Foreign Policy. By Sam C. Nolutshungu. (New York: Africana Publishing, 1975. Pp. ix, 329. $20.00.)

"South Africa's policy towards the rest of Africa was a fundamental failure despite its apparent successes . . . Africa was not made safe for *apartheid*" (p. 298). This is the theme presented by a black South African political scientist in his lucid but lengthy analysis of the white regime's ambivalent response to decolonization in Africa. The volume is the latest attempt to describe, "explain and interpret" (p. vii) South Africa's foreign policy. The analysis is sensitive but inconclusive; it is only a modest advance on previous investigations, and it does not represent cumulative scholarship. It is a work of interpretation rather than a novel explanation or a theoretical presentation.

The book has two parts which adopt different levels of analysis, moving from national policy formation to regional relationships. The first section examines South Africa's policy toward colonial Africa and concentrates on its "great men," especially on Smuts, Malan, Louw, and Verwoerd. The second half analyzes South Africa's regional interactions with the states of southern Africa, both black and white. Nolutshungu also deals with domestic constraints and changes within South Africa, especially with the evolution of its ideology. In particular he discusses white ethnic politics, characterized by "the growing emphasis on *Realpolitik* in South Africa and the supersession of some Afrikaner racial sensitivities by the ascendant principle of State interest" (p. 126).

The strengths of Nolutshungu's analysis are in his attention to detail and in his mastery of the subtleties of African international relations. The weaknesses of his approach, however, lie in his somewhat uneven and abstract style and in his propensity to tackle some rather obscure theoretical issues. Nevertheless, despite such unnecessary academic trimmings and the author's occasional tangential detour, this book does point to many of the contradictions and continuities in South Africa's interactions with Africa. In particular, it documents South Africa's continual but hopeless advocacy of a variety of economic and military regional organizations. It also provides insights into the dynamics of regional politics, especially into the coexistence of conflict and cooperation in southern Africa. Nolutshungu usually adopts a dispassionate and objective mode of analysis which emphasizes continuities and constraints. Sometimes, however, he slips into a more critical form of inquiry in which he examines South Africa's "sub-imperial" role (pp. 171 and 302).

The author's focus on ideological conflict is not always sustained, and he remains concerned with economic and strategic factors; he does not confront alternative explanations and is generally uncritical about the economic forces behind South Africa's racist ideology. Moreover, his regional focus tends to reduce the importance given to South Africa's isolation in world politics and to its dependence on external trade and technology. The book contains several typographical and factual mistakes [Lesotho has *not* exchanged diplomats with South Africa (p. 89)]; and the tables and bibliography are rather dated and abbreviated.

Nevertheless, this volume deserves to be read by students of African and international politics; it is not a definitive work, but it does contain information and insights not readily available elsewhere. In particular, Nolutshungu discusses factional politics in South Africa in response to international crises and to a changing African environment. He also starts a useful comparative analysis of the foreign policies of the black states, especially examining the different stances of Botswana, Lesotho, and Swaziland, and of Malawi and Zambia toward South Africa. His analysis of factionalism over Malan's African Charter and Vorster's "outward-looking policy" are informative; and his investigation of the "linkage politics" of conservatism in Black Africa are perceptive and suggestive (pp. 279–286). Despite the several limitations of his substantial manuscript, Nolutshungu does identify the major contradiction in South Africa's search for regional hegemony: "If the quest for influence in the continent arose out of a desire to facilitate the retention of white privilege within South Africa, white minority rule itself was the principal obstacle to South African policy" (p. 1).

TIMOTHY M. SHAW

Dalhousie University, Nova Scotia

Integracion Andina. By Edgar Camacho Omiste. (La Paz: Editorial Los Amigos del Libro, 1975. Pp. 254. Price unknown.)

In recent years American political scientists have begun to recognize the utility of a political economy approach to political problems. Nowhere is this more evident than in the analysis of international phenomena. The interdisciplinary perspective adopted by Edgar Camacho Omiste, a Bolivian scholar and diplomat, in his study of Andean integration will thus be of some interest to the American academic community.

For Camacho, "to study an historical event from a predominantly political perspective, signifies that one is examining it in its intimate links with the nature of the State, the relation of classes which gave it rise and sustain it. . ." (p. 43). He shares the view put forth by Marxist theorist Nicos Poulantzas that the state reflects the *political* interests of dominant classes in the society (p. 230). From this perspective, regional integration can be seen to form part of the domestic political strategy of the governing social forces. Yet as Camacho points out, the Andean Group initially had the support of diverse political groups. How and why did this support come about?

"The principal barrier to unity and commerce between neighboring countries," Camacho argues, "is the prevailing international economic structure and the world division of labor which determines in large part the social and political conditions of each of the countries individually and of all of them as a whole" (p. 46). Most notable among these conditions is the distorted pattern of Latin American industrial development characterized by a high concentration of production in consumer goods, small scale, inefficient production and little technological assimilation. Those who manage this sector have become a ruling class "comfortable in their role as a protected minority shareholder" and have thus never developed into a dynamic national bourgeoisie capable of leading their countries toward self-sustaining and independent development (p. 87). To preserve their position, the state provided them with the necessary high protective tariff barriers.

In the 1950s the Economic Commission for Latin America (ECLA) advocated regional integration as a means to overcome the narrow markets of most Latin American countries and improve possibilities for further import-substituting industrialization. The Central American Common Market and the Latin American Free Trade Association were modeled on this approach. According to Camacho, integration in these two systems, however, did not promote development. Rather, it increased reliance on direct foreign investment and provided a windfall for multinational corporations who were the only actors capable of taking advantage of the opportunities offered by the enlarged, protected market. Because the net beneficiaries in these two systems were the multinational corporations, it became commonplace for radical critics to dismiss integration as a strategy for dependence rather than development. Camacho, however, argues that integration is a strategy which can be differentially used for conservative or progressive ends depending upon the type of state initiating and carrying it out (p. 231). But if an integrative system is to function as a progressive strategy, it must meet certain minimum requisites.

To overcome the problems of dependence, an integrative system must stimulate the development of technology within nationally owned industries and must regulate the process of direct foreign investment. In addition, where an equitable distribution of benefits is not forthcoming because of unequal levels of industrial development among the member countries, Camacho strongly advocates mechanisms to insure a more equitable distribution of benefits. The principal mechanism is joint industrial programming. Camacho thus explicitly rejects the argument that within the capitalist mode of production, balanced development is impossible and that the system will always be characterized by disquilibria among industrial sectors, among regions within a country, and among countries (p. 142).

The Andean Group embodies each of the minimum requirements for integration sketched above. It accepts the need to distribute equitably the benefits derived from integration and has an elaborate system of industrial programming in process. In Decision 24 of the Junta del Acuerdo de Cartagena there is a measure to stimulate technological development and control the process of direct foreign investment. In discussing this unique common code, however, Camacho argues that it "established that foreign firms would have to transform themselves gradually into national or mixed entities within fifteen years, extended to twenty in the case of Bolivia and Ecuador" (p. 175). Yet whether divestment is required of *all* foreign firms both old and new is hotly contended within the Andean Group today.

In his conclusions, Edgar Camacho Omiste returns to the theoretically interesting question of why diverse groups and classes lent their support to the Andean Group. His answer, however, presents us with something of a paradox. The extent to which multinational corporations had penetrated the Latin American political economy and the growing integration of Latin America "with the hegemonic centers of the capitalist system," he argues, were becoming obvious to the "underdeveloped industrial bourgeoisie" in these countries who saw such processes as tending to result not only in their absorption but in their very destruction. "Under such conditions, the viability of these social classes and of the national states that represent them would appear to depend upon the modalities of their insertion into the world scene" (p. 225). Integration, then, has a clear utility. Yet this argument assumes that the "underdeveloped industrial bourgeoisie" indeed favors a progressive form of regional integration. It does not explain why the bourgeoisie behind the Bolivian state, for example, sees Andean integration as a means of maintaining its dominant position – witnessed by their joining the Andean Group – but Bolivian industrialists have gone on record as opposing a mechanism like Decision 24 which could, according to Camacho, be an instrument for insuring their survival. Camacho has pointed the way for others to plunge deeper into the analysis of such contradictions.

LYNN KRIEGER MYTELKA

Carleton University, Ottawa

International Marine Environment Policy: The Economic Dimension. By Charles S. Pearson. (Baltimore: The Johns Hopkins University Press, 1975. Pp. 127. $9.00, cloth; $2.65, paper.)

Can human beings survive on a planet with a dead ocean? This book does not attempt to provide a scientific answer to that question, but it is concerned with programs and policies that may help future generations avoid having to live with that threat made real. The book, in brief, asks and seeks to provide answers to the basic questions concerning the maintenance of ecological stabilities of the world ocean. The author examines the nature of the problems facing the ocean environment today and in the future and describes and evaluates some of the major collective actions undertaken by global and regional groups to try to cope with them. The problems are approached mainly from an economic perspective, although political factors are also discussed.

Without question, protection of that great commons, the world ocean (look at a globe and you can see that all of the world's oceans are linked together) ranks high or highest in the order of environmental values and priorities. There is abundant evidence, however, that human beings in this technological age are polluting the ocean at a rapid pace, but how dangerously is an open question that United Nations' monitoring and research programs are attempting to discover.

The author, who is a professor in the School of Advanced International Studies of Johns Hopkins University, begins by making a case for governmental intervention. But the difficulty of dealing with marine pollution is that in many cases the problem – or at least the solution – is global or regional, whereas government, in the usual meaning of that term, is not. The author also demonstrates that private enterprise, operating within a market economy, is not suited for the job. In the absence of national governmental and private solutions, the conclusion is obvious that only cooperation and consensus engendered through the means provided by international organizations – public and private, regional and global – are available to meet the challenge.

Can international organization do the job of meeting the future challenges that most indicators predict will be staggeringly difficult as we near the 21st century? The author's review of what has transpired thus far during the 1970s to cope is partly encouraging and partly discouraging. The 1972 U.N. Conference on the Human Environment (Stockholm Conference) was a major motivating force, and the Declaration of Principles and Recommendations that was contained in its Action Plan clearly prescribes what states must be willing to do to avoid ocean catastrophe. Creation of the United Nations Environment Program (UNEP) with its subsequent operational activities spanning the globe was a major step forward. Numerous international lawmaking treaties have also been concluded, but few have been put into effect with sufficient ratifications. Two are of particular importance, and the author has rightfully devoted a chapter of his book to each. The Convention on the Prevention of Marine Pollution by Dumping of Wastes and other Matter (Ocean Dumping Convention) was agreed to in November 1972, and the following year the International Convention for the Prevention of Pollution from Ships was concluded. Yet these conventions and the problems they seek to solve are only peripheral to the real pollution problems that are national in origin. The author recognizes this situation in noting that "the oceans lie below the continental land masses, and the laws of gravity make them the natural sinks for societies' wastes" (p. xi). Related, of course, are the difficult problems of achieving political agreement that will limit national actions within sovereign boundaries, and the failure to date of the world community to agree on the limits of national jurisdiction and the internationalization of the ocean and ocean floor beyond.

The book, although brief, is a valuable contribution to the growing literature in the ocean-environmental field. It contains a wealth of informative statistics in a number of tables. The author, however, fails to describe the important informal enforcement roles carried on by international agencies, such as the voluntary compliance with IMCO rules by shipping companies which results in lower rates charged by insurance companies like Lloyds of London that classify ships for insurance purposes. On balance, however, the book is a significant contribution to a field that will continue to increase in importance.

JACK C. PLANO

Western Michigan University

A Memoir of China in Revolution: From the Boxer Rebellion to the People's Republic. By Chester Ronning. (New York: Pantheon Books, 1974. Pp. 306. $10.00.)

Chester Ronning's life has included several careers. His father came from Norway, graduated from theological seminary in Minnesota, and became a missionary in Fancheng on the Han River in Hupei province where Chester was born in 1894. He grew up speaking Chinese with his schoolmates. His family escaped the Boxer year by the accident of being on leave in Norway and the United States. Accustomed to speaking Norwegian with their father, the Ronning children spoke English with their mother. After she died in 1907, Chester and his brother became Americans at school in Iowa, but soon they joined their father, who had bought land in Canada, and the whole family became Canadians. Chester really grew up in Grande Prairie where he soon aimed to become a rancher, but to make a living he had to start teaching. Once he got through a Lutheran normal school, he found himself a sapper and then an air cadet in World War I, although the war ended before he saw combat. So in 1921 at the age of twenty-seven he went back to China, had a year of language work in Peking, and became principal of a middle school in his home city of Fancheng. This was his first experience of revolutionary China – in the 1920s when the Nationalists and their Kuomintang were coming into power with a new regime and much hope. He saw something of the capacity of Chinese students to organize in protest against imperialism.

This first phase of his international experience was ended in 1927 by the missionary evacuation from China. Chester Ronning found himself back at the Lutheran college in Canada where he had learned his teaching, and for fifteen years (1927–1942) he served as a college president. Thus his career in the international world caught up with him only when he was almost fifty. In the war against Japan he was in the Canadian Air Force intelligence intercepting Japanese communications. Only in 1945 did the Canadian Department of External Affairs advertise for a Chinese-speaking assistant to the Canadian ambassador in Chungking. Ronning was asked first to be the examiner, but after VJ day he was sent to China as first secretary of the embassy for a trial year. From there on his career broadened out. He became indispensable in the embassy in China, acting sometimes as chargé. Later he returned to the Department of External Affairs in Ottawa, went as ambassador to Norway and then as high commissioner to India, and served as acting head of the delegations from Canada to the Geneva Conferences on Korea in 1954 and on Laos in 1961–62. Obviously, for those who are prepared with a multilingual, multinational background, the troubled world will have a place.

His book is a memoir in the proper sense, a reminiscence of various phases of his career. There are chapters on ancient China's collapse, on the 1911 revolution and the split in the united front of the 1920s ("the 1927 disaster"), and on the Communist long march and establishment at Yenan. These chapters make plain that Mr. Ronning had from the first been sympathetic to his revolutionary students of the 1920s and to the real and urgent problems of the Chinese populace. Going to China in 1945 he ran into the Communist Party leader Tung Pi-wu in Calcutta, on his way back from the founding of the United Nations at San Francisco. Having left China just as the Communist-led social revolution was suffering betrayal doubly at the hands of Stalin and of Chiang Kai-shek, Mr. Ronning returned eighteen years later to find the revolutionary movement in full flower but no more in love with the Russians than it had been before. His journal in Chungking and later in Nanking from 1945 to 1949 gives interesting sidelights on the events of the time – the decline of the Nationalist regime, the beginning of civil war, the Marshall mediation, the eventual collapse and Communist victory. When Huang Hua, who became the permanent representative of the People's Republic of China to the United Nations, in late 1949 called together the foreign diplomats in Nanking, newly occupied by the Communist forces, he addressed them in Chinese and offered no English translation. This was to mark the end of China's subservience to imperialism. Chester Ronning was the only diplomat present capable of making the translation into English for his colleagues.

In the twenty-five years since the establishment of the People's Republic Mr. Ronning has been in numerous posts concerned with China. For example, when he was Canada's high commissioner to India he was much concerned with the Vietnam War and Chinese policy toward the American intervention there. At the Geneva Conference on Korea in 1954 he saw his old friend Chou En-lai and John Foster Dulles, among others, close at hand. He gives many interesting and personal sidelights on the American military efforts in China, in Korea, and in Vietnam. Without stooping to outright denunciation, Mr. Ronning cannot conceal his sense of dismay, bafflement, or outrage on numerous occasions when American policy seemed to him either woefully misguided or unconscionably aggressive. To an even greater degree than the American foreign service officers in China, Chester Ronning had experienced at first hand, as a close observer, certain distinctive phases of the Chinese revolution. He not only saw the old order at the end of the Ch'ing in his native city of Fancheng; he also superintended a middle school during the revolution of the 1920s. After 1945 he was intimately connected with Canada's China policy. He knew which way the winds of history were blowing in China. Below the level of ideological rhetoric he could see the futility and downright evil of certain American outworn attitudes and purblind policies. He helped Canada to record its continual misgivings, even when for reasons of *realpolitik* the Canadian policy had to go along with that of the United States or at least not counter it directly. Mr. Ronning is a kindly man but a man of conviction based on experience, and his picture of the American performance is not flattering.

J. K. FAIRBANK

Harvard University

Defence Yearbook 1974. Edited by the Royal United Services Institute for Defence Studies. (New York: Praeger Publishers, 1974. Pp. 338. $20.00.)

This compendium of articles on military issues and developments in military science belongs in the reference library next to the Yearbooks of the Stockholm International Peace Research Institute and the monographs of the International Institute of Strategic Studies. It is in the tradition of the best military writing, seeing the central issues of defense not solely in the classical terms of strategy and weaponry but in the broad context of international political and economic affairs.

Those whose interests lie in the direction of arms control and reduction may find the orientation of the *Yearbook* too military. It is difficult to assign a single classification to a work to which so many extremely knowledgeable experts have contributed, but I think it would be fair to say that the contributors tend to view the strategic issues of the times in terms of conflict rather than conflict resolution. This is, after all, a defense (read military) annual. It does not subscribe to a concept of diminished polarization of world politics. The issues that it identifies are seen essentially in terms of antagonisms, crisis, and military threat, and the means of addressing them as military means.

The eleven authors of Part I of the volume are primarily British and American, with the addition of a French and a German contributor. The author selection then is international but essentially Alliance international. This need not necessarily produce a we/they point of view, but the volume does quite often have a NATO ring to it, on guard against the Soviet threat, whether in relation to the Arab oil weapon which "it seems likely that the Kremlin encouraged – even if it did not indeed demand" (p. 79), or the Soviet threatened intervention in the 1973 Middle Eastern War, which "exposed the shallowness of her [Soviet] sincerity over detente" (p. 159). The NATO stamp is reenforced by the fact that Part II of the book, presumably prepared by the Institute staff, includes specific recommendations for the Western forces on weapons priorities.

Those qualifications aside, the high professional quality of the entire collection should be emphasized. The authors, who are identified at the start of each article with a brief descriptive paragraph on their backgrounds (a useful practice which more compendia should follow), are associated with universities or with institutions specializing in defense studies. All are distinguished specialists in the subjects on which they write.

The *Yearbook* itself can boast an impressive heritage. Previously Brassey's Annual, it began in 1886 as a British Naval Annual, intended to study the events of the year, draw lessons for the future, and stimulate an informed public debate. The Royal United Services Institute for Defence Studies, with which it is now joined in the publication of the present Yearbook, is the oldest institute of its kind in the world. Established in 1831 for the study of British defense and overseas policy, the Institute, like Brassey's, broadened its interests to cover defense in an international context and to reflect the views of the range of disciplines – political science, economics, behavioral and physical sciences – which today are concerned with military matters.

The scope and complexity of the subject matter in the 1974 *Defence Yearbook* make it impossible for a brief review to evaluate the individual presentations. A listing of topics in Part I should give a sense of the broad range of content: China's strategic posture in a changing world, the 1973 Middle East War, the continuing crisis in Southeast Asia, the strategic aspects of the energy crisis; also four essays dealing with the superpowers: their arms negotiations, confrontation at sea, strategic balance in the Mediterranean, and the global balance; and two focusing on France: the economics of French strategy, and French policy and the defense of Europe.

Each subject is treated in a thoughtful analytical discussion backed with historical and factual detail. The authors succeed in adding clarity and information even to those topics which have had constant attention in American journals and the press.

Part II of the volume is encyclopedic in its coverage of the state of the military arts. It covers the full range of weapons systems in service or under development, Soviet as well as Western. The publisher indicates that this section will be updated in successive editions. It will therefore serve as a continuing and invaluable source of information on the development of weapons technology.

Whether or not one feels the need to know such details as the tonnage of the principal classes of nuclear attack submarines and the range of all antitank missiles, the weapons section is worth perusal by anyone with even a remote curiosity about what is being bought these days with the $60–70 billion that is spent annually for weapons in the world. It may be useful to know, for example, that the modern weapons inventory includes enhanced radiation mininukes that will kill people but spare buildings, unmanned aircraft that can be controlled by a pilot sitting in perfect comfort many miles away, smart bombs which home on the target by means of laser guidance with almost zero error. These are the kinds of things that have revolutionized warfare and that carry the promise that the next battlefield could very well be your backyard and mine. It is as good a reason as any to read a book.

RUTH LEGER SIVARD

Washington, D.C.

The King's Parliament of England. By George O. Sayles.(New York: W.W. Norton and Co., 1974. Pp. 164. $7.95, cloth; $2.95, paper.)

This monograph in a series entitled *Historical Controversies* summarizes forty years of research by Professor Sayles (aided by H. G. Richardson). Its significance to political scientists, in particular to students of legislatures and their development, lies in the challenge to a set of "myths" generated over several centuries regarding the genesis and early history of parliament in England, myths which no doubt influenced attempts to transplant parliamentary institutions in the British Empire and elsewhere. Professor Sayles seeks to dispel the view that the parliament of the seventeenth century had its roots in the medieval parliaments, by looking at parliament through the eyes of the men of the time, and examining their perceptions of its function. He emphasizes the minor, even negligible, part played by representatives of the people, asserting that parliament was but one of several devices used by monarchs to settle questions of law and administration.

Sayles attacks several studies which he believes distort seriously any understanding of the medieval parliaments of England. Seventeenth-century writers, he argues, were less concerned with what actually happened in the past than with those things they considered important in terms of the current political debate as to whether the king alone, or the king and parliament together, was to be supreme. Such constitutional controversies were not, however, those of the Middle Ages, the problem of sovereignty never being posed in the form in which it appeared in their own day. He also charges that writers of official reports and major studies in the nineteenth century looked for the origins of democratic institutions at too early a date. Such "conventional wisdoms" about medieval parliaments were given further credence in 1951 by the foundation of the History of Parliament Trust. Sayles essentially argues that if we see in parliament "merely a public spectacle of political struggles between Crown and Commons and of democracy in embryo, we shall certainly never understand the medieval parliaments" (p. 18). Above all, it was the king's parliament, and agreement – not dispute – was the name of the political game. Sayles guides the reader through the evidence which sustains his interpretation – i.e., the medieval parliaments ended in strong monarchical rule, not in constitutional democracy. According to Sayles, Henry VIII, in 1529, deliberately selected parliament as an instrument to his liking, setting it on a new course as a legislative assembly, with no reason to suspect that cooperation would eventually turn into control. Medieval parliaments were the creatures of the king, summoned or not as he pleased, listened to or not as he deemed fit, and with no individual right of attendance – a device of government with an uncertain tenure, liable to be outmoded and superseded or remodeled and revivified.

This cogent, erudite argument for a history of parliament written not as the seed-bed of representative government, avoids the trap that historical continuity implies identity of function, despite the changing needs of society. Thus it provides an important lesson for students of political development no less than students of legislative assemblies.

JOHN D. LEES

University of Keele, England

Freiheit – Die Anfänge der Bürgerlichen Emanzipationsbewegung in Deutschland im Spiegel ihres Leitwortes. By Jürgen Schlumbohm. (Düsseldorf, West Germany: Pädagogischer Verlag Schwann, 1975. Pp. 299. Price not listed.)

Jürgen Schlumbohm is concerned with analyzing the several meanings of the word "freedom" as this word reflected social and economic changes in German society between 1760 and 1800. His approach to this challenging task is Marxist. The author is quite precise in stating this. For example, on page 13 we find the following: "the contents of consciousness and their verbal expression are essentially conditions through the constellations of social reality: however, thinking and speech in turn also have reciprocal effects upon social existence – only under these presuppositions does political speech and writing truly possess a real function." Throughout the work, Schlumbohm attempts to adhere to the method implicit in this statement. On the whole, he succeeds quite admirably.

By examining the writings of political publicists, Schlumbohm is able to demonstrate that there was indeed correspondence between changes in the ideational content of the word "freedom" and changes in society. For the somewhat antediluvian nobility – the supporters and beneficiaries of a half-mythical *Ständestaat* – freedom meant freedom from the several varieties of princely authority as well as the preservation of certain privileges, e.g., hunting or water rights. These remnants of a feudal social order were extent in Germany even as the nobility became a capitalistic class economically. For the bourgeoisie, advancing economically if not necessarily socially and politically, freedom became a far more complex concept. It implied freedom from feudal constraints upon social mobility. In this context it was "the advance toward [the] elimination of hindrances [to] social ascent which lay in the feudal social order" (p. 93). Freedom, however, also could and did imply such things as freedom of the press, freedom of conscience, and the freedom to advance economically. Obviously, according to Schlumbohm, this last "freedom" was of the greatest importance for the bourgeoisie.

Thus, the period under question saw the clashing of several notions of freedom, each representative of a particular class. The nobility and its supporters – particularly after 1789 – sought to defend its rights and privileges by appealing to a more "traditional" conception of freedom. Buttressing this position was an increasing tendency, as exemplified by Friedrich Gentz, to call upon Germans to defend their own particular "national freedom." This was a potent weapon. "For when the bourgeois emancipation movement in Germany oriented its conceptions of freedom toward the model of progressive Western lands, the validity of such 'foreign' models, on the other hand, now was opposed [on the basis of] principle" (p. 70). Concepts of "Natural Law" could be used to defend the nobility's position; but usually they served the more critical function of picking apart the old social order by revealing its lack of congruence with a presumed *natural* order in which all men ought to be "free," if not necessarily equal. The Natural Law argument, combined with an increasing emphasis upon a *"Rechtsstaat"* free from noble privilege or absolutist caprice, constituted the salient points of the bourgeois argument in favor of their particular permutation of freedom. Yet, despite the virulence of the clash between the several concepts of freedom, the bourgeoisie remained politically, and even to some extent socially, weak during this time. Hence, in the hiatus between things as they were and things as they ought to have been, there developed that peculiarly German (at least in the modern world) sense of "inner freedom," both a result of and a justification for, political inaction. Schlumbohm ends his work by considering the "radical" challenge to the proponents of the bourgeois conception of freedom – that of *economic equality,* i.e., of true social freedom. It was out of this confrontation that there developed the first great challenge to capitalism, even before large-scale capitalism predominated in Germany.

Schlumbohm has written a work that is of immense value to anyone – political scientist, historian, or philosopher – who is interested in how a word like "freedom," always so pregnant with meaning, can have its ideological and hence its ideational content radically altered by social and economic change. At the very least, Schlumbohm has vindicated the heuristic value of the Marxist method.

There are, however, several problems – and at least one irritant – in this work. Schlumbohm, perhaps out of some defensiveness arising from his Marxist approach, spends far too much time on methodological questions. In this regard, his introduction, which is only nine pages, reads as if it were thirty. Also, Schlumbohm from time to time raises issues in such a fashion as to suggest that he is about to offer a radical new interpretation of some phenomenon or other, but fails to follow through. For example, at the beginning of Chapter I, Schlumbohm seems to suggest that bourgeois economic development in the 1760s and 1770s has been grossly underestimated by conventional historians. By the end of the chapter, however, he has come around to the more traditional interpretation, *viz.*, that the German bourgeoisie, while gaining in economic strength throughout Germany and actually strong in certain areas, was still relatively small and quite weak politically and socially. This conclusion is repeated at the end of the book. One problem which the author does not consider at all concerns the somewhat anomalous position of the German nobility. By the middle of the eighteenth century, this class had been practicing capitalistic agriculture for some time. Yet in terms of the *social* relationships between it and other classes, the nobility remained happily ossified in feudalism. The problems raised here for Marxist interpretation in general and for Schlumbohm's in particular are great indeed. But, as far as I know, *no* Marxist ever has succeeded in dealing with this question convincingly.

Finally, Schlumbohm has a perhaps culturally conditioned tendency to carry on a substantial part of his argument in the footnotes. This is particularly annoying in the introduction and in Chapters III and V. Much of the material in his footnotes, replete with substantive argument and documentation, should go into the text. It is true, of course, that many classical authors, most notably Gibbon, added a certain amount of zest to their analyses through their pungent use of footnoting. But even bearing in mind the substantive qualification that this work is not in English, Schlumbohm, alas, is no Gibbon.

These criticisms notwithstanding, Schlumbohm's work is important for any scholar interested in the unhappy vicissitudes of the concept of "freedom" in Germany. Both because of the method employed and the wealth of hitherto unknown evidence provided, this book is highly recommended.

ROBERT A. POIS

University of Colorado

Fourth Shore: The Italian Colonization of Libya. By Claudio G. Segrè. (Chicago: The University of Chicago Press, 1975. Pp. 237. $15.00.)

Political scientists and historians interested in European imperialism and in modern Italy will be grateful for Professor Segrè's work, which well deserves a place in the University of Chicago's Studies in Imperialism series. It is precisely what a good monograph should be: concise, clear, and based on extensive original research on an important topic. It analyzes Fascist policy from a perspective that has thus far received little attention; and it will provide useful data for comparative studies of European imperialism in North Africa. The first important work on this subject to appear in any language, *Fourth Shore* will be read with profit on both sides of the Atlantic.

Although Professor Segrè is primarily interested in the imperialist policies of Italy's Fascist era, particularly in the plans and activities of Volpi and Balbo, he is careful to place the Fascist colonization of Libya in historical perspective. The first part of his book shows the remarkable degree of continuity that existed between the economic and demographic preoccupations of the liberal era, the Nationalist myth of "proletarian imperialism" (as distinct from the imperialism of the rich industrialized nations) and the Fascist attempt to turn Libya into a political and economic extension of the mother country.

In the second and much longer section of his work, Professor Segrè traces the history of Italian colonization in Libya from the end of Turkish rule in 1912 to the Fascist era. He shows that from the very beginning of Italian involvement, some politicians, economists, and intellectuals had grave doubts about the wisdom of holding on to what they derisively called "that big sandbox." Their misgivings were compounded by a long and costly fight against native Arab insurgents unwilling to exchange their Turkish masters for Italian ones. As for Italy's land-hungry peasants and unemployed workers, it was to America, not to Italy's Empire, that they looked for a more promising future. But the restrictive American immigration laws of the 1920s and the Depression later on revived interest in the colonization of an officially "pacified" Libya. As Professor Segrè shows, private capital could not provide either the means or the initiative to promote peasant colonization. Although the Fascist regime had embraced the old Nationalist dream of a Fourth Shore in North Africa, it discovered through the experiences of Volpi and De Bono that costly infrastructures would have to be built before peasant families could be settled in Libya. Thus, the actual building of the Fourth Shore was delayed until the years immediately preceding the Second World War. The advocates of colonization then found an ideal propagandist and organizer in Italo Balbo, whom Mussolini wished to keep as far away from Rome as possible. Professor Segrè's portrait of Balbo is perceptive and informative, if perhaps a bit too laudatory. He too seems to have succumbed to the charm and enthusiasm that made Balbo one of the most popular (and dangerous) figures of the Fascist regime.

In three very interesting essays, Professor Segrè examines the rise and decline of the Fourth Shore from the perspective of the colonists, of the technical experts involved in the project, and of the Libyans themselves. Together with his pages on Balbo, these essays document his main thesis. Although inspired by economic and demographic pressures, Italy's colonization of Libya was above all a political enterprise, designed to enhance the domestic and international position of the Fascist regime. It could not have materialized without a massive commitment of state resources which previous Italian governments had been unwilling to provide. Despite the hard work and sacrifices of many Italians, it could not survive the downfall of the regime.

CLARA M. LOVETT

Baruch College,
City University of New York

The Americas in a Changing World. Selected Papers. By Kalman H. Silvert et al. (New York: Quadrangle/The New York Times Book Co., 1975. Pp. 248. $3.95, paper.)

This book includes a collection of scholarly essays by specialists in Latin American affairs and the lengthy Report of the Commission on United States-Latin American Relations. Both the report and the essays are unified by a common theme which argues that the last few years have witnessed fundamental changes throughout Latin America and that the United States must thoroughly reconstruct its foreign policy in response to these changes.

The Report of the Commission enumerates thirty-three specific policy recommendations. These recommendations are directed at achieving five policy objectives, which include strengthening Latin American cooperation in the global context, heightening our sensitivities to Latin American interests, eliminating paternalistic and discriminating programs, preserving human rights, and promoting synergistic economic relations between Latin American nations and the United States. None of the recommendations is particularly novel or striking to the student of Latin American politics. The value of the Report is not in its freshness but in its excellent integration of these previously articulated recommendations into a unified policy statement based on carefully conceived and explicitly articulated assumptions about the present state of the world.

In its factual thoroughness and theoretical breadth, the Report previews much of the substantive material which follows in the essays. Although the close relationship between the materials in the Report and the essays contributes to the unity of the volume, this unity is not achieved without some cost of repetition to be born inevitably by the reader. Taken individually, however, the essays are quite readable and offer a limited range of approaches and suggestions for restructuring U.S.-Latin American policy.

The opening essay by Kalman Silvert argues that the United States must carefully tailor its foreign policy to respond to the different levels of political development of each of the Latin

American nations. Silvert outlines a developmental typology of Latin American polities to be used by policy makers in matching policy to levels of development and determining the appropriate form of U.S. intervention for individual Latin American nations.

Like Silvert, Riordan Roett notes the importance of political differences among Latin American nations and cautions against formulating a comprehensive regional policy for the area. Unlike Silvert, however, he focuses on national differences in geopolitical influence rather than developmental regime form. Roett convincingly argues for a pragmatic U.S. policy which would recognize the emerging international pecking order in Latin America and which would channel most of our foreign policy efforts into those nations among the upper echelons of this order.

A more global perspective is offered by Stanley Hoffmann, who describes a world that is still dominated by superpowers restrained by nuclear considerations but vulnerable to countless opportunities for both international terrorism and localized interstate chaos. Since the U.S. cannot preserve world order by itself, argues Hoffmann, it must enlist other nations by democratizing world politics and encouraging their participation in the management of international affairs. Hoffman therefore concludes that U.S. policy toward Latin America should facilitate a more responsible role for Latin American nations in managing the international community.

Jorge Dominguez analyzes Cuban/U.S. relations as a series of games alternatively involving various actors including the superpowers, multinational corporations, other Latin American governments, and domestic U.S. interest groups. After describing a range of conceivable U.S. policy options toward Cuba and discussing their comparative costs in terms of relations with the aforementioned actors, he finds that a policy of sufficiency involving little change is preferable. In his conclusions, Dominguez is somewhat at variance with the prescriptions of the Commission's Report.

Robert Cox presents a sobering analysis of policy options and their attendant costs in the Canal Zone. For the United States, reveals Cox, the Canal is a military liability and yields little economic benefit; for Panama, however, it is the most vital and salient issue on which President Torrijos has staked his political life. Cox stresses the certainty of bloodshed and damage to the U.S. hemispheric image absent an immediate accommodation which will minimize the U.S. presence in the Zone.

Changes in Latin American security conditions, notes David Ronfeldt, have prompted military leaders to shift their concern from problems of domestic violence to the deterrence of external aggression. Like the Commission Report, Ronfeldt sees the role of future military assistance to the region as largely diplomatic. He argues that such assistance should strive to minimize Balkanization, facilitate military technology transfers, and maintain the necessary contacts for mobilizing military joint ventures.

C. Fred Bergsten assumes that economic issues will provide the impetus for U.S.-Latin Relations in the future. He argues that to achieve its domestic economic objectives, the U.S. must retreat from a policy which treats Latin America as a special regional bloc. Alternatively, he suggests that the U.S. should conduct multilateral trade negotiations with Latin American nations as members of worldwide producer groups or specialized cartels where appropriate. Given the reality of transnational economic interdependence, the U.S. should take the initiative in imposing control on multinational enterprises and must promote global programs for the benefit of both the United States and Latin America.

The concluding essay by Roger Hansen further describes the magnitude and breadth of the changes in Latin America noted by the Commission and other authors. Hansen is pessimistic about the ability and willingness of the U.S. to respond completely to Latin America's requests for revisions in foreign policy. His essay rejects the soundness of alternative policy orientations grounded exclusively on either bilateralism or a special relationship with Latin America. Rather Hansen recommends a globally oriented foreign policy with a tilt toward Latin America which recognizes the historically significant institutional and economic linkages between the U.S. and its Latin neighbors.

The recommendations of the Commission and the individual authors are generally liberal in the context of American foreign policy, but only moderately so when viewed from a more global perspective. Third World nations and their advocates will find the recommendations unresponsive; and for different reasons, powerful North American pressure groups will find them inconsistent with their interests. This provides grounds for the political cynics among us to question not only the practicability of these recommendations, but also the weight of their potential impact in prompting changes in U.S. foreign policy.

WILLIAM M. BERENSON

Boston University Law School

The Strained Alliance: Peking, P'yongyang, Moscow, and the Politics of the Korean Civil War. By Robert R. Simmons. (New York: The Free Press, 1975. Pp. 287. $10.95.)*

This is an ambitious book. For the author sets out to demolish the conventional wisdom about the origins, conduct, and consequences of the Korean War and replace it with a set of new hypotheses. Nevertheless, as Professor Simmons admits at the outset, it is "not a definitive work." What is disappointing is that his hypotheses are so precariously grounded that their plausibility remains exceedingly low.

The thrust of his arguments is aptly summed up by the title and subtitle of the book: that the alliance between the three Communist nations during the Korean War was by no means harmonious but rather was strained; that the Korean War was primarily a civil war whose causes must be sought in the internal situation in the Korean Peninsula rather than in external factors; and that the dynamics of that "civil war" were closely linked to the strains among Peking, P'yongyang, and Moscow.

In order to bolster his arguments, the author examines a wide array of materials, with particular emphasis on transcripts (for the most part, in English translation) of the Chinese, Russian, and North Korean radio broadcasts. His principal methodology is Kremlinology – that delicate art of scrutinizing the esoteric Communist communication in an attempt to decipher its hidden meanings and messages. Although, given the paucity of usable data, Kremlinology is widely used in Communist studies, its practitioner is nonetheless constrained by the canons of scholarship and the rules of logic.

Even if one keeps in mind the author's explicit disclaimer that his are nothing more than hypotheses, one is still entitled to ask, of a scholarly work, whether the hypotheses have a reasonable basis – whether the evidence the author presents does in fact support or, at the least, can be reasonably construed as supporting, his conclusions or inferences. When Simmons's study is subjected to the preceding test, it does not fare too well. Due to the limits of available space, only a few of the problems will be discussed below.

As noted, one of the author's major contentions is that the Korean War was a civil war – that, in particular, North Korea played a larger role in it than has thus far been assumed. The boundary line between a civil war and other (presumably international) wars, however, remains hazy. Setting aside its origins for the moment, no one, including the author, denies that there was massive external intervention in the war. The number of belligerents alone amounted to nineteen nations. When the countries which provided various types of assistance are added, the magnitude of international intervention in the war becomes staggering. Besides, the *principal* belligerents included two major non-Korean powers – the United States and the People's Republic of China.

Simmons underlines the fact that North Koreans themselves referred to the conflict as "the civil war" (pp. 128–129). The text of the article in *Nodong sinmun* (North Korean party newspaper) which he cites at length, however, is two steps removed from the original version: it is an English translation (by the Current Digest of the Soviet Press) of a Russian translation (by *Pravda*) of the Korean-language article. Since the Korean language makes no rigorous distinction between civil and other types of wars (the generic term, *chonjaeng* [war] is used in most cases), one may question the utility of Kremlinological exercise here. In fact, North Koreans have used two expressions most frequently in reference to the war – *tongjok sangjaeng ui naeran* [fractricidal rebellion] and *choguk haebang chonjaeng* [fatherland liberation war]. According to P'yongyang, South Korea started the former, and North Korea responded with the latter.

More basic to the author's emphasis on the civil nature of the war is his hypothesis that the timing of the war was determined by North Korea. Among other things, he notes that "weapons for the invasion were still coming through the Soviet 'pipeline' in June, which would indicate an invasion plan for sometime later than June 25, when all of the military preparations would have been completed" (p. 120). First of all, it is doubtful that anyone can authoritatively differentiate "weapons for invasion" from weapons intended for use in the postinvasion phase of the war. Second, is one to assume that an invasion begins only after the inflow of weapons from their principal supplier ceases? Is it not a continuing process?

As "a 'harder' indication that June 25 was not the prearranged date," Simmons cites a statement allegedly made "at a conference of the communist Korean League in Japan" in April, 1950, that "[o]ur operations are scheduled, until further notice, for August" (p. 118). It simply strains one's imagination to believe that the North Korean leadership would either engage in or tolerate the public revelation of its invasion plans.

Since relations among states involve a clash of, or, at least, interaction between, perceived self-interests of the states concerned, they usually entail some measure of rivalry, tensions, and strains. That the relations among the three Communist states on the eve of, during, and after the Korean War were consistent with this general pattern therefore does not come as a surprise. It is when the author tries to underscore the magnitude of such strains that problems arise. In order to demonstrate "China's disappointment with the Soviet Union's U.N. stance" (p. 138), for example, Simmons cites various Chinese statements praising the Soviet delegates' return to the Security Council after a costly boycott (pp. 128–140).

---

*See Correspondence p. 1063.

He then writes: "The Chinese media made almost every comment on the situation but the direct one: 'Why wasn't the Soviet Union in the Security Council during June and July blocking the successful American pursuit of legitimacy' " (p. 140)? In short, he would have us believe that what passed as a commendation was really a rebuke – a rebuke which was so carefully veiled that only the hindsight of subsequent Sino-Soviet relations helped to uncover it. Sino-Soviet relations have since deteriorated to the point where Peking no longer shrinks from using explicitly denunciatory language vis-à-vis Moscow, and vice versa. One wonders why Peking has not articulated its alleged displeasure over Moscow's U.N. stance. In November, 1975, Peking even *distorted* Soviet Ambassador Malik's U.N. speech on the Korean question in order to accuse Moscow of "hypocrisy" – of allegedly siding with South Korea while pretending to support North Korea (see *Peking Review,* November 7, 1975, p. 23 and compare it with the text of Malik's speech in U.N. Document, A/C.1/PV.2061, 21 October 1975, 53–55). If Peking goes so far as to distort the truth, why doesn't it set the record straight with respect to its perception of Moscow's role in the U.N. in 1950?

One of the symptoms of the strains in P'yongyang-Moscow relations which Simmons cites is a passage in Kim Il-song's message to the Soviet Union in August, 1952 that "our people proudly call Comrade Stalin 'our father and savior.' " This Simmons construes as "an indirect rebuke to the Soviet Union," because, according to a *Pravda* dispatch, "[i]n Korea they have a saying that a 'good friend is better than a relative' " (p. 214). First of all, it is hard to believe that the North Korean leader, in the midst of a war that was being fought on his side with predominantly Soviet weapons, would be so rash and foolish as to include a rebuke, no matter how indirect, in a message expressing his gratitude for Soviet help in the liberation of Korea. Secondly, and more importantly, regardless of the authenticity of the alleged Korean saying, "father" is not a disrespectful term in Korea. In fact, Kim Il-song himself, the object of an unequalled personality cult, is referred to as *oboi suryong* [father(ly) supreme leader].

These are some of the numerous instances of doubtful evidence and questionable inference one encounters in the book. Before closing this review, however, I am compelled to mention another seemingly minor but potentially serious problem. To my considerable surprise, I found striking similarities between several passages in the book and those in my own work:

> This third group had the unique advantage of being both politically organized and militarily trained (p. 23).
>
> "This group had the unique advantage of being both politically organized and militarily trained..." (B. C. Koh, *The Foreign Policy of North Korea* [New York: Praeger, 1969], p. 3).
>
> A fourth group vying for power in postwar North Korea was the "domestic" Communist faction (p. 23).
>
> "Still another group vying for power in post-Liberation North Korea was the domestic Communist faction" (Koh, p. 4).
>
> The Yenan faction had meanwhile organized itself into a separate New People's Party on March 30, 1946. The latter attracted a wide following among the petitbourgeoisie and intellectuals; its growing popularity was viewed with alarm and jealousy by the Soviet-Korean and Kapsan-sponsored Korean Communist Party... (p. 26).
>
> "The Yenan faction ... organized itself into the New People's Party ... on March 30, 1946. The party attracted mainly petty bourgeois citizens and intellectuals. Its wide popularity ... was viewed with a keen sense of alarm and jealousy by the North Korean Workers' Party..." (Koh, p. 9).

The first two passages in the Simmons book are not accompanied by any footnotes, but the third passage is attributed to a Korean source (Kim Ch'ang-sun, *Pukhan sibonyon-sa,* pp. 97–99). it so happens that this is precisely the source I cited for my own passage. However, since I did not quote directly from Kim Ch'ang-sun, it is unlikely that reference to the same source accounts for the similarity. More important, whereas Simmons implies in an earlier footnote (note 5, p. 35) that he is using the English translation of the Kim book, the page numbers given above belong to the Korean version, which I used.

It should be noted that none of the passages in question contains any novel ideas; they simply describe aspects of North Korean politics in the pre-Korean War stage which are well known to students of North Korea. What is being questioned here, then, is the apparent lack of integrity on the part of the author. Together with the weaknesses in evidence and reasoning illustrated above, this serves seriously to undercut the author's credibility.

B. C. KOH

University of Illinois at Chicago Circle

Unequal Partners: Philippine and Thai Relations with the United States 1965–75. By W. Scott Thompson. (Lexington, Mass.: D.C. Heath and Co., 1975. Pp. 183. $13.50.)

It is probably fair to say, as the author does at one point, that between unequal partners "the greater the disparity in size and resources, the less overt pressure the greater power will have to exert. The smaller power is only too aware of how much the greater power can do for him and conversely, of how much can be withdrawn if he does not go along."

Both the Philippines and Thailand were closely bound to the United States at the end of World War II, each with different historical antecedents. Their separate (but often similar) perceptions of survival needs, together with internal quests for power and riches, constituted one side of the ledger. America's newfound world role – Asian department – and an abundance of resources formed the other, and unions of sorts were forged. Professor Thompson provides abundant information on the varied phases of the relationships among these nations.

Dependency for a small power may be necessary at times, but it can be exceedingly hazardous, and small powers should eschew it whenever they are able. But the small power, as Thompson shows, is not defenseless or wholly devoid of bargaining power. It can help or hinder the large state in various ways, among them by allowing or withholding the use of strategic sites. Such locations, in the cases of the Philippines and Thailand, have been significant, but it is abundantly clear now that the long-range value to the United States of strategic locations in Thailand is far less important than that of strategic locations in the Philippines. Thailand (especially because of location) has had to relearn how to bend like the bamboo, and indeed both have been making their peace with erstwhile enemies.

Professor Thompson poses early on the interesting question of whether the adaptive mechanisms of a small state are better managed by democratic or by autocratic systems. He partly answers this by stating that "the circumstances determine which is more pertinent." In a sense, this view is sustained by his evidence which indicates that under some circumstances authoritarian regimes have the advantage, while under others the reverse obtains.

This book is disquieting, not because it postulates something terribly new, but rather because it documents some uncomfortable notions that have been around for some time. Does the largesse of democratic America flow more easily to democratic polities? Apparently not. Indeed, the scuttling by Marcos of the Philippines' long and rather distinguished democratic tradition seems to have resulted in closer ties, while Thailand's new experiment with democracy appeared to cool the American-Thai association. Thompson suggests that American confidence is attracted by stability, not democracy. But he also notes that although the small sovereignty in a buyers' market seems best served by internal centralized authority, in cases where the larger nation is pressuring the smaller nation, democratic institutions are observed to strengthen the latter's hand.

The primary issue, however, is not democracy as such but rather stability and order. The reaction of both American business and government to Philippine martial law seemed almost euphoric, but in all fairness, conditions of urban order, security, and internal politics in the country seemed to be at a low ebb. Thompson wrote that while democratic institutions in the Philippines were annihilated (by Marcos), "genuine social and economic reforms began to be undertaken which left open the possibility of a fuller and wider democracy in the future." But as time goes on, that hope seems less viable.

This volume suggests another line of reflection. In a world often characterized as interdependent, one nevertheless wonders whether interdependence is always the ultimate reality, or whether it is often a mere shortsighted excuse for following a line of least resistance. When a small nation becomes dependent upon a great power, it may well be on the way toward compromising its most transcendent interests in the guise of protecting them. This is not to argue that great states particularly like to cast their lesser allies aside with impunity, but as Thompson observes, that they simply do so when their vital interests are at stake. Perhaps this is as it should be, for is not enlightened self-interest supposed to be the rational rule of national behavior in the international arena? If so, then the enlightened self-interest of the small polity should also be its rule. Clearly self-reliance remains more than just a minor virtue.

It may be, as Thompson seems to suggest, that internal concern with democratic values must be divorced from parallel concerns with the internal affairs of other states, a version no doubt of the "democracy is not for export" thesis. Trade, economic ties, and bakshish may be among the ties that bind, but it remains to be suggested that in a great state's dealings with others, not only its material interests are at stake, but clearly its reputation is as well. In such settings if the United States compromises democracy, as it does when it clearly favors authoritarian regimes, it cannot escape diminishing its own integrity thereby.

Professor Thompson's book is complex and consequently it does not always flow easily in the reading, but it is intensely interesting. His detailing of the course of conflict and cooperation between the United States and Thailand and the Philippines is full of information and insight. On the whole it is well written and balanced, and constitutes a valuable contribution.

H. B. JACOBINI

Southern Illinois University-Carbondale

Intercountry Income Distribution and Transnational Enterprises. By Constantine V. Vaitsos. (New York: Oxford University Press, 1974. Pp. 198. $16.00.)

Despite the recent resurgence of interest in political economy, political scientists and economists still have difficulty communicating with one another. This lucid and admirably organized book was written by an economist for, one suspects, other economists. Nevertheless it contains a great deal of information that should be of interest to political scientists. Furthermore most of the technical economic calculations have been relegated to the appendices and anyone with a basic understanding of economics should have little difficulty following the main argument.

Unlike so many of the writers on the still highly controversial subject of multinational corporations, the author eschews dogma and polemic. He apparently has no ox to gore, saying that investment of multinational firms can be "mutually beneficial" (p. 119) to both the firm and the host country. But the inescapable conclusion, based on an analysis of the effects of foreign investment in Colombia and several other South American countries, is that in practice most of the benefits have accrued to the multinational firms. With great lucidity the author discusses the variety of techniques and practices multinational firms have employed to ensure the maximum profitability of their subsidiaries in foreign lands. Among the most important are the use of tie-in arrangements; the underdeclaring of profits; the resort to tax havens; the manipulation of royalties, patents, and interest payments on interaffiliate loans; and the utilization of transfer pricing on interaffiliate sale of goods. The author makes it clear that in resorting to these devices, most multinational firms are doing nothing illegal. Indeed, they are for the most part operating within the terms of the contracts negotiated with their host countries.

Undoubtedly the part of the book that will most interest political scientists is that on the bargaining process. Particularly worthwhile is the analysis of the Comité de Regalías, a committee established by the Colombian government in 1967 to negotiate and approve all contracts of technology transfer. Despite "significant achievements" (p. 130) the problems faced by the committee prevented it from being totally successful in deriving maximum benefit from foreign investment for the host country. Among these problems were lack of information on alternatives, lack of expertise to match that at the command of the multinationals, and susceptibility to corruption. In perhaps the most valuable section of this book, Dr. Vaitsos advances suggestions for improving the bargaining power of less developed countries.

These suggestions are most welcome at the present time. Developing nations, at recent international meetings such as the UNIDO conference in Lima and the Seventh Special Session of the United Nations General Assembly, have vigorously emphasized their determination to increase the extent and rate of their industrialization. But in order to achieve the anticipated benefits of industrialization, a new bargaining framework is necessary. The work of the United Nations in developing codes of acceptable behavior for multinational firms and in encouraging the elimination of restrictive business practices is a hopeful sign. But in the final analysis each country must look after itself. This book should be invaluable to those national officials charged with the responsibility of negotiating with multinational corporations and to political scientists interested in analyzing the negotiating process.

JOHN P. RENNINGER

United Nations Institute for Training and Research

Nonintervention and International Order. By R. J. Vincent. (Princeton, N.J.: Princeton University Press, 1974. Pp. viii, 457. $18.50.)

This is an intelligent, well-written book designed to state a case for upholding the principle of nonintervention. Part One contrives an "approximate" definition of intervention as ". . . that activity undertaken by a state, a group within a state, a group of states or an international organization which interferes coercively in the domestic affairs of another state . . . it is not necessarily lawful or unlawful. . ." (p. 13). It is not particularly helpful as a definition except perhaps to indicate the area of problems chosen by Professor Vincent for study.

Part Two outlines the history of nonintervention, in three chapters dealing successively with principle, theory and practice. The pre-1945 material is, of course, interesting and its treatment is valid in so far as the author regards the principle of nonintervention as a principle which long antedated the U.N. Charter and survives it. There is, however, an inevitable difficulty in transposing practice, or conclusions drawn from practice, from one era to another when the rules of law have undergone radical transformation from one era to another.

Part Three is a study of the principle in contemporary world politics. It contains a very good survey of Soviet practice, including episodes such as Hungary and Czechoslovakia, and explains Soviet adherence to the principle of nonintervention on the hypothesis that Soviet theory admits only of intervention by *capitalist* states. This is probably correct. Certainly I have never found a persuasive account of the problem which reconciles Soviet practice with any general principle of nonintervention, although the theory is patently absurd.

A separate chapter on U.S. practice begins to expose some of the author's weaknesses. Professor Vincent seems to be a political scientist rather than a lawyer, for although he tackles the law with courage backed by wide reading, he is least perceptive here. For example, he sees the crucial legal point over Vietnam as being whether the conflict was an international conflict or a civil war (p. 224). In fact, granting the U.S. view that it was an international conflict, there remained the most crucial question: "What right does the U.S. have to intervene in such an international conflict?" In essence, the problem therefore becomes one of defining what right is conferred by the right of *collective* self-defense: the point escapes the author. Admittedly, he has from the outset decided to exclude all but intervention in domestic affairs; but this raises a doubt about his judgment in so doing, for the most difficult and potentially destructive (to world peace) interventions are those in an international conflict. To take a further example, there is the transition from a discussion of intervention by states to a discussion of intervention by the United Nations in Chapter 8, without any real attempt to inquire whether the rules governing intervention by states and by the U.N. are, or even should be, the same. Nor does one find any real discussion of the vexed question whether regional groups of states have, as such, rights of intervention denied by law to states acting alone. Even the highly problematical "exception" to the prohibition of intervention claimed by the Afro-Asian states in the guise of a freedom to act to promote self-determination, or eradicate colonialism, is scarcely mentioned.

Perhaps the mistake is to expect too much of one book, or one author. Some of the chapters are highly informative and rewarding, especially to lawyers, perhaps because the author brings to them skills which most lawyers lack. For example, the sketch of the debates in the U.N. leading up to the 1965 and 1970 Declarations is highly perceptive and sensitive to policy implications in a way which few lawyers could emulate. The difficulty, by no means uncommon, is that the author who writes in the field of international relations will rarely satisfy all the specialists whose areas of study come under his scrutiny.

With the author's overall conclusion I would certainly have no quarrel. He sees the principle of nonintervention as a "neutral norm of abstention" (p. 386) which in the present system of independent and often antagonistic states is the essential prerequisite to the maintenance of peace and order. This seems very true. If one examines the many cases of intervention, by both socialist and capitalist states, by East and West, the overriding conclusion is that they have brought little profit to anyone and usually posed very considerable threats to world peace.

D. W. BOWETT

Queens' College, Cambridge

International Law and Psychology; Two Studies: The Intrusion of Order and Conscience and Society. By Ranyard West. (Dobbs Ferry, N.Y.: Oceana Publications, Inc., 1974. Pp. 418. $17.50.)

This volume contains two studies, *Conscience and Society* (1942) and *The Intrusion* *of Order* (1973). Both studies analyze international politics and legal theory from the interdisciplinary perspectives of psychology, political philosophy, and jurisprudence.

The earlier study develops the political and social theories of Hobbes, Rousseau, and Locke as functions of their social and political times and their personal psychological and social situations. A further elaboration of political and social theory follows, with summaries of Hegel's, Bosanquet's, Hobhouse's and others' views of law, the state, and the relationship between the individual and the state. Dr. West, holder of both the M.D. and D.Phil. degrees and a trained analyst, argues that to construct a relevant theory of government one must account for the psychological component of human behavior. Since each theory of government contains assumptions about human motivation, the task is to identify what human nature is about and then choose the appropriate theory.

The author contends that since the purpose of law is to control innate human aggressiveness, law without sanctions, either internal or external to the individual, is impossible. From here it is not hard to see where the argument flows about international law. Without the external sanction to compel sovereigns to obey, international law is a flop. The internal sanction which often restricts human aggressiveness is unavailable in international relations since the blinders of nationalism, prejudice, and ideological self-justifications arouse popular support for action calculated for short-run national interest. The proposed solution is to move from the nation-state as the fundamental unit of international politics and substitute a world state or global confederation of states. The argument should be familiar to students of international politics and international law.

The later study, *The Intrusion of Order* (1973), continues themes developed thirty years earlier. In a short introduction to Freudian social theory, Dr. West identifies power and prejudice as the governing processes of international politics. Extending the psychoanalyst's technique to recent events, the author presents four episodes to complete the argument: the Abyssinian crisis of 1935; appeasement in Europe, 1937–39; Soviet-American relations, 1945–50; and Korea, 1950.

These cases identify alternative policies which, though not followed by the political leaders of the time, might have altered events. Had Great Britain followed the results of the Peace Ballot, Dr. West argues, war might not have been avoided, but it would have occurred for a "good cause." Had Neville Chamberlain followed a collective policy to include both France and the Soviet Union to confront Germany, or, had he taken the lead in defusing Hitler's appeal to the German people by offering colonial redress and treaty revision, war might have been averted. In both cases the political leadership reverted to nationalist thinking and anti-Soviet prejudices, making the outcome probable – war with Germany.

In discussing the postwar era, Dr. West turns to the United States and its attachment to nationalist thinking and anti-Communist prejudices. Here too he outlines the theme of missed opportunities: an aggressive anti-Communist policy, a Communist reaction which triggered further anti-Communist policy, a Communist reaction which triggered further anti-Communist action, and the descent into a thirty-year old cold war with its off-again, on-again crisis antagonisms bringing the world to the brink of "hot" war. Dr. West concludes that if the nation-state continues to represent the power interests of human communities the chances for calamity will increase. "This must continue until and unless the institutions of cooperative law and order are set up upon a world-wide scale" (p. 92).

What then is the remedy Dr. West prescribes to deal with this dangerous and potentially fatal affliction? Following the analogy of how individuals reorient their perspectives when confronted with insecurity or resistance to unpleasant facts, Dr. West asks us to ". . . re-examine the Russian and the Chinese Communists, their behavior, their nature, their doctrines and your own response to their presence in your world during the past half century of doubts and misunderstandings!" (p. 119). In Moscow and Peking a similar reinterpretation should occur with respect to United States doctrine and action. To conduct this all-around examination "massive . . . factual knowledge" and "enlightenment as a flash or as a glimmering that increases" will help us identify a "common humanity we share with our 'enemies' . . ." (p. 120).

Dr. West assumes throughout that the rethinking will lead to the more ordered peaceful relations which could have arisen had alternative policies been followed. Just the opposite may occur: increased examination of action and motive may lead to a more nationalist policy than the present admits. Also, politicians' capacity to rethink old beliefs (and therefore minimize the impact of resistance to new ideas) is most likely to reveal itself when there is some political advantage to be gained. Recent United States policy toward the People's Republic of China indicates that politicians can restructure their most deeply rooted beliefs when the international and domestic environment will provide the incentives for change. Dr. West implicitly recognizes this mechanism, but unfortunately minimizes its operation.

In *Conscience and Society* Dr. West argues a psychological theory of law and social order which includes an all-too-brief examination of contract. Since law and promise-keeping are fundamental elements in social order within society, the author argues, the absence of kept promises, or the belief that promises will not be kept, prevents a stable international society. Recognizing this as a doubtful premise, nonetheless can we suppose or hope that promises now made within the international arena have a greater chance to be kept as the interconnectedness among states increases? The four episodes in *The Intrusion of Order* are national security or diplomatic issues, but will energy, development, ecology, and trade issues effectively alter promise-keeping among states and encourage reliance on international institutions? Maybe yes.

International Law and Psychology most usefully presents the traditional problems of human motivation, political organization and world (dis)order. For political scientists concerned about international relations and law most of *Conscience and Society* will be restatement. When the section on the "Psychological Theory of Law" is read as a prelude to *The Intrusion of Order* however, speculative possibilities arise. As an empirical examination of cases leading to general conclusions about the inadequacy of the current era, Dr. West's volume resembles the psychologist's daily appointment record: who would condemn him if he sees the world motivated solely by prejudice, nationalism, and recurrent crises if he examines only Abyssinia, appeasement, the roots of the cold war, or Korea? Dr. West holds up a psychological mirror to our era – what he cannot do is give us an insight into the instrumental forces of change. For that we rely on circumstance, politics, and social science research (?).

KENNETH D. AUERBACH

Syracuse University

The Politics of Foreign Aid. By John White. (New York: St. Martin's Press, 1974. Pp. 316. $13.95.)

The study of political aspects of foreign aid has generally focused on the policies of a particular donor country or on case studies of particular donor-recipient relations on a specified project or over a defined time period. The field of general analysis of foreign aid has usually been left to the economist or to the quasi-scholarly study aimed at predetermined policy ends such as the Pearson report. This study by John White is an important and largely successful attempt to pull together the fruits of more particularistic research and to begin a systematic analysis of the role of foreign aid in domestic and international politics.

White, who is on the staff of the Institute of Development Studies, University of Sussex, consciously seizes a middle ground between what he calls the cynical ideologue who sees aid as nothing more or less than an attempt by the donor to generate and perpetuate the economic and political dependence of the recipient and the credulous moralist who sees aid as the altruistic acceptance by the rich donor of an obligation to help the less fortunate poor recipient. White claims the role of a skeptic who avoids automatic acceptance or rejection of stated explanations of aid policy but examines each on its merits.

The only *general* criticism of aid policy he finds meritorious is that sometimes the wrong kind of aid has been given because of foolishness rather than malevolence and sometimes it has done more harm than good. He remains a believer in aid, primarily on moral grounds, but it is difficult to see why. His chronicle of aid activity and his survey of the literature suggest that the wrong kind of aid has been given more often than not and that there seems to be no little malevolence in some aid decisions. Moreover and more importantly, the political impediments to administrative and policy reform are great, and, in spite of his avowed faith in aid, White is not sanguine that the requisite political will is available to make the kinds of changes his study so clearly recommends.

Two major contributions of this study are rare in the literature. First, White's approach is genuinely comparative. He is interested in the motives and administrative practices which characterize various national and multinational aid programs and analyzes not only the major donors but the smaller newcomers as well. He also uses a comparative framework in analyzing the different uses to which aid has been put in different settings and the different motives for and costs of seeking and receiving aid.

White rightly points out that assessing motives for aid-giving and -receiving is a very slippery and hazardous undertaking because the many agencies and political institutions involved bring divergent motives to their aid decisions. He concludes that most aid programs originate in political crises which are almost historically accidental. Thereafter, aid programs simply continue with incremental changes because, short of another political crisis in the relations of donor and recipient, both lack the political interest or the political will to make major changes.

The second major contribution, often neglected in the literature, is to break down the monolithic image of the state, both donor and recipient. It is not uncommon to analyze the influence of bureaucratic bargaining and infighting, of political competition and public opinion on the aid policies of *donor* states. White, however, also deals with the effect of aid policies on bureaucratic power and of bureaucratic and political considerations on aid utilization in *recipient* states. While generally ignoring the much better documented impact of aid on the power and prominence of the military in developing societies, he emphasizes the impact of aid on nonmilitary segments of recipient societies and bureaucracies, most notably planning commissions and finance ministries, and the struggles within recipient governments for aid as a resource in internecine battles.

White also provides a useful description and analysis of foreign aid theories and the evolution of aid, as well as an assessment of the costs and benefits – both economic and political – of aid which is especially insightful on the subject of technical assistance.

Though White's definition of aid as "any transfer of resources from rich countries to poor countries which the former choose to call 'aid'" (p. 23), clearly includes both military and developmental assistance, he elects to focus almost exclusively on developmental aspects. This is unfortunate for, purely military aid apart, it is exceedingly difficult, especially in the case of the major donors, to disentangle developmental and security facets of aid programs. The developmental focus directs our attention to one type of aid impact but it largely ignores another whole range of impacts which are at least of equal importance.

This is an important book. It provides a systematic and perceptive inventory of what we know and, equally important, what we do not know about the purposes and impacts which characterize the foreign aid game. It is a major contribution to an overall understanding of the role of foreign aid in development strategies and in international and domestic politics.

M. GLEN JOHNSON

Vassar College

Technology in Comecon: Acceleration of Technological Progress Through Economic Planning and the Market. By J. Wilczynski. (New York: Praeger Publishers, 1975. Pp. xvii, 379. $22.50.)

The author, an Associate Professor of Economics at the Royal Military College in Australia, presents in this book a description of the progress of technological innovation in the European Comecon countries. It will be a disappointment to anyone who expects a systematic examination of the subtitle's three variables and of their interrelationship. Nevertheless, political scientists may find it useful for the information it contains, if not for the analysis.

Following two introductory chapters on today's technological revolution and on manpower and investment in Comecon, both of which contain familiar material, the author deals in turn with various sectors of the civilian economies in Eastern Europe and the USSR. He begins with energy, in which we see as elsewhere that Comecon has made great strides in production in the 1950–1970 period, but at the same time that its levels of technology are "well below" those of the West. It is also characteristic of Comecon that its leading member, the Soviet Union, is "ahead of the West" in certain advanced forms of energy technology, but not yet uniformly so.

The chapters on computerization and quality in production seem of greater than average interest. The discussion of obstacles to computerization is particularly good. Also of interest is the author's statement that even the degree of mechanization, let alone automation, is low in Comecon. This suggests that a proper account of technology in the European Communist states should begin with mechanization rather than the more sophisticated elements of the "scientific and technical revolution," as Wilczynski has done.

The chapter on technical cooperation would have had more interest if certain questions had been broached. We do learn that equalization of economic levels of member countries is a Comecon policy. But what policy, one wonders, does the organization have on technology; what are its goals? What politics are involved in securing cooperation on R and D or on specialization? There are indications, Wilczynski says, of the primitivization of the Czechoslovak, East German, and Hungarian economies in these cooperative arrangements. If this is true, is it because the flow of technology is generally undirectional? Unfortunately, the answer cannot be found here because no general theory of the diffusion of technology is utilized as a framework. Indeed, there appears to be no framework at all; even the author's concept of technology is not defined.

Only in the penultimate chapter do we learn of the priority assigned to the military sector in Comecon. In fact, this is a self-sufficient, high-technology sector; the civilian one is residual, enjoys little technological "spinoff" from the military, and acquires technology unevenly, erratically. In view of this, it is curious that Wilczynski devotes so little space to the military economy, and so much to the civilian.

In the final chapter the author at last turns his attention to "theoretical" questions. Actually, however, these comprise neither theory in the social scientific sense nor philosophy. They are ideological. And instead of explanation we are given speculation. Once again it is "socialism" *versus* "capitalism." "Which system" the author asks rhetorically (p. 361), "has a greater capacity for technological progress?" How much "capitalist" technology can the Comecon countries absorb before they turn completely away from "communism"? A tempting response is, who cares? More seriously, the problem is that these considerations, even if they were theoretically refined (which they are not) are introduced far too late for the author to answer them on the basis of the foregoing chapters.

Despite its theoretical shortcomings, this is a rather stimulating book because it suggests implicitly but strongly the need for a policy study of technological progress in the Soviet Union and Eastern Europe, a challenge that should be undertaken by some political scientists in the near future. As it stands, it should help area specialists update their knowledge of the Comecon economies, and would be of value for students in interdisciplinary courses. It contains numerous tables, several diagrams ranking the Comecon countries against the rest of the world, and an index. Despite its recent publication, however, most of the data do not go beyond 1970.

BOHDAN HARASYMIW

The University of Calgary, Canada

Case Studies on Human Rights and Fundamental Freedoms: A World Survey. 2 vols. published for the Foundation for the Study of Plural Societies. Edited by Willem A. Veenhoven and Winifred Crum Ewing. (The Hague: Martinus Nijhoff, 1975. Pp. 1137. Price unknown.)

This is a reference work containing some thirty-six separate essays and studies dealing with the enjoyment of fundamental rights and freedoms on the part of ethnic groups, though some of the authors also treat the extent of such rights of all kinds of groups and others deal with subcultures which are not exactly ethnic. The following countries and regions are covered in the survey: Soviet Union, China, United States, West Germany, Spain, Peru, Middle East, Pakistan, Africa, Japan, Eastern Europe, Australia, Britain, Belgium, Israel, India, Sri Lanka, Fiji, West Irian, South Africa, Mexico. In addition some of the essays deal with groups and issues which are not discussable in terms of a single country or region, e.g., essays on anti-Semitism, the plight of Amerindians in Latin America, the Kurds, the Kabyls, women in Islamic societies, and the evils of communism in general.

ERNST B. HAAS

University of California, Berkeley

Plato's Republic. Translated by G. M. A. Grube. (Indianapolis, Ind.: Hackett Publishing Co., 1974. Pp. vii, 263. $10.00, cloth; $1.50, paper.)

This new translation recommends itself to teachers and students because of its price: the paperback version will be the cheapest available, and the publishers are to be commended for their economy. Moreover, the translation is quite literal. It almost goes without saying that it is superior to the Cornford translation. Anyone who knew Greek, who was concerned with faithfully conveying what the Greek meant, and who was not under the delusion that his intellectual superiority allowed him to edit out large portions of Plato's original would produce something better than Cornford. The continued widespread use in schools of the Cornford edition is an unhappy testimony to the ignorance or carelessness of those who assign it. But to recommend a translation because it is preferable to Cornford is faint praise, and Grube deserves more than this. His effort is more careful and accurate than (in ascending order of merit) Jowett, Spens, Chambry's French, Lee, Lindsay, and Shorey; in point of clarity if not precision Grube also improves on Davis. The translation is inferior, however, both in clarity and accuracy, to that of Bloom.

For the most part Grube reproduces what Plato wrote, and in down-to-earth English. Still, Grube is not always painstaking and hence cannot be relied upon in details. This is because he does not believe the *Republic* merits philosophical attention to its details. As he says in the Preface, the *Republic* "is not an esoteric treatise for students of philosophy, it is addressed to the ordinarily educated man." Grube has arrived at this remarkable judgment on the basis of the fact that in his own reading he has found the "difficult and highly controversial passages" to be "few." To this I am tempted to respond that Grube's reading has been "blessed" – in the Socratic usage of the term. At any rate, since Grube has not himself experienced a sense of perplexity and bafflement over very much of the *Republic,* he is not inclined to conceive of his work as aimed at helping readers who have had such difficulties. Above all, he does not strive to render important Greek words consistently.

I will confine myself to stating what I hope will be helpful cautions pertaining to Grube's translation of some key terms. First as regards "justice" and "injustive," the theme of the work. The Greek word and its relatives are not always translated into words related to the English "justice" or "right": in some crucial passages (e.g. 330e–331a), "injustice" is rendered as "sin" or "sinning," without even a footnote warning against religious – not to say Christian – overtones. What Paul Shorey remarked of the word "faith" ("Always avoid in translating Plato" – note to Loeb ed., 534) seems to apply even more to "sin." Second, the word *politeia.* This is the title (traditionally written "Republic" from the Latin) and is one of the basic theoretical terms in Plato's political theory altogether. It is among the most important words in the dialogue, and the reader ought to be able to follow its usage, observing how its meaning unfolds and develops as the conversation proceeds. He cannot do so with the present translation. Instead of always trying to translate *politeia* in the same way or at least noting each time it appears, Grube translates it variously as "community" (397d), "city" (424a), "administration" (427a), "form of government" (455c–d), and "constitution" (497a). Only the last two are really accurate, and the reader should be alerted to the danger in importing Montesquieu's familiar terminology ("forms of government") into Plato. Some additional brief warnings: "dictator" is not a harsh enough equivalent for the Greek *tyrannos* ("tyrant" is preferable); the "noble lie" is a *lie,* not a "noble fiction" ("fictions" are not necessarily meant to deceive; the noble lie is a deliberate deception – contrast p. 82 with p. 48n.); the diagram Grube uses to illustrate the famous "Divided Line" is wrong – Grube has failed to discern that the carefully stated ratios and the theorems of Euclidean geometry require the two central line segments to be equal (p. 164n.).

One is also forced to note a certain lack of scholarly professionalism in this edition. There are not sufficient footnotes to explain many important allusions. The book lacks an index. The bibliography is out of date and makes no reference to the very important scholarly advances that have been made in the past ten years in our understanding of Plato's political philosophy. Most important, Grube never indicates what edition of the Greek text he is using. The only edition in the bibliography is Adam, but it is quite clear that Grube usually follows Burnet (although not at 533e, 568d, or 574d). In particular, Grube seems to accept from Burnet (against Adam) the authority of the manuscript Vindobonesis F. This is intelligible, but some note of the controversy seems fitting, especially when there are important points where this choice – as well as other differences with Adam – transforms the meaning (e.g. 362d, 365e, 515b, 585c). Indeed Grube never lets the reader know about manuscript difficulties or possible alternate readings that would seriously affect the content of what is said (see especially 533e, 574d).

All in all, this is a competent and helpful translation, useful for the classroom but not quite accurate enough for the serious student intent on understanding Plato's political philosophy.

THOMAS L. PANGLE

Yale University

Listing and Order Form for Publications of the AMERICAN POLITICAL SCIENCE ASSOCIATION

THE AMERICAN POLITICAL SCIENCE REVIEW. Quarterly journal of scholarly articles and book reviews in political science. Included in APSA membership. Back issues: $10.50 per copy; $42.00 per volume.

PS. Quarterly journal of Association news and articles of professional concern. Included in APSA membership. Back issues: $2.00 per copy; $8.00 per volume. ($5.00 for the Spring issue containing the preliminary program.)

DEA NEWS FOR TEACHERS OF POLITICAL SCIENCE. Newspaper on education and the curriculum. Included in APSA membership.

ANNUAL MEETING PROGRAM. Final program for APSA Annual Meeting. Included in Annual Meeting Registration. Extra copies: $1.50.

APSA ANNUAL MEETING PAPERS.** Hard copies, $3.00 each.

APSA ANNUAL MEETING PROCEEDINGS.** Microfilm reels of all papers, 1901-1912; 1956-present. $22.00 per reel (approx. 7 reels per meeting).

CUMULATIVE INDEX TO THE AMERICAN POLITICAL SCIENCE REVIEW.** 1906-1968, $6.50.

CUMULATIVE INDEX TO THE PROCEEDINGS OF THE ANNUAL MEETINGS.** Key word index to all papers included in proceedings of Annual Meetings for 1904-1912; 1956-1970; $18.50.

WOMEN IN POLITICAL SCIENCE: STUDIES AND REPORTS, 1967-71 OF THE APSA COMMITTEE ON THE STATUS OF WOMEN IN THE PROFESSION.** PB 486, $6.00 microfilm; $16.50 Xerographic reprint (U.S. prices).

POLITICAL SCIENCE THESAURUS. A major reference tool in political science; this volume represents the terminology control device for a computer-based information retrieval service. Individuals: Hardback, $20.00; paper, $15.00. Institutions: Hardback, $30.00; paper, $25.00.

APSA BIOGRAPHICAL DIRECTORY (Sixth Edition, 1973). Biographical information on over 7,000 Association members, plus names and addresses for approximately 12,500 members. Members: Hardback, $7.50; paper, $5.00. Non-Members: Hardback, $12.50; paper, $10.00.

APSA DIRECTORY OF MEMBERS: ANNUAL SUPPLEMENT TO THE APSA BIOGRAPHICAL DIRECTORY. Names and current addresses of all APSA members. $3.00 each.

GUIDE TO PUBLICATION IN POLITICAL SCIENCE. Co-sponsored by the APSA Committee on the Status of Women and the Women's Caucus for Political Science. $1.00.

ROSTER OF WOMEN IN POLITICAL SCIENCE (4th edition). Biographical information on women political scientists. $3.00 each for members; $5.00 for non-members.

ROSTER OF CHICANOS IN POLITICAL SCIENCE. Biographical information on Chicano political scientists. $3.00 for members; $5.00 for non-members.

ANNUAL APSA DIRECTORY OF DEPARTMENT CHAIRPERSONS. Names and addresses of Chairpersons of departments offering political science at four-year institutions. $20.00 each.

ANNUAL APSA SURVEY OF DEPARTMENTS. Annual report on the results of an extensive questionnaire sent to all four-year institutions offering political science. Includes salary information, enrollment trends and many other items of current interest. $20.00 each.

PERSONNEL SERVICE GUIDELINES FOR EMPLOYERS AND APPLICANTS. Pamphlet containing information and guidelines for members of the APSA Personnel Service and for employers listing their vacancies in the Personnel Service Newsletter. No charge.

PERSONNEL SERVICE NEWSLETTER. Monthly listing of positions for political scientists. APSA Members: $8.00 subscription.

CAREERS AND THE STUDY OF POLITICAL SCIENCE: A GUIDE FOR UNDERGRADUATES. A monograph discussing the relationship between political science knowledge and skills and careers in law, government, business, journalism, teaching, etc. $.50 each; bulk rates available.

**Order from: Customer Service Department
University Microfilms
300 North Zeeb Road
Ann Arbor, Michigan 48106

CAREER ALTERNATIVES FOR POLITICAL SCIENTISTS: A GUIDE FOR FACULTY AND GRADUATE STUDENTS, by Thomas E. Mann. A monograph containing practical advice for obtaining nonacademic employment. $1.00 each.

GUIDE TO GRADUATE STUDY IN POLITICAL SCIENCE. Compilation of Ph.D. and Masters programs in political science. The 1977 Guide is revised and expanded to include listings of faculty and fields of specialization. $4.00 each.

RESEARCH SUPPORT FOR POLITICAL SCIENTISTS. A monograph containing detailed listings of public and private agencies that fund research fellowships, grants, and contracts. Includes section on strategic thinking about funding research and a guide to preparing research proposals. $4.00 each.

STORMING WASHINGTON: AN INTERN'S GUIDE TO NATIONAL GOVERNMENT by Stephen Frantzich. A monograph on Washington, D.C. internships. $2.00 each.

GLOBAL DIMENSIONS IN U.S. EDUCATION. A joint publication of the APSA Committee on Pre-Collegiate Education, the Education Commission of the International Studies Association, and the Center for War/Peace Studies of the N.Y. Friends Group, Inc. Four publications: The Elementary School, The Secondary School, The University, The Community. $1.50 each, $5.00 per set.

COMPARING POLITICAL EXPERIENCES. Complete description of an alternative course for high school government instruction. $2.75 each.

SETUPS: AMERICAN POLITICS (Supplementary Empirical Teaching Units in Political Science). New computer related instructional materials whereby students learn important substantive topics in American politics as they learn methods of analysis. *Voting Behavior: The 1972 Election* by Bruce D. Bowen, C. Anthony Broh, Charles L. Prysby. *Political Socialization Across the Generations* by Paul Allen Beck, Jere W. Bruner, L. Douglas Dobson. *Political Participation* by F. Christopher Arterton, Harlan Hahn. *Representation in the United States Congress: 1973* by Ray A. Geigle, Peter G. Hartjens. *The Supreme Court in American Politics: Policy Through Law* by John Paul Ryan, C. Neal Tate. *U.S. Energy, Environment and Economic Problems: A Public Policy Simulation* by Barry Hughes. *The Dynamics of Political Budgeting: A Public Policy Simulation* by Marvin K. Hoffman. For information and price list write: APSA Division of Educational Affairs. $3.50 each; lower price on bulk orders.

INSTRUCTIONAL RESOURCE MONOGRAPHS. Guides on materials and methods for teaching and learning in political science. *A Guide to Library Sources in Political Science: American Government* by Clement E. Vose, $6.50 hardback, $4.00 paperback. *PSI and Political Science: Using the Personalized System of Instruction to Teach American Politics,* edited by Ralph B. Earle, Jr., $3.50 paperback. *Simple Simulations,* by Charles Walcott with Anne Walcott, $3.50 paperback. *Computer-Assisted Instruction in Political Science,* edited by Jonathan Pool, $4.00 paperback. *U.S. Census Data for Political and Social Research: A Resource Guide,* $4.00, and *U.S. Census Data for Political and Social Research: A Student Manual,* $2.50, both by Phyllis G. Carter. *The Case of the Unreturned Cafeteria Trays* by Lloyd S. Etheredge, $1.00 each.

LEGISLATIVE SERVICE PROJECT MANUALS. Individual manuals available for: California, Kansas, Kentucky, Montana, Ohio, Oregon, Rhode Island. $2.50 each.

POLITICAL SCIENCE AND STATE AND LOCAL GOVERNMENT. A volume on the current and future state of research and educational programs in the field of state and local government. $3.75 each.

APSA MEMBERSHIP MAILING LISTS AND LABELS. For information and price list, contact Membership Secretary, APSA.

APSA POLITICAL SCIENCE DEPARTMENT CHAIRPERSONS MAILING LABELS. For information and price list, contact APSA Departmental Services Program.

Order Form

I am enclosing a check for $________________, for the following publications: ____________________

____

____

Name ____

Address ____

City ______________________________ State ________________ Zip __________

All orders must be prepaid.

Order from: American Political Science Association
1527 New Hampshire Avenue, N.W.
Washington, D.C. 20036

New from Penguin

Mao Tse-Tung and China
C. P. FitzGerald

C. P. FitzGerald, one of the world's foremost Sinologists, traces the career and achievements of Mao Tse-Tung, from his early days to his death in 1976. He provides a fascinating account of how Mao revolutionized Chinese society, transforming the world's most populous country from a state of anarchy to a unified nation. A Pelican Book
197 pages 0-14-021947-1 $2.50

The Whores of War
Mercenaries Today
Wilfred Burchett and Derek Roebuck

An important new book which explores the current resurgence in the use of mercenaries in international conflict. Focusing on the Luanda trial of 1976 and the civil war in Lebanon, the authors highlight the political implications of the role of these "soldiers of fortune" and their recruiters, as well as the historical, legal, and moral aspects of their work. A Pelican Book.
240 pages 0-14-022027-5 $2.95

PENGUIN BOOKS
625 Madison Avenue
New York, N.Y. 10022

Two new volumes in the Perspectives on Southern Africa Series

Land and Racial Domination in Rhodesia

Robin Palmer

Land has always provided the lifeblood of Rhodesian politics. From the first day of European occupation of Mashonaland in 1890, when land was immediately parcelled out to the incoming settlers, to the present situation of guerilla incursions into Rhodesia, the land question has been a major determinant of black-white relations. Palmer analyzes the way in which Europeans have used control over land to secure a position of economic and political dominance closely modeled on the South African pattern.

Perspectives on Southern Africa, No. 24

319 pages, 8 photos, 13 tables, 5 maps, $15.75

The Black Homelands of South Africa

The Political and Economic Development of Bophutatswana and KwaZulu

Jeffrey Butler, Robert I. Rotberg, and John Adams

With the independence of the Transkei in the fall of 1976, South Africa has set in motion what it hopes will be a final realization of the grand design of separate development. The success of South African strategy depends to a large extent upon the political and economic viability of its homelands and exhomelands. This book is the first to examine this topic by means of case studies of two of the three largest homelands.

Perspectives on Southern Africa, No. 21

260 pages, 20 tables, 3 maps, $12.50

The Antelope

The Ordeal of the Recaptured Africans in the Administrations of James Monroe and John Quincy Adams

John T. Noonan, Jr.

In 1820, *the Antelope*, a ship containing 281 humans in chains, was intercepted off the Florida coast. For the next eight years whether these beings were persons or property was disputed in the American courts. This is the story of these Africans and of the Americans who became involved in the case.

205 pages, $10.00

Labor Market and Wage Determination

The Balkanization of Labor Markets and Other Essays

Clark Kerr

These nine essays by a noted educator and former industrial arbitrator are fresh and vigorous. They combine theory with practical experience in a most convincing way.

240 pages, $12.00

The Huk Rebellion

A Study of Peasant Revolt in The Philippines

Benedict J. Kerkvliet

This is a study of why peasants revolt. Because Kerkvleit's account of the Huk rebellion considers its evolution in both a local and a regional framework, it should interest not only readers concerned with Philippine society but also those involved in political science and comparative peasant studies.

322 pages, 15 tables, 4 maps, $16.00

Bureaucrats Politicians, and Peasants in Mexico

A Case Study in Public Policy

Merilee Serrill Grindle

Grindle analyzes the activities of one important federal agency as its officials planned and pursued a new policy for rural development under the Echeverria administration (1970-1976).

239 pages, 34 tables, 13 figures, $12.50

At bookstores

UNIVERSITY OF CALIFORNIA PRESS
BERKELEY 94720

RECENT BOOKS FROM HARPER & ROW

MARINA AND LEE

By Priscilla Johnson McMillan. The work of a Russian scholar who both interviewed Oswald in Moscow and worked on John F. Kennedy's senatorial staff in the 1950s is a deeply convincing psychological portrait of Lee Harvey Oswald, his motives and actions, and his life with his Russian wife Marina.
Illustrated. $15.00

UNCERTAIN GREATNESS
Henry Kissinger & American Foreign Policy

By Roger Morris. A former National Security Council staff member has written a critical analysis "that throws much new light on the horrors of the Nixon-Kissinger years."
—*Anthony Lewis, N.Y. Times.*
$10.95

THE INTERNATIONAL MONETARY SYSTEM 1945-1976
An Insider's View

By Robert Solomon, Senior Fellow, The Brookings Institution. "An authoritative and insightful account of what has happened to the world's money." — *Walter H. Heller.*
$17.50

THE CUBAN REVOLUTION

By Hugh Thomas. The history of Cuba since 1952 and the final half of the author's brilliant and encyclopedic *Cuba: The Pursuit of Freedom.* "A moving evocation of a diverse and tragic history." —*Arthur Schlesinger, Jr.*
Illustrated. Paperback, $9.95

ORGANIZING FOR PEACE

A Personal History of the Founding of the United Nations

By Clark M. Eichelberger; foreword by George W. Ball. "Full of insights into the history of the period and the important role of non-governmental organizations in the building of the U.N." —*Francis O. Wilcox, The Atlantic Council.* $12.50

GLOBAL FRACTURE

The New International Economic Order

By Michael Hudson. "Angry, brilliant, convention-shattering… Hudson is one of a tiny handful of economic thinkers…who are forcing us to look at old questions in startling new ways." —*Alvin Toffler.* $12.50

THE BUSINESSMAN'S GUIDE TO THE MIDDLE EAST

By Lillian Africano. Skillfully probing the differences between American corporate structure and Arab business hierarchies, this information-packed guide anticipates both the short-and-long-term problems faced by Western business travelers in the Mid-East. Maps. $12.95

RUSSIAN SELF-PORTRAITS

By David Attie. Presented with a life-size mirror and a camera they could activate themselves, Russian families, couples, and individuals attending a USIA traveling photography show in Kiev took pictures they wished to present to the world. The self-portraits shown here reveal much about a people we hardly know. Illustrated. Paperback, $6.95

THE HARPER DICTIONARY OF MODERN THOUGHT

Edited by Alan Bullock and Oliver Stallybrass. In creating this unparalleled one-volume reference library, British and American experts in the fields of science, philosophy, religion, psychology, literature, art, music, politics, history, and sociology cover the entire spectrum of 20th century thinking. Over 4,000 essay-articles. Indexed. $20.00

At bookstores

Harper & Row
10 E. 53rd St., New York 10022
1817

PROBLEMS OF WORLD MODELLING
Political and Social Implications
Karl W. Deutsch, Harvard University, Bruno Fritsch, Swiss Federal Institute of Technology, Andrei S. Markovits, Harvard University, and Helio Jaguaribe, University of Candido Mendes, Editors
First presented at a Harvard Seminar on Global Opportunities and Constraints for Regional Development, these papers focus on major trends in world economics, politics and ecology—concrete suggestions for planing and developing effective world models.
$17.50

SOFT ENERGY PATHS
Toward a Durable Peace
Amory B. Lovins , Friends of the Earth, Inc. (Published for Friends of the Earth, Inc.)
Based on his 1976 article in *Foreign Affairs*—which *Energy Daily* called "a seminal chapter in the history of energy policy"—Mr. Lovins proposes a coherent non-nuclear energy policy. Previous books have treated specialized topics piecemeal—energy conservation, alternative technologies, nuclear concerns, and the structural problems of our society. This book, for the first time, ties all the issues together—engineering, economic, and thermodynamic concepts are thoroughly but concisely treated along with the social, environmental, and political perspectives.
$15.00

JUSTICE AND PUNISHMENT
Edited by J.B. Cederblom and William L. Blizek, University of Nebraska
The interface between criminal justice and political philosophy is the dominant theme of these papers which reflect the strong current interest in development of the retributivist or "just deserts" theory of punishment. They include a review of important issues in punishment and corrections, an assessment of the rationale for the retributive theory of punishment, and an examination of both theoretical and practical aspects of the retributivist theory. Contributions by John Hospers, Hugo A. Bedau, Edmund L. Pincoffs, Martin P. Golding, James Q. Wilson, David Fogel, Norval Morris, and Richard Wasserstrom.
$16.50

ASSESSING THE CRIMINAL
Edited by John Hagel and Randy Barnett, Harvard University
These papers constitute a systematic inquiry into new approaches to the soution of problems in the U.S. criminal justice system. Leading scholars examine alternatives within a framework of two basic questions: "What are the major factors influencing criminal behavior?", and "What strategies are most effective in reducing crime rates?".
$16.50

YOUTH CORRECTIONS IN MASSACHUSETTS
Center for Criminal Justice, Harvard UniUniversity Law School
Sweeping reforms in youth corrections in the Commonwealth of Massachusetts have made that state's Department of Youth Services a highly visible national symbol for a new policy approach. For the Harvard Center for Criminal Justice, this tumultous period of crisis, reform, and reaction in state correctional policy (chiefly the repudiation of training schools and advocacy of community-based services) afforded a unique opportunity for observation and evaluation. In this seven year study, seventeen separate data-gathering efforts generated information for detailed and intensive analyses of various aspects of the process: recidivism, program dynamics, relations between youths and the Division of Youth Services staff, politics of reform and counter-reform movements. The five books in this series are a product of this definitive study. They offer insightful perspectives and a body of knowledge which will be a useful guide to other states in improving the effectiveness of youth corrections.

REFORMING JUVENILE CORRECTIONS
The Massachusetts Experience
Lloyd E. Ohlin, Robert B. Coates, and Alden D. Miller, Center for Criminal Justice, Harvard Law School
ca. 450 pages $15.00 (est.)

A THEORY OF SOCIAL REFORM
Correctional Change Processes in Two States
Alden D. Miller, Lloyd E. Ohlin, and Robert B. Coates, Center for Criminal Justice, Harvard Law School
ca. 432 pages $15.00 (est.)

DIVERSITY IN A YOUTH CORRECTIONAL SYSTEM
Handling Delinquents in Massachusetts
Robert B. Coates, Alden D. Miller, and Lloyd E. Ohlin, Center for Criminal Justice, Harvard Law School
ca. 400 pages $15.00 (est.)

DESIGNING CORRECTIONAL ORGANIZATIONS FOR YOUTH
Dilemmas of Sub-Cultural Development
Craig A. McEwens, Bowdoin College
ca. 400 pages $15.00 (est.)

NEUTRALIZING INMATE VIOLENCE
Juvenile Offenders in Institutions
Barry C. Field, University of Minnesota
ca. 400 pages $15.00 (est.)

Ballinger
PUBLISHING COMPANY
17 Dunster Street, Harvard Square, Cambridge, MA 02138

The Shotgun behind the Door

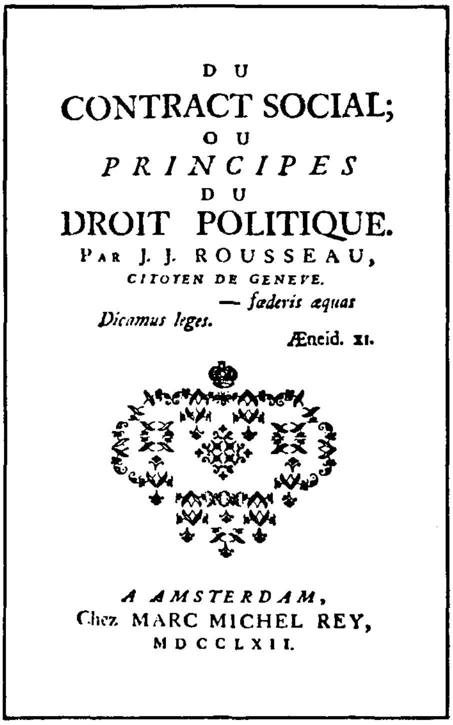
Liberalism and the Problem of Political Obligation

Philip Abbott reformulates the liberal response to the problem of political obligation by accepting the initial premises of liberal ethical theory and the political impulses which emerge from it while discarding the theories of political obligation that have been attached to it. "This is a genuinely original contribution to its field" — Russell Kirk. 212 pages / $8.00

Rousseau's Political Philosophy

An Exposition and Interpretation

In his careful analyses of *Discourse on the Origins and Foundations of Inequality among Men, Discourse on Political Economy,* and *The Social Contract,* **Ramon M. Lemos** refutes the claims of critics that Rousseau's writings abound with obscurities and inconsistencies. By providing detailed clarification of the development of the philosopher's political theory, the difficulties presented by the successive texts, and the apparent contradiction between them, the book presents Rousseau's work as an intelligible and coherent system of thought. 344 pages / $14.50



DU
CONTRACT SOCIAL;
OU
PRINCIPES
DU
DROIT POLITIQUE.
PAR J. J. ROUSSEAU,
CITOYEN DE GENEVE.

— fœderis æquas
Dicamus leges.
Æneid. XI.

A AMSTERDAM,
Chez MARC MICHEL REY,
MDCCLXII.

The Press, Politics, and Patronage

The American Government's Use of Newspapers, 1789–1875

During the first century of the American republic a unique relationship existed between the nation's newspapers and the government, political leaders, and political parties. **Culver Haygood Smith** chronicles the story of this relationship in a comprehensive history of government patronage of the press from the time Hamilton and Jefferson enlisted Fenno and Freneau as editors of opposing weeklies until official executive patronage of the press ended a century later. 400 pages / $18.50

The University of Georgia Press

Athens 30602

Fall Choices From Cambridge

THE POLITICAL WORKS OF JAMES HARRINGTON

J. G. A. POCOCK, Editor

Pocock reassesses the evidence concerning Harrington's life and writings in this first modern edition of his complete political writings that includes his best-known work *Oceana. Cambridge Studies in the History and Theory of Politics* $49.50

DOCUMENTS ON CONTEMPORARY BRITISH GOVERNMENT

MARTIN MINOGUE, Editor

Volume 1: British Government and Constitutional Change

Covers parliamentary government, the governing of the public sector and the control of government, among other topics.
Hardcover $23.50 Paper $6.95

Volume 2: Local Government in Britain

The main characteristics of local government systems in Britain during a period of far-reaching reform and reorganization are illuminated in this carefully edited selection of documents.
Hardcover $23.50 Paper $6.95

THE LIBERAL MIND 1914-1929

MICHAEL BENTLEY

Cambridge Studies in the History and Theory of Politics $19.95

POLITICAL PHILOSOPHY AND RHETORIC

A Study of the Origins of American Party Politics

JOHN ZVESPER

Cambridge Studies in the History and Theory of Politics $16.95

CHINA'S ECONOMIC REVOLUTION

ALEXANDER ECKSTEIN

"...a well-informed, compact, balanced, and lucid account of China's modern economy that is unquestionably the best introduction to the subject."—*Library Journal* "It should be a standard source for teachers and students of the Chinese economy for years to come."— *Lloyd G. Reynolds, Sterling Professor of Economics, Yale University* Hardcover $19.95 Paper $6.95

MAO TSE-TUNG IN THE SCALES OF HISTORY

DICK WILSON, Editor

In a penetrating and lively analysis, eleven scholars of international renown assess Mao's career in its most interesting facets. *Contemporary China Institute Publications*
Hardcover $19.95 Paper $5.95

THE MARXIST CONCEPTION OF IDEOLOGY

A Critical Essay

MARTIN SELIGER

Professor Seliger contends that Marx and Engels failed to abide by the Marxist "dogmatic" conception of ideology in their analysis of economic and political phenomena. *International Studies 8*
$13.95

FEUERBACH

MARX W. WARTOFSKY

The first major full-length study to appear in English concerns the development of Feuerbach's philosophical thought and his place in the history of 19th century philosophy. $24.95

Cambridge
University Press
32 East 57th Street, New York, N.Y. 10022

"Cummings and Wise have done it again!"

DEMOCRACY UNDER PRESSURE

An Introduction to the American Political System

THIRD EDITION

MILTON C. CUMMINGS, JR., The Johns Hopkins University
DAVID WISE, Author and political analyst

Presenting some enthusiastic responses to the new Third Edition of this superb bestselling introduction to American government... thoroughly revised *in every chapter* to extend its coverage of contemporary events to the first months of the Carter administration:

"Cummings and Wise have done it again! *Democracy Under Pressure* meets the need for a readable, comprehensive and candid coverage of American politics. I'm going to adopt the Third Edition, just as I did the First and Second." —JERRY SULLIVAN, Lambuth College

"The Third Edition is an informative, up-to-date, and readable account of our political system — a worthy successor to the first two editions. This is as fine an American government textbook as there is."
—ROBERT W. LANGRAN, Villanova University

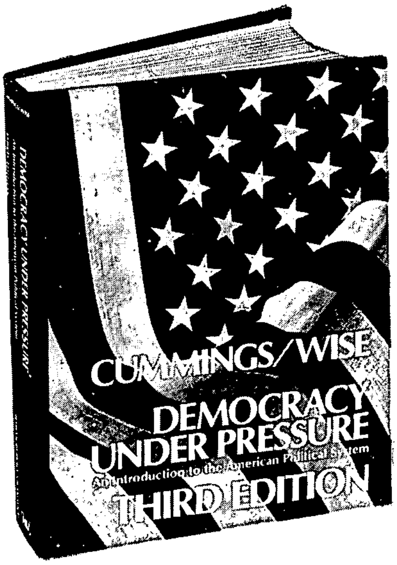
"A sophisticated yet easily understood analysis of contemporary political and social issues. This is far and away the best introductory text I have seen."
—RICHARD AULT, Simpson College

"The new edition looks good — even better than the Second Edition. The book is written on a level of clarity and with a degree of sophistication that makes it outstanding as a teaching/learning instrument for introductory students."
—JACK C. PLANO, Western Michigan University

"The best introductory textbook that I have seen."
—VIRGINIA R. MEHAFFEY, Blanton's College

716 pages

Accompanied by *Test Booklet* with Lecture and Discussion Topics and Learning Objectives



Available in January 1978 for spring classes:

a new STUDY GUIDE by the authors

Provides for each chapter of the textbook: *key points* giving an overview of the chapter; a *glossary,* keyed to the textbook, with space under each item for students to attempt their own definitions; a *chronology* of important events wherever a sense of historical development is meaningful; a *self-quiz* consisting of true/false, multiple-choice, and short-answer questions; a *learning unit* providing an in-depth self-test on one particular topic; *learning objectives;* and inventive *study exercises.*
Paperbound. 224 pages (probable)

HBJ

HARCOURT BRACE JOVANOVICH, INC.
New York • San Diego • Chicago • San Francisco • Atlanta

P.C.I.
LIBERTAS
M.S.I
DEMOCRAZIA PROLETARIA
PLI
PARTITO SOCIALISTA ITALIANO
PSI
partito radicale
SOCIALISMO

REPRESENTATION IN ITALY

Institutionalized Tradition and Electoral Choice

Samuel H. Barnes

Analyzing the results of a massive empirical study of Italian voter behavior, Barnes offers vital insights into electoral politics and national policy making.

208 pages Cloth $16.00 September

REACHING THE PEASANT FARMER

Organization Theory and Practice in Kenya

David K. Leonard

Leonard provides insights and solutions for problems of agricultural extension management in Africa and much of the developing world.

"Leonard's analysis of problems in the administration of rural development is both imaginative and persuasive."—Norman Uphoff, Cornell University

320 pages Cloth $19.00 September

HAROLD D. LASSWELL ON POLITICAL SOCIOLOGY

Edited and with an Introduction by Dwaine Marvick

Lasswell's intellectual framework, characteristic style, and diverse methods are revealed in these selections reflecting his central political interests.

Heritage of Sociology series, edited by Morris Janowitz

496 pages Cloth $22.00 September

BY COLOUR OF LAW

Legal Culture and Constitutional Politics in England, 1600-1689

Howard Nenner

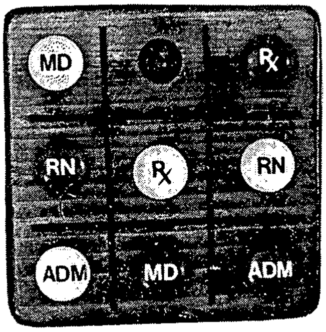
An incisive explication of how the use of legal concepts in royal and parliamentary politics translated revolution into constitutional regularity.

"*By Colour of Law* is an excellent monograph in the domain of constitutional and legal history. . . . "—Thomas A. Green, Law School, University of Michigan

xx, 252 pages Cloth $16.00 July

The University of Chicago PRESS

Now in Paper



HEALTH CARE POLITICS

Ideological and Interest Group Barriers to Reform

Robert R. Alford

Winner of the 1975 Woodrow Wilson Award from the American Political Science Association

"An important, exciting, and intriguing book. . . . Any future research in this area will have to take account of [it]." —Allen W. Imershein, *Social Forces*

xiv, 294 pages Paper $4.95 August

THE ARGUMENT AND THE ACTION OF PLATO'S *LAWS*

Leo Strauss

With a Foreword by Joseph Cropsey

"Provides the reader with . . . innumerable insights which obviously flow from a long and careful probing of that text."—Allan D. Nelson, *Canadian Journal of Political Science*

x, 186 pages Paper $3.95 August

THE IMPACT OF HITLER

British Politics and British Policy, 1933-1940

Maurice Cowling

"A cogent analysis of the connexion between party politics and foreign policy . . . based on an immense knowledge of primary and secondary sources."—*Times Literary Supplement*

x, 446 pages Paper $7.95 August

TOO MUCH OF A GOOD THING

John Sparrow

A provocative and humane discussion dealing with broad issues of equality, humanity, and liberty in the context of contemporary social dilemmas.

104 pages Paper $2.95 September

SOCIAL CONTROL OF THE WELFARE STATE

Morris Janowitz

"This imaginative and stimulating discussion of some of the central dilemmas of the modern welfare state is a welcome contribution . . . . "—Lewis A. Coser, *Political Science Quarterly*

xxii, 170 pages Paper $3.95 September

AGRARIAN REVOLT IN A MEXICAN VILLAGE

Paul Friedrich

With a Preface, 1977

Friedrich describes agrarian reform in a Mexican Indian village in the 1920s, raising important questions about local politics in all peasant societies.

xvi, 160 pages Paper TBA

Chicago 60637

The University of Chicago Press

New Viewpoints

FOR YOUR COURSES IN
POLITICAL SCIENCE

NEW

YOUTH AND CHANGE IN AMERICAN POLITICS
By Louis M. Seagull, formerly, University of Pennsylvania
Views the political behavior of youth as following certain generational patterns, with youth as an agent for social, cultural, and political change—not as a consequence of recent extraordinary events, but as part of a basic cyclical societal evolution.
Pub. date: November 1977
Clothbound: $12.50
Paperback: $5.95

NEW

ST. LOUIS
Edited by Selwyn K. Troen, Ben Gurion University, and Glen E. Holt, Washington University
A history of the city told through documents, articles, and other source materials. A volume in the *Documentary History of American Cities* series.
Pub. date: November 1977
Clothbound: $15.00
Paperback: $6.95

New Viewpoints
A Division of Franklin Watts, Inc.
730 Fifth Avenue
New York, N.Y. 10019

NEW

CRIMINAL JUSTICE THROUGH SCIENCE FICTION
Edited by Joseph D. Olander, Special Assistant to the Florida Commissioner of Education, and Martin H. Greenberg, University of Wisconsin at Green Bay
Thirteen gripping science fiction stories are used to illustrate three aspects of the field of criminal justice.
Pub. date: November 1977
Clothbound: $15.00
Paperback: $6.95

THE CONDUCT OF AMERICAN FOREIGN RELATIONS: The Other Side of Diplomacy
By Thomas H. Etzold, Naval War College
Clothbound: $10.00
Paperback: $5.95

THE GRASS-ROOTS MIND IN AMERICA
By Conal Furay, Webster College
Clothbound: $10.00
Paperback: $5.95

HARRY TRUMAN'S CHINA POLICY: McCarthyism and the Diplomacy of Hysteria, 1947-1951
By Lewis McCaroll Purifoy, Emory and Henry College
Clothbound: $15.00
Paperback: $6.95

A special interest group... from Halsted Press.

TURMOIL AND CONSENSUS
The World of Politics

Edited by **Barbara Callaway** & **Alan Stone,**
both of Rutgers University

This collection of readings considers political change, obligation, and state sovereignty while retaining important coverage of political philosophy and sociology. Discusses the relationships between politics and other social sciences, sources of ideologies, and issues of power/domination. CONTENTS: **Introduction: What Are Political Issues? Causes of Political Change** — Ideology and Change. Causes of Revolutionary Change. Tactics and Revolutionary Change. Change through Reform. **Domination and Political Change** — Racism. Colonialism. Imperialism. Nationalism. Sexual Dominance. **Political Change and Stability** — The Virtues of Stability. Political Leadership and Stability. The Tension between Democracy and Stability. **The Future of Politics** — The Western World. The Non-Western World. (A Schenkman publication)

(0 470 13015-6) 1977 approx. 320 pp.
$14.50 cloth
(0 470 13016-4) $ 6.95 paper

THE NEW POLITICAL ECONOMY
The Public Use of the Private Sector

Edited by **Bruce L.R. Smith,**
Columbia University

Explains how to reconcile democratic accountability with private freedom. Details numerous specific problem areas including health care and traditional federalism. (Rights: U.S.)

(0 470 15157-9) 1977 344 pp. $4.95 paper

PSYCHOPOLITICAL ANALYSIS
Selected Writings of Nathan Leites

Nathan Leites, *Rand Corporation*
Edited by **Elizabeth Wirth Marvick**

Analyzes the moral assumptions and operational codes of today's most influential political elites from a psychoanalytical perspective. Examines the leadership of French, Soviet and Chinese Communist, Vietnamese and Viet Cong, and Nazi regimes, with particular focus on international trends in moral sentiment and susceptibility to propaganda. (A Sage publication)

(0 470 99058-9) 1977 384 pp. $20.00

TRADE AND DEVELOPING COUNTRIES

Kathryn Morton & **Peter Tulloch,**
General Agreement on Tariffs and Trade Secretariat

Introduces the trading problems of developing countries. Examines the rapid growth of trade in manufactures from some developing countries, protection problems, and the possibilities for trade liberalization through GATT and UNCTAD. Also surveys the growing role of the multinational corporation in the export trade of developing countries, the advantages and disadvantages of relying on these firms, and the techniques used by developing countries to control their activities. (Rights: Western Hemisphere)

(0 470 99054-6) 1977 376 pp. $19.95

HALSTED PRESS
a division of John Wiley & Sons, Inc.
605 Third Avenue
New York, New York 10016

Mail to: Dept. APSR-1
HALSTED PRESS
a division of John Wiley & Sons, Inc.
605 Third Avenue,
New York, New York 10016

Please send me the books indicated below for *30 days free* examination.

○ Callaway & Stone, TURMOIL/(0 470 13015-6) cloth
○ Callaway & Stone, TURMOIL/(0 470 13016-4) paper
○ Smith, POLITICAL ECONOMY/ (0 470 15157-9) paper
○ Leites & Mavrick, PSYCHOPOLITICAL/ (0 470 99058-9)
○ Morton & Tulloch, COUNTRIES/ (0 470 99054-6)

☐ PAYMENT ENCLOSED + applicable sales tax. Halsted pays postage/handling. We usually ship within 10 days. If payment accompanies order and shipment cannot be made within 90 days, full payment will be refunded. Foreign payment must be made in U.S. currency, by U.S. bank draft, international money order, or UNESCO coupons. **If your order totals $75 or more, attach a purchase order or enclose 25% partial payment.**
☐ BILL ME + postage/handling.
☐ BILL INSTITUTION + postage/handling.

Please print.
NAME ____________________
AFFILIATION ____________________
ADDRESS ____________________
CITY ____________ STATE ______ ZIP ______

Prices subject to change without notice and slightly higher in Canada.
IN CANADA: John Wiley & Sons Canada, Ltd.
22 Worcester Road,
Rexdale, Ontario

A 8179-67

The politics of business.

STORM OVER THE MULTINATIONALS
The Real Issues
Raymond Vernon

"The literature of the multinational corporation is the vast swampland of economic writing... Vernon's work is the exception, and this admirable survey and summary is where one should begin on the subject and possibly also end."
—*John Kenneth Galbraith*
$12.50

The business of politics.

INTERPRETATIONS OF FASCISM
Renzo De Felice
Translated by Brenda Huff Everett
Introduction by Charles F. Delzell

If you want to learn more about fascism, this is where to start. Renzo De Felice examines the nature, growth, and power of fascism; discusses the ways others have perceived it; and offers his own highly controversial interpretations.
$15.00

HARVARD UNIVERSITY PRESS Cambridge, Mass. 02138

Asian Profile

AN INTERNATIONAL JOURNAL ON ASIAN STUDIES FROM ASIA

SPECIAL FEATURES

- **Asian Profile** is a multi-disciplinary journal devoted exclusively to Asian affairs
- **Asian Profile** is an independent journal not affiliated to any organization, institution or agency.
- **Asian Profile** publishes articles from all view-points on all aspects of Asian Studies.
- **Asian Profile** provides an open forum for scholars both inside and outside Asia.
- **Asian Profile** promotes both research in, and the teaching of, Asian Studies.
- **Asian Profile** is designed to bridge the gap between the academics and the public.

FREQUENCY - Published 6 times a year in February, April, June, August, October and December.

LANGUAGE - All articles in English.

COVERAGE - China, Japan, Korea, Southeast Asia, South Asia and Southwest Asia.

CONTENT - Topics of current interest, explicitly illustrated with photographs, maps, diagrams and tables.

FORMAT - 7½" x 10½" size, about 600 pages per volume, with sturdy binding and quality printing.

SUBSCRIPTION RATES -
1 year (6 issues) US$25.00 postpaid
2 years (12 issues) US$45.00 postpaid
Back Issues:
Vol. 1, 1973 (3 issues) US$12.50 postpaid
Vol. 2, 1974 (6 issues) US$25.00 postpaid
Vol. 3, 1975 (6 issues) US$25.00 postpaid
Vol. 4, 1976 (6 issues) US$25.00 postpaid
Single copy US$4.50 postpaid

CONTRIBUTIONS ARE INVITED

Asian Profile welcomes original papers on any aspect of Asian Studies. Manuscripts submitted will be given prompt attention. Upon publication, authors will receive free 50 offprints and a copy of the journal in which their articles are published. Book reviews are also welcome.

Asian Research Service
P. O. BOX 2232, G. P. O.
HONG KONG

The facts behind the headlines

Now there's quick access to official documents on the most burning issues of today...and tomorrow

The R.R. Bowker Company proudly announces the **Public Documents Series.** The purpose of this brand-new series is quite simple and quite explicit: to provide students, scholars, and citizens with ready access to the most important government documents – reports, hearings, inquiries, position papers, etc. – pertaining to the most important problems of our time.

The need for this series is obvious. Most of the documents included are now out of print or, for practical purposes, inaccessible.

In each volume, the editors have included: (1) those documents that illuminate the historical development of the issues considered (2) those documents that define the parameters of the issues (3) those documents that will be consulted as sources for the resolution of the issues.

The editors provide bridging narrative to facilitate transition between the documents and to clarify the material. The editors also supply notes and references, identify the source of each document, and indicate if it is reprinted in its entirety or abridged. Each volume contains an index of included documents and a subject and name index.

The Intelligence Community: History, Organization, and Issues.

Compiled and edited by Tyrus G. Fain, in collaboration with
Katharine C. Plant and Ross Milloy.
Introduction by Senator Frank Church.
ISBN 0-8352-0959-8. 1977. 6¼x9″. 1,215 pages. $35.00

"The publication of this documentary collection makes eminent sense at this time. Not only has 'intelligence' for the first time become a major and continuing public concern, but it is unlikely that many new documents of serious interest will become available in the next several years.... This volume, as it stands, is a remarkably thorough collection and can be considered definitive as of this date." – Harry Rositzke, former CIA official and author of the March 1977 publication, **The CIA's Secret Operations.**

This volume is an unprecedented documentary record of the U.S. intelligence establishment drawn from official reports, hearings, inquiries and other studies released by the government from 1970-1975. Part I deals with the legal and historical underpinnings of the U.S. foreign intelligence effort. Part II treats the principle agencies of the intelligence community. Part III examines the broad public policy issues. Numerous indexes, appendixes, and narrative notes are included.

Federal Reorganization: The Executive Branch

Compiled and edited by Tyrus G. Fain in collaboration with
Katharine C. Plant and Ross Milloy.
Foreword by Bert Lance.
ISBN 0-8352-0981-4. 1977. 6¼x9". 790 pages. $32.50.

Here's a carefully edited compilation of the most important public domain documents pertaining to the reorganization of the Executive branch of the federal government. All of the documents – whether reprinted in their entirety or abridged – are drawn from a wide variety of congressional hearings and reports, Executive branch studies, and statements by members of the Congress which, for the most part, are now out-of-print or simply inaccessible. The material covers many of the subjects which the Carter Administration and Congress will have to examine and contains proposals which President Carter himself will utilize in the reorganization effort. The time span of the selection ranges from the issuance of the Ash Council report in 1971 to the election of Jimmy Carter as president.

Part I deals with the consolidation of Executive departments. Part II covers Regulatory reform. Part III deals with Sunset-systems and Zero-based budgets. Bridging narratives facilitate transition between documents and illuminate the material. The sources of all the documents are identified and numerous appendixes provide additional selected readings and bibliographies.

National Health Insurance

Compiled and edited by Tyrus G. Fain in collaboration with
Kathrine C. Plant and Ross Milloy.
Introduction by Senator Edward Kennedy.
ISBN 0-8352-0960-1. August 1977. 6¼"x9". c. 700 pages. $29.95 tent.

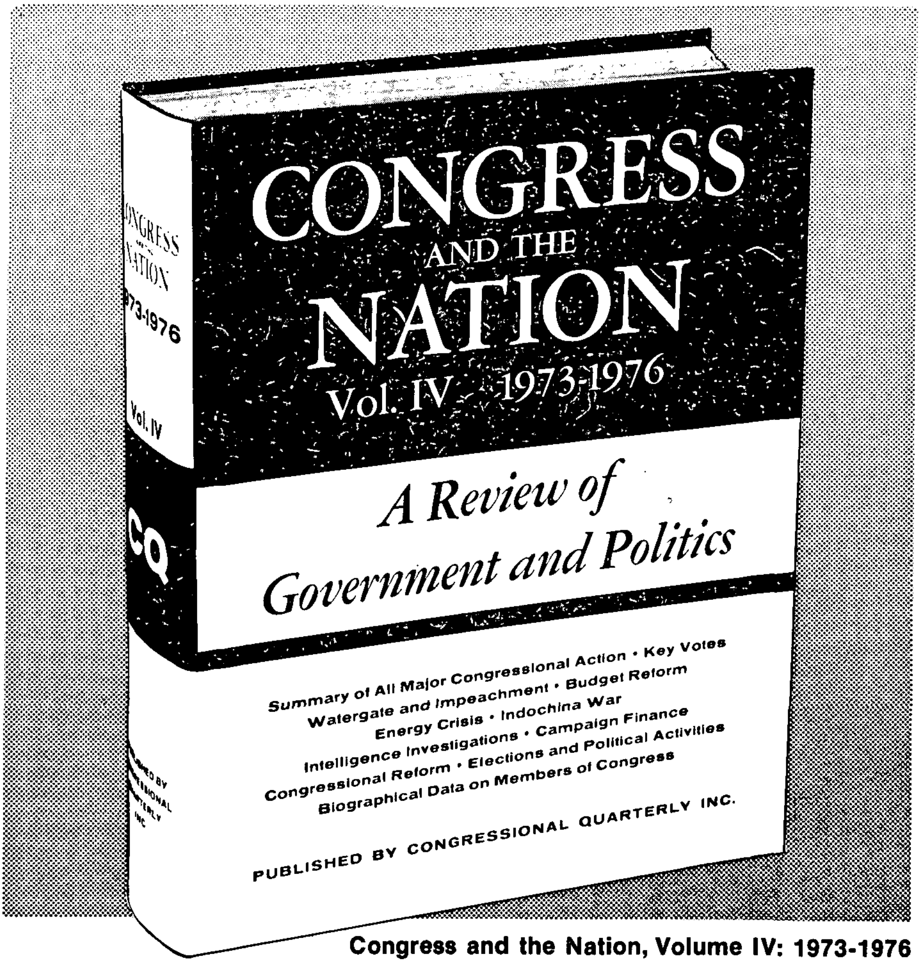
Here are the most important "public domain" documents pertaining to the organization and delivery of health care in America and the search for a successful, affordable nationwide program. All of the documents – whether reprinted in their entirety or carefully abridged – have been drawn from a large body of government documents and reports which, for the most part, are now out-of-print or, for practical purposes, inaccessible.

Part I deals with the issues of the national health insurance debate, the methods of financing such programs, and the health insurance experiences of other countries. Part II examines the key facts and components of America's health care industry such as health care institutions, public programs, and health care costs. As with previous volumes in this series, the editors have provided explanatory bridging narrative to facilitate transition between the documents and to elucidate the material. The editors have also supplied notes and references which identify the original sources. The volume includes a bibliography; appendixes containing a list of acronyms and the national health bills introduced; an index of included documents; and a subject and name index.

Order now from
R.R. Bowker Order Dept.
P.O. Box 1807, Ann Arbor, Michigan 48106

Shipping and handling included in list price. Sales tax added where applicable. (Price subject to change without notice and slightly higher outside the U.S. and its possessions. Outside Western Hemisphere: Bowker, Erasmus House, Epping, Essex, England.)

New Volume!



CONGRESS AND THE NATION
Vol. IV 1973-1976
A Review of Government and Politics
Summary of All Major Congressional Action • Key Votes
Watergate and Impeachment • Budget Reform
Energy Crisis • Indochina War
Intelligence Investigations • Campaign Finance
Congressional Reform • Elections and Political Activities
Biographical Data on Members of Congress
PUBLISHED BY CONGRESSIONAL QUARTERLY INC.
CQ

Congress and the Nation, Volume IV: 1973-1976
October 1977. Approx. 1,200 pages, 8½" x 11". $49.50

Congress and the Nation is a comprehensive summary and review of legislative, political and presidential developments since 1945. With the publication of Volume IV: 1973-1976, the series distills into 5,500 pages thirty-two years of Congressional Quarterly's authoritative reporting of Capitol Hill. **Congress and the Nation** now recounts and places in historical perspective all significant activities in government and politics during the administrations of Truman, Eisenhower, Kennedy, Johnson, Nixon and Ford. It provides summaries of all major legislation, key Supreme Court opinions, as well as biographical data on members of Congress and coverage of elections and politics.

Orders now being accepted for **Congress and the Nation, Volume IV** to ensure immediate delivery upon publication.

CQ

CONGRESSIONAL QUARTERLY INC.
1414 22nd St. N.W. Washington, D.C. 20037 (202) 296-6800

A Decade of Federal Antipoverty Programs

ACHIEVEMENTS, FAILURES, AND LESSONS

Edited by ROBERT H. HAVEMAN

A Volume in the INSTITUTE FOR RESEARCH ON POVERTY POVERTY POLICY ANALYSIS Series

CONTENTS: *R. H. Haveman*, Introduction: Poverty and Social Policy in the 1960's and 1970's—An Overview and Some Speculations. *L. M. Friedman*, The Social and Political Context of the War on Poverty: An Overview. *L. E. Lynn, Jr.*, A Decade of Policy Developments in the Income-Maintenance System. *H. M. Levin*, A Decade of Policy Developments in Improving Education and Training for Low-Income Populations. *K. Davis*, A Decade of Policy Developments in Providing Health Care for Low-Income Families. *P. E. Peterson and J. D. Greenstone*, Racial Change and Citizen Participation: The Mobilization of Low-Income Communities through Community Action. *E. J. Hollingsworth*, Ten Years of Legal Services for the Poor. *P. A. Wallace*, A Decade of Policy Developments in Equal Opportunities in Employment and Housing.

1977, 392 pp., Cloth: $17.00/£12.50
ISBN: 0-12-333250-8; Paper: $5.00/£3.55
ISBN: 0-12-333256-7

Revolution at Work

MOBILIZATION CAMPAIGNS IN CHINA

By CHARLES P. CELL

A Volume in the STUDIES IN SOCIAL DISCONTINUITY Series

This book presents the first quantitative research on China's mobilization campaigns. Using the best available data, it derives values for level of mobilization, achievements, and shortcomings of 36 selected campaigns. It uses these values to rate the utility for China's socialist transformation process of the three basic types of campaigns: economic, ideological, and struggle. Surprisingly, it finds economic campaigns (with the Great Leap Forward as one of the few exceptions) to be the most efficient, and struggle campaigns to be the least. It also provides historical and theoretical background on the campaigns, and discusses their future in the wake of Mao's death.

1977, 240 pp., $14.95/£10.65
ISBN: 0-12-164750-1

Social Change in a Peripheral Society

THE CREATION OF A BALKAN COLONY

By DANIEL CHIROT

A Volume in the STUDIES IN SOCIAL DISCONTINUITY Series

Based on an examination of the social history of the Romanian province of Wallachia between 1250 and 1917, this important book sets forth a particular evolutionary sequence that describes the different historical stages through which peripheral societies pass. In tracing the evolution of Wallachia from a simple "trading state" to a modern neo-colonial society, Professor Chirot cogently demonstrates that the typologies of modernism vs. traditionalism of capitalism vs. feudalism are not relevant in an Eastern European context—or, in fact, in the context of any society peripheral to the main world-systems to which it has been economically and politically tied.

1976, 192 pp., $12.50/£8.85
ISBN: 0-12-173150-2

Social Experimentation

A METHOD FOR PLANNING AND EVALUATING SOCIAL INTERVENTION

Edited by HENRY W. RIECKEN and ROBERT F. BORUCH

"The authors . . . are clearly experienced in both policy planning and research—and policy research —thereby lending the book realistic goals and credible methods for attaining them. If any book can win more converts to the concept of social experimentation, this one should."—*Contemporary Sociology*

"Altogether, this is a model interdisciplinary product from an SSRC and a good example, also, of useful collaboration between university professionals and government advisory and funding agencies. It should be kept at their right hand by social policy makers at all levels of government and private organizations and by the social scientists who help them design more effective programs."—*Political Science Quarterly*

1974, 329 pp., $16.75/£11.90
ISBN: 0-12-588150-9

Send payment with order and save postage plus 50¢ handling charge. Orders under $15.00 must be accompanied by payment.

Prices are subject to change without notice.

ACADEMIC PRESS, INC.

A Subsidiary of Harcourt Brace Jovanovich, Publishers
111 FIFTH AVENUE, NEW YORK, N.Y. 10003
24-28 OVAL ROAD, LONDON NW1 7DX

PRINCETON

PERUVIAN DEMOCRACY UNDER ECONOMIC STRESS

An Account of the Belaúnde Administration, 1963-1968

PEDRO-PABLO KUCZYNSKI

"This is one of the few detailed treatments of economic policy-making in Peru during the Belaúnde period. As an insider of that regime, the author provides valuable insights into the political and bureaucratic infighting of the period, and also into the complex personality of Fernando Belaúnde."—*Charles T. Goodsell, Southern Illinois University* $16.50

THE POVERTY OF REVOLUTION

The State and the Urban Poor in Mexico

SUSAN ECKSTEIN

"An impressive synthesis of survey research and historical analysis, Eckstein's study of the fate of Mexico's urban poor is enlivened throughout by a sharp debate between prevalent academic interpretations and a class perspective on politics and development."—*Maurice Zeitlin, University of California* $17.50

Now in Paperback

PEASANTS, POLITICS, AND REVOLUTION

Pressures toward Political and Social Change in the Third World

Joel S. Migdal • Paper, $4.95 • Cloth, $16.50

PRINCETON UNIVERSITY PRESS

Princeton, New Jersey 08540

Lexington Books

Youth and Politics in Japan
Joseph A. Massey, Dartmouth College
256pp. $18.50 LC 75-21304
ISBN 0-669-00163-5

One Nation, So Many Governments
Michael N. Danielson, Princeton University,
Alan M. Hershey, Mathematica, Inc.,
John M. Bayne, H.E.W.
Foreword by **Mitchell Svirdoff**
A Report to the Ford Foundation
160pp. $14.00 LC 76-53868
ISBN 0-669-01293-9

Public Administration and Public Policy
edited by **H. George Frederickson, Charles Wise**, Indiana University
A Policy Studies Organization Book
In Press Fall 1977 LC 76-53666
ISBN 0-669-00738-2

Local Economic Development
Curtis H. Martin, Merrimack College,
Robert Leone, Harvard University
In Press Summer 1977 LC 76-55537
ISBN 0-669-01319-6

The Political Economy of Fiscal Federalism
edited by **Wallace E. Oates**, Princeton University
368pp. $17.00 LC 76-24471
ISBN 0-669-00961-x

Political Generations and Political Development
edited by **Richard J. Samuels**, Harvard University
Foreword by **Myron Weiner, Samuel P. Huntington**
160pp. $14.00 LC 77-168 ISBN 0-669-01463-x

The Energy Syndrome
edited by **Leon N. Lindberg**, The University of Wisconsin
400pp. $14.00 LC 76-6772 ISBN 0-669-00662-9

Selected Readings on International Payoffs
edited by **Yerachmiel Kugel, Gladys W. Gruenberg**, St. Louis University
208pp. $16.50 LC 77-155 ISBN 0-669-01458-3

Small Town Police and the Supreme Court
Stephen L. Wasby, Southern Illinois University
288pp. $18.00 LC 76-5621 ISBN 0-669-00654-8

Write for complete descriptions and catalog.

Lexington Books
D. C. Heath and Company
125 Spring Street
Lexington, MA 02173

HEATH

Deregulating American Industry
edited by **Donald L. Martin, Warren C. Schwartz**, University of Miami
In Press Fall 1977 LC 77-5273
ISBN 0-669-01603-9

The Interaction of Economics and the Law
edited by **Bernard Siegan**, University of San Diego Law School
In Press Summer 1977 LC 76-1223
ISBN 0-669-01340-4

Issues and Prospects for the New International Economic Order
edited by **William G. Tyler**, The University of Florida
In Press Summer 1977 ISBN 0-669-01445-1

Science and Technology Policy
edited by **Joseph Haberer**, Purdue University
A Policy Studies Organization Book
In Press Summer 1977 LC 77-70081
ISBN 0-669-01374-9

Tinkering with the System
Robert K. Yin, Karen A. Heald, Mary E. Vogel, The Rand Corporation
304pp. $16.00 LC 57903 ISBN 0-669-01360-9

Political Behavior in Libya: Selected Studies
edited by **Omar I. Fathaly**, Arab Development Institute
In Press Fall 1977 LC 77-712
ISBN 0-669-01426-5

Political Development and Bureaucracy in Libya
Omar I. Fathaly, Arab Development Institute,
Monte Palmer, The Florida State University
In Press Fall 1977 LC 77-713
ISBN 0-669-01427-3

Soviet Foreign Trade
William N. Turpin, Appalachian State University
In Press Summer 1977 LC 76-47337
ISBN 0-669-01143-6

Legal Policy Analysis
Stuart S. Nagel, Marion Neef, University of Illinois
256pp. $21.00 LC 76-14046
ISBN 0-669-00731-5

Ocean Thermal Energy Conversion
The American Society of International Law
In Press Summer 1977 LC 77-2049
ISBN 0-669-01441-9

The United States at Sea
Gerald J. Mangone, University of Delaware
In Press Fall 1977 LC 77-243
ISBN 0-669-01432-x

NEW 4TH EDITION

contemporary political ideologies

a comparative analysis

by lyman tower sargent,
University of Missouri, St. Louis

The fourth revision of this popular paperback has been updated with the results of current scholarship on all ideologies. The author maintains his basic format of analytical objectivity and an easily comprehensible writing style to facilitate the comparison of major and minor ideologies. The final chapter "The Varieties of Socialism" has been divided into two new chapters—"The New Left" and "Third World Ideologies." The book can be used as a general survey of political ideologies or as a focus on specific ideologies by following up on the current Suggested Readings at the end of each chapter. (Paperbound)

Available November

THE DORSEY PRESS
Homewood, Illinois 60430

Examination copies for adoption consideration available on request; please indicate course title and text presently used.

Write for our complete Catalog.

New from Columbia

TRUMAN AND THE STEEL SEIZURE CASE
The Limits of Presidential Power
MAEVA MARCUS

Truman's seizure of the steel mills in 1952 was an unprecedented use of presidential power. In this probing study Maeva Marcus explores the consequences of the Supreme Court decision in the *Steel Seizure* case on presidential action and the traditional separation of powers, with implications for the recent constitutional crisis precipitated by Watergate.
Contemporary American History Series $14.95

THE WEAK IN THE WORLD OF THE STRONG
The Developing Countries in the International System
ROBERT L. ROTHSTEIN

This timely exploration of the role of the developing countries in a very complex international system stresses the interaction between the domestic economic and political policies of the developing countries, the orientations they have adopted toward the external world, and the responses of the developed countries to their demands and needs.
Institute of War and Peace Studies $15.00

UNOFFICIAL DIPLOMATS
MAUREEN R. BERMAN and JOSEPH E. JOHNSON, Editors

This is the first attempt to examine the little-publicized role of non-officials in international diplomacy. The volume explores the contributions of "outsiders" in several cases of transnational diplomatic activity and describes the conditions since World War II that led to their growing involvement in relations among states today.
cloth, $15.00; paper, $4.95

JAPANESE INTERNATIONAL NEGOTIATING STYLE
MICHAEL BLAKER

Utilizing original Japanese sources, the author presents the most in-depth study yet undertaken of Japanese behavior in diplomatic negotiations. In this illuminating study Dr. Blaker identifies Japanese bargaining style in the half century before Pearl Harbor as consistent, scrutable, and undeserving of much of the criticism it has received.
Studies of the East Asian Institute $15.00

COLUMBIA UNIVERSITY PRESS
Address for orders: 136 South Broadway, Irvington, New York 10533

Distinguished New Books in Political Science

BROADCASTING AND DETENTE

Gerhard Wettig

This incisive study deals with the attitudes and policies of the Soviet Union and her principal allies *vis-a-vis* Western international broadcasting. The problem is analyzed from the example of the five German and American radio stations transmitting programs from West Germany to East Europe.

June 1977 130 pages $9.95, cloth

ENOCH POWELL AND THE POWELLITES

Douglas Schoen

Using published and unpublished public opinion poll data to enrich his analysis of Powell's career, Douglas Schoen demonstrates that Powell commands higher levels of support than has generally been believed. The first serious study of Enoch Powell's political impact, this book offers important new insights into contemporary British society and political cutlure.

May 1977 256 pages $18.95, cloth

SOVIET POLITICAL ELITES

The Case of Tiraspol

Ronald H. Hill

The first full-scale political study of a small Soviet provincial town, *Soviet Political Elites* is a major contribution to the literature on politics in the USSR. This study examines the influence of both party and state institutions at the local level, the relationship between local administration and the central authorities, the concept of an elite within the context of Soviet politics, and the organization of the local Communist party as well as the recruitment of politicians for leadership in the national arena.

May 1977 200 pages, illustrated $16.95, cloth

STABILITY AND CHANGE IN AUSTRALIAN POLITICS

Don Aitkin

"...a superb work of political analysis."
—*Donald E. Stokes, Princeton University*

Don Aitkin's searching analysis of Australian political behavior in recent decades sets out to explain the reasons for the extraordinary degree of stability in government, and the causes for those changes that do occur.

March 1977 296 pages $18.95, cloth

SOCIAL DEMOCRATIC PARTIES IN WESTERN EUROPE

William E. Paterson and Alastair H. Thomas

In Western Europe social democratic parties represent the largest single political force. This volume provides both an up-to-date survey of the current state of the parties and a base for some general comparisons about the parties and the condition of social democracy in Western Europe.

September 1977 82 pages $12.95, cloth

POLITICAL BUDDHISM IN SOUTHEAST ASIA

The Role of the Sangha in the Modernization of Thailand

Somboon Suksamran
with an introduction by Trevor O. Ling

"South-East Asia provides some important examples of the interrelationship between religion and politics. What Somboon Suksamran has perceived is the importance of the connection between Thai Buddhism and the Thai state in the present situation. His book provides a close-up and an interpretative analysis, and certainly deserves the attention of anyone who is concerned with the study of politics, or of religion, or of the problems of modern South-East Asia."
–from the Introduction

September 1977 160 pages $13.95, cloth

To order the books, please write to:

St. Martin's Press
P.O. Box 5352
New York, N.Y. 10017

Now extensively revised, with special emphasis on readability

GOVERNMENT BY THE PEOPLE, 10th Edition
James MacGregor Burns—Williams College; **Jack Peltason**—University of Illinois; **Thomas E. Cronin**—Brandeis University

The classic text on contemporary American government has again been updated to reflect the ongoing political process:

- incorporates the latest scholarship and research on elections, federalism, and policy-making processes
- utilizes both traditional and behavioral methods of analysis
- expands coverage of parties and elections
- extends contemporary analysis through Carter's first nine months in office
- updates foreign policy in thoroughly revised chapters
- provides several new chapters, including one on Congressional/Presidential relations
- supplies improved and expanded visuals—graphs and photographs
- includes illustrative case study materials
- appends a newly compiled glossary

Supplementary aids: Teacher's Manual, Student Guide, Test Item Booklet

GOVERNMENT BY THE PEOPLE, 10th Edition, is available in three editions to help you meet your classroom needs:

Basic Edition—a core text
1978 **560 pp. (est.)** **Cloth $12.95**

National Edition—additional chapters on governmental policies and function
1978 **688 pp. (est.)** **Cloth $13.95**

National-State-Local Edition—all of the above material plus chapters on state and local government.
1978 **832 pp. (est.)** **Cloth $15.95**

For further information, or to reserve your examination copy of any of the above editions, please write to: Robert Jordan, Prentice-Hall, Inc., Dept. J-868, Englewood Cliffs, New Jersey 07632.

Prices subject to change without notice.

Prentice-Hall

More outstanding titles from the Prentice-Hall contemporary Political Science list

UNDERSTANDING PUBLIC POLICY, 3rd Edition

Thomas R. Dye—Florida State University

A fundamental examination of the policies governments pursue, why these policies are selected, and what their consequences are.

1978 **352 pp. (est.)** **Cloth $11.95**

POLITICS IN STATES AND COMMUNITIES, 3rd Edition

Thomas R. Dye—Florida State University

Utilizing a "conflict management" point of view, Dye examines the vital political function of American states and communities in the political arena.

1977 **494 pp.** **Cloth $12.95**

WHO'S RUNNING AMERICA: INSTITUTIONAL LEADERSHIP IN THE UNITED STATES

Thomas R. Dye—Florida State University

A lively, in-depth study of the power elites in the United States based on the author's original research data.

1976 **272 pp.** **Cloth $8.95** **Paper $5.95**

AMERICAN CONSTITUTIONAL LAW: INTRODUCTORY ESSAYS AND SELECTED CASES, 6th Edition

Alpheus Thomas Mason—Emeritus, Princeton University; **William M. Beaney**—University of Denver

Selected major cases along with introductory essays which place the decisions in their historical, economic, and social contexts.

1978 **794 pp. (est.)** **Cloth $15.95**

WHAT IS GOVERNING? PURPOSE AND POLICY IN WASHINGTON

Richard Rose—University of Strathclyde, Scotland

A critical, yet sympathetic, examination of more than a dozen different interpretations of the purpose of governing America today.

1978 **192 pp. (est.)** **Paper $5.95**

THE FEDERAL SYSTEM IN CONSTITUTIONAL LAW

C. Herman Pritchett—University of California, Santa Barbara

Selected decisions of the U.S. Supreme Court dealing with the relationships between the branches of the federal government and the position of the states in the federal union.

1978 **350 pp. (est.)** **Paper $7.95**

PRINCIPLES OF AMERICAN GOVERNMENT, 8th Edition

Albert B. Saye—University of Georgia; **John F. Allums**—The American University; **Merritt B. Pound**

Concise introductory text for undergraduates objectively presents the important facts and relationships, with an expanded treatment of parties, politics, and elections.

1978 **416 pp. (est.)** **Paper $7.95**

THE POLITICS OF AMERICAN DEMOCRACY, 6th Edition

Marian D. Irish—Emeritus, The American University; **James J. Prothro** and **Richard J. Richardson**—both of the University of North Carolina, Chapel Hill

The workings of American government, including political parties, decision-making agencies, and pressure groups—from a systems-analysis approach.

1977 **480 pp.** **Cloth $13.95**

CITY GOVERNMENT AND URBAN PROBLEMS: A NEW INTRODUCTION TO URBAN POLITICS

Demetrios Caraley—Barnard College, Columbia University

An up-to-date examination of city government combining decision-making and performance approaches to political analysis.

1977 **449 pp.** **Cloth $12.95**

TEXAS: ITS GOVERNMENT AND POLITICS, 4th Edition

Wilbourn E. Benton—Texas A & M University

A balanced analysis of governmental procedures and contemporary political issues and problems in Texas. The real world of politics based on close observation.

1977 **372 pp.** **Paper $6.95**

THE AMERICAN LEGISLATIVE PROCESS: CONGRESS AND THE STATES, 4th Edition

William J. Keefe and **Morris Ogul**—both of University of Pittsburgh

Wide-ranging and timely analysis of the role of legislature in the political system. Places institutions firmly within their political contexts.

1977 **497 pp.** **Cloth $12.95**

LEADING CONSTITUTIONAL DECISIONS, 15th Edition

Robert F. Cushman—New York University

Over 40 edited Supreme Court cases with introductory notes tracing the development of selected aspects of the U.S. Constitution.

1977 **464 pp.** **Paper $7.95**

For further information, or to order or reserve an examination copy of any of these outstanding texts, please write to: Robert Jordan, Prentice-Hall, Inc., Dept. J-868, Englewood Cliffs, New Jersey 07632.

Prices subject to change without notice.

Prentice-Hall

BEHIND THE SCENES IN AMERICAN GOVERNMENT

Personalities and Politics

Peter Woll

Brandeis University

The premise of this unique reader is that the character and style of political leaders and the people surrounding them have a profound impact on the political process. Extracted from popular periodicals and best-selling books, Woll's selections illustrate how different personalities interact with political institutions and processes to shape government. There are fascinating vignettes on party politicos, a pressure group leader, a Washington lawyer, a political reporter, Presidents and First Ladies, judges, a Cabinet Secretary and presidential advisor, and a top-level bureaucrat. Each piece underscores an important concept in American political structure or process.

Woll has also prepared an Instructor's Manual which analyzes each selection in the book and provides additional background on the personalities discussed.

BEHIND THE SCENES IN AMERICAN GOVERNMENT is an ideal supplement to any American Government text. It brings politics alive, and we can practically guarantee its success with your students.

Paper 316 pages March 1977 $5.95

WAR, PEACE, AND INTERNATIONAL POLITICS

David W. Ziegler

Western Washington State College

Organized around the theme of war and its prevention, this new text provides an unusually coherent introduction to the basic concepts and propositions of international politics. David Ziegler's extensive experience teaching the introductory course convinced him that a text should "nurture students' interest, not exhaust it"; it should provide a solid foundation while motivating students to develop that foundation in subsequent courses.

The central theme focuses students' attention and provides a convenient way of introducing key topics such as international law and organization, diplomacy, defense policy, deterrence, nationalism, transnational influences, and the problems of global resources. Throughout the text Ziegler provides concrete illustrations of abstract concepts. Historical cases that bear on modern decision-making (Munich, Pearl Harbor, Korea) are included, along with maps that help clarify these situations. The Instructor's Manual includes chapter by chapter multiple-choice questions, plus general essay questions.

Paper 444 pages February 1977 $8.95

COLLEGE DIVISION

LITTLE, BROWN AND COMPANY

34 Beacon Street, Boston, Massachusetts 02106

New from

AMERICAN DEMOCRACY DEBATED: AN INTRODUCTION TO AMERICAN GOVERNMENT

Charles Dunn (Clemson University)

In this unique approach to introductory courses in American government, each chapter is organized around a debate – with both the affirmative and negative sides presented. By combining traditional coverage with this innovative, classroom-tested approach, this new text stimulates student interest while providing the "nuts and bolts" of American government.

500 pages $10.95 (tentative) cloth 1978

IMPORTANT PAPERBACKS from

Updated, revised, and eminently useful . . .

RESEARCH GUIDE IN POLITICAL SCIENCE

Carl Kalvelage (Minot State College)
and Morley Segal (The American University)
224 pages $4.50 1976

A perspective proving to be prophetic . . .

CONGRESSIONAL REFORM IN THE SEVENTIES

Leroy N. Rieselbach (Indiana University)
132 pages $3.95 1976

A reappraisal of the sources of – and reasons for . . .

CORRUPTION IN THE AMERICAN POLITICAL SYSTEM

Larry L. Berg, Harlan Hahn, and John R. Schmidhauser (all, University of Southern California)
216 pages $4.50 1976

To request examination copies, please visit our booth
or write Director of Marketing

GENERAL LEARNING PRESS

Morristown, New Jersey 07960, Box APSR977
Available in Canada from GLC Publishers, Ltd., Agincourt, Ontario

Introducing a Dynamic Revision
Of a Superb Teaching Tool . . .

CRIMINAL JUSTICE:
INTRODUCTORY CASES AND MATERIALS

Second Edition

by John Kaplan, *Professor of Law, Stanford University*

- Completely updated through the Spring 1977 term of the Supreme Court.
- Contains a wholly new chapter on capital punishment and an entirely new bibliography.
- Materials organized with painstaking clarity and readability.
- Contains a wealth of comments and questions on all pertinent subjects.
- Illustrates clearly the author's own broad knowledge of and discerning eye for the best material on every specific topic discussed.
- Provides an enlightened and thorough overview of the issues and dilemmas which both characterize and bedevil the American criminal justice system today.
- Original edition used successfully at some 400 colleges and universities.

About 750 pages — November 1977 publication

And available now . . .

LEADING CONSTITUTIONAL CASES ON
CRIMINAL JUSTICE

1977 Edition

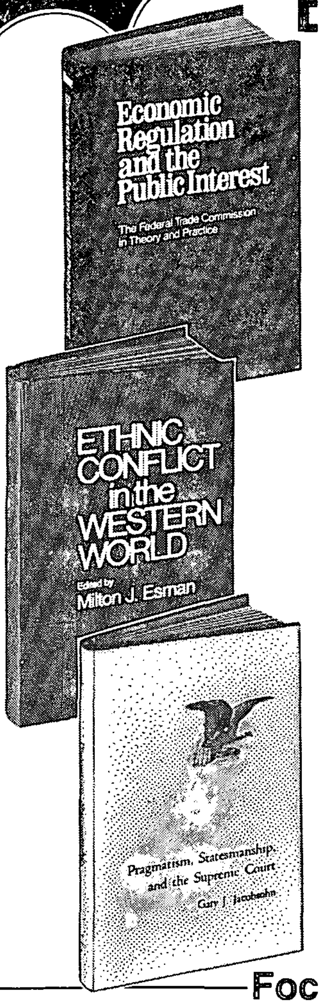
by Lloyd C. Weinreb, *Professor of Law, Harvard University*

THE FOUNDATION PRESS, INC.
170 OLD COUNTRY ROAD
MINEOLA, N.Y. 11501

A respected name in political science and government circles

Cornell
University
Press

New from Cornell


Economic Regulation and the Public Interest
The Federal Trade Commission in Theory and Practice
ETHNIC CONFLICT in the WESTERN WORLD
Edited by Milton J. Esman
Pragmatism, Statesmanship, and the Supreme Court
Gary J. Jacobsohn

Economic Regulation and the Public Interest

The Federal Trade Commission in Theory and Practice

By ALAN STONE. Written from the perspective of a political scientist who spent eight years as an attorney for the FTC, this book is a knowledgeable account of the origins and performance of the Commission. It clarifies the nature and potential of economic regulation. $15.00

Ethnic Conflict in the Western World

Edited by MILTON J. ESMAN. Why, in the late 1960's, did ethnic minorities such as Scots and Welsh, Québecois, Bretons and Basques unexpectedly begin to protest and assert their demands with fresh vigor, confidence and even violence? Addressing this question from historical and theoretical perspectives, this book is the first systematic attempt to deal with the re-emergence of ethnic and regional conflict in Western societies. $17.50

Pragmatism, Statesmanship, and the Supreme Court

By GARY J. JACOBSOHN. This broad-ranging study examines and criticizes the application of pragmatism to questions in constitutional law. The author's aim is to evolve a jurisprudential approach that would enable the Supreme Court to carry out its unique functions in a statesmanlike way. $12.50

Focusing on Asian Politics

Policymaking in Contemporary Japan

Edited by T.J. PEMPEL. This book consists of nine original essays that analyze and describe in detail a number of issues central to Japanese politics. Setting forth Japanese experience in analytical terms that are relevant to American thinking, this book makes a solid contribution to the study of the internal workings of public policymaking. $17.50

The Politics of Bhutan

By LEO E. ROSE. Examining the political culture of a country that the outside world knows little about, Professor Rose gives a broad overview of the historical and cultural environment in which Bhutan's present political system developed and analyzes the country's external relations, primarily with India. (*South Asian Political Systems* series.) $19.50

CORNELL UNIVERSITY PRESS • ITHACA AND LONDON

Listing and Order Form for Publications of the AMERICAN POLITICAL SCIENCE ASSOCIATION

THE AMERICAN POLITICAL SCIENCE REVIEW. Quarterly journal of scholarly articles and book reviews in political science. Included in APSA membership. Back issues: $10.50 per copy; $42.00 per volume.

PS. Quarterly journal of Association news and articles of professional concern. Included in APSA membership. Back issues: $2.00 per copy; $8.00 per volume. ($5.00 for the Spring issue containing the preliminary program.)

DEA NEWS FOR TEACHERS OF POLITICAL SCIENCE. Newspaper on education and the curriculum. Included in APSA membership.

ANNUAL MEETING PROGRAM. Final program for APSA Annual Meeting. Included in Annual Meeting Registration. Extra copies: $1.50.

APSA ANNUAL MEETING PAPERS.** Hard copies, $3.00 each.

APSA ANNUAL MEETING PROCEEDINGS.** Microfilm reels of all papers, 1901-1912; 1956-present. $22.00 per reel (approx. 7 reels per meeting).

CUMULATIVE INDEX TO THE AMERICAN POLITICAL SCIENCE REVIEW.** 1906-1968, $6.50.

CUMULATIVE INDEX TO THE PROCEEDINGS OF THE ANNUAL MEETINGS.** Key word index to all papers included in proceedings of Annual Meetings for 1904-1912; 1956-1970; $18.50.

WOMEN IN POLITICAL SCIENCE: STUDIES AND REPORTS, 1967-71 OF THE APSA COMMITTEE ON THE STATUS OF WOMEN IN THE PROFESSION.** PB 486, $6.00 microfilm; $16.50 Xerographic reprint (U.S. prices).

POLITICAL SCIENCE THESAURUS. A major reference tool in political science; this volume represents the terminology control device for a computer-based information retrieval service. Individuals: Hardback, $20.00; paper, $15.00. Institutions: Hardback, $30.00; paper, $25.00.

APSA BIOGRAPHICAL DIRECTORY (Sixth Edition, 1973). Biographical information on over 7,000 Association members, plus names and addresses for approximately 12,500 members. Members: Hardback, $7.50; paper, $5.00. Non-Members: Hardback, $12.50; paper, $10.00.

APSA DIRECTORY OF MEMBERS: ANNUAL SUPPLEMENT TO THE APSA BIOGRAPHICAL DIRECTORY. Names and current addresses of all APSA members. $3.00 each.

GUIDE TO PUBLICATION IN POLITICAL SCIENCE. Co-sponsored by the APSA Committee on the Status of Women and the Women's Caucus for Political Science. $1.00.

ROSTER OF WOMEN IN POLITICAL SCIENCE (4th edition). Biographical information on women political scientists. $3.00 each for members; $5.00 for non-members.

ROSTER OF CHICANOS IN POLITICAL SCIENCE. Biographical information on Chicano political scientists. $3.00 for members; $5.00 for non-members.

ANNUAL APSA DIRECTORY OF DEPARTMENT CHAIRPERSONS. Names and addresses of Chairpersons of departments offering political science at four-year institutions. $20.00 each.

ANNUAL APSA SURVEY OF DEPARTMENTS. Annual report on the results of an extensive questionnaire sent to all four-year institutions offering political science. Includes salary information, enrollment trends and many other items of current interest. $20.00 each.

PERSONNEL SERVICE GUIDELINES FOR EMPLOYERS AND APPLICANTS. Pamphlet containing information and guidelines for members of the APSA Personnel Service and for employers listing their vacancies in the Personnel Service Newsletter. No charge.

PERSONNEL SERVICE NEWSLETTER. Monthly listing of positions for political scientists. APSA Members: $8.00 subscription.

CAREERS AND THE STUDY OF POLITICAL SCIENCE: A GUIDE FOR UNDERGRADUATES. A monograph discussing the relationship between political science knowledge and skills and careers in law, government, business, journalism, teaching, etc. $.50 each; bulk rates available.

**Order from: Customer Service Department
University Microfilms
300 North Zeeb Road
Ann Arbor, Michigan 48106

CAREER ALTERNATIVES FOR POLITICAL SCIENTISTS: A GUIDE FOR FACULTY AND GRADUATE STUDENTS, by Thomas E. Mann. A monograph containing practical advice for obtaining nonacademic employment. $1.00 each.

GUIDE TO GRADUATE STUDY IN POLITICAL SCIENCE. Compilation of Ph.D. and Masters programs in political science. The 1977 Guide is revised and expanded to include listings of faculty and fields of specialization. $4.00 each.

RESEARCH SUPPORT FOR POLITICAL SCIENTISTS. A monograph containing detailed listings of public and private agencies that fund research fellowships, grants, and contracts. Includes section on strategic thinking about funding research and a guide to preparing research proposals. $4.00 each.

STORMING WASHINGTON: AN INTERN'S GUIDE TO NATIONAL GOVERNMENT by Stephen Frantzich. A monograph on Washington, D.C. internships. $2.00 each.

GLOBAL DIMENSIONS IN U.S. EDUCATION. A joint publication of the APSA Committee on Pre-Collegiate Education, the Education Commission of the International Studies Association, and the Center for War/Peace Studies of the N.Y. Friends Group, Inc. Four publications: The Elementary School, The Secondary School, The University, The Community. $1.50 each, $5.00 per set.

COMPARING POLITICAL EXPERIENCES. Complete description of an alternative course for high school government instruction. $2.75 each.

SETUPS: AMERICAN POLITICS (Supplementary Empirical Teaching Units in Political Science). New computer related instructional materials whereby students learn important substantive topics in American politics as they learn methods of analysis. *Voting Behavior: The 1972 Election* by Bruce D. Bowen, C. Anthony Broh, Charles L. Prysby. *Political Socialization Across the Generations* by Paul Allen Beck, Jere W. Bruner, L. Douglas Dobson. *Political Participation* by F. Christopher Arterton, Harlan Hahn. *Representation in the United States Congress: 1973* by Ray A. Geigle, Peter G. Hartjens. *The Supreme Court in American Politics: Policy Through Law* by John Paul Ryan, C. Neal Tate. *U.S. Energy, Environment and Economic Problems: A Public Policy Simulation* by Barry Hughes. *The Dynamics of Political Budgeting: A Public Policy Simulation* by Marvin K. Hoffman. For information and price list write: APSA Division of Educational Affairs. $3.50 each; lower price on bulk orders.

INSTRUCTIONAL RESOURCE MONOGRAPHS. Guides on materials and methods for teaching and learning in political science. *A Guide to Library Sources in Political Science: American Government* by Clement E. Vose, $6.50 hardback, $4.00 paperback. *PSI and Political Science: Using the Personalized System of Instruction to Teach American Politics,* edited by Ralph B. Earle, Jr., $3.50 paperback. *Simple Simulations,* by Charles Walcott with Anne Walcott, $3.50 paperback. *Computer-Assisted Instruction in Political Science,* edited by Jonathan Pool, $4.00 paperback. *U.S. Census Data for Political and Social Research: A Resource Guide,* $4.00, and *U.S. Census Data for Political and Social Research: A Student Manual,* $2.50, both by Phyllis G. Carter. *The Case of the Unreturned Cafeteria Trays* by Lloyd S. Etheredge, $1.00 each.

LEGISLATIVE SERVICE PROJECT MANUALS. Individual manuals available for: California, Kansas, Kentucky, Montana, Ohio, Oregon, Rhode Island. $2.50 each.

POLITICAL SCIENCE AND STATE AND LOCAL GOVERNMENT. A volume on the current and future state of research and educational programs in the field of state and local government. $3.75 each.

APSA MEMBERSHIP MAILING LISTS AND LABELS. For information and price list, contact Membership Secretary, APSA.

APSA POLITICAL SCIENCE DEPARTMENT CHAIRPERSONS MAILING LABELS. For information and price list, contact APSA Departmental Services Program.

Order Form

I am enclosing a check for $________________, for the following publications: ____________________

____

____

Name____

Address____

City________________________State______________Zip________

All orders must be prepaid.

Order from: American Political Science Association
1527 New Hampshire Avenue, N.W.
Washington, D.C. 20036

from ABC-CLIO

Peter H. Merkl, Series Editor

STUDIES IN INTERNATIONAL AND COMPARATIVE POLITICS

Political Culture and Group Conflict in Communist China

Alan P. L. Liu

Liu's skillful blend of autobiographical accounts, Red Guard publications, official media, and scholarly documents provides a highly readable and thought-provoking account of the Cultural Revolution and of revolutionary phenomena and totalitarianism in general. *No. 4 in the Series.* 1976. 205 pages. $17.95 casebound, $5.75 paper.

Total Revolution

A Comparative Study of Germany under Hitler, the Soviet Union under Stalin, and China under Mao

C. W. Cassinelli

The studies of Germany, the Soviet Union, and China are each divided into four chapters covering 1) the ideology of the movement; 2) the social conditions that prevailed when the movement captured state power and how the leader took advantage of them; 3) the changes that the revolutionary leaders made and attempted to make in their societies; and 4) the methods they invented to implement their revolutionary policies. *No. 10 in the Series.* 1976. 231 pages. $19.75 casebound, $6.25 paper.

American Bibliographical Center - Clio Press
Riviera Campus, 2040 A.P.S. P. O. Box 4397,
Santa Barbara, California 93103

A B C - CLIO INC.

American Government

CROTTY

POLITICAL REFORM AND THE AMERICAN EXPERIMENT

William J. Crotty. Here is the author's original thinking on voting, campaign financing, and the structure of presidential nominating systems and political parties. For each issue, historical background is presented and contemporary proposals for reform are evaluated. Incorporates recent Supreme Court rulings on campaign finance, and is the only available text with material on comparative voting registration. 312 pages. 1977. ISBN 0-6900-0869-4. $6.75/paper.

VAN DER SLIK

AMERICAN LEGISLATIVE PROCESSES

Jack R. Van Der Slik. Organized around the systems approach of David Easton, this text provides an empirically oriented, thorough, and concise analysis of legislative processes at the state and local levels. Using various systematic studies the text explores what legislators are like, how they make decisions, and how they organize themselves. Also covers the way American political culture shapes the practice of politics, the relationships of legislatures and legislators with the people and other institutions, interlegislature behavior, and policy outputs. 370 pages. 1977. ISBN 0-6900-0870-8. $10.50.

California Government

HYINK ET AL.

POLITICS AND GOVERNMENT IN CALIFORNIA, Ninth Edition

Bernard L. Hyink, Seyom Brown, and Ernest W. Thacker. An up-to-date analysis of government affairs in California, featuring a separate chapter on political campaigning, party organizations, pressure groups, and lobbying; special attention to revisions of the California constitution and recent changes in rules for the Democratic presidential primary, among other topics. Instructor's Manual. 276 pages. 1975. ISBN 0-6900-0809-0. $5.50/paper.

ISSUES IN CALIFORNIA POLITICS AND GOVERNMENT

Bernard L. Hyink, Seyon Brown, Ernest W. Thacker, and Steven D. Brown. To supplement the authors' text or others, this supplement analyzes six issues involving fundamental value choices for California residents: political leadership in California; controlling the buying and selling of politicians; protecting California's coast; the nuclear power debate; the farm labor controversy; and financing public education. Selected references. 117 pages. 1977. ISBN 0-6900-1520-8. $2.50/paper.

Thomas Y. Crowell Company 10 East 53d Street, N.Y., N.Y. 10022
To request examination copies, please write to Heidi Seliger, Dept. 459.

NEW FROM

HOOVER INSTITUTION PRESS

1977 YEARBOOK ON INTERNATIONAL COMMUNIST AFFAIRS
P170 612 pp $30.00 cloth
Richard F. Staar
Editor

TEST CASE:
Italy, Ethiopia, and the League of Nations
P159 384 pp $14.95 cloth
George W. Baer

SELF GOVERNMENT BY DISTRICT
Myth and Reality
P162 149 pp $4.00 paper
Robert B. Hawkins, Jr.

COMMUNIST REGIMES IN EASTERN EUROPE
Third Edition
P171 302 pp $13.95 cloth $4.95 paper
Richard F. Staar

Forthcoming

THE 25TH CONGRESS OF THE CPSU
P184 144 pp *Price not set*
Alexander Dallin
Editor

NOTHING BUT HONOR
The Story of the Warsaw Uprising 1944
P183 ca. 300 pp *Price not set*
J. K. Zawodny

THE MARSHALL PLAN SUMMER
An Eyewitness Report on Europe and the Russians in 1947
Special Project ca. 250 pp *Price not set*
Thomas A. Bailey

Stanford University, Stanford, California 94305

POLITICS IN WEST GERMANY

Second Edition

Lewis J. Edinger

Columbia University

This new edition of Lewis Edinger's analysis of contemporary West German politics is virtually a new book. It places public policy processes in the Federal Republic in the context of the comparative analysis of advanced industrial societies. The book is based on materials drawn from recent German studies, extensive interviews with West German leaders, opinion polls, census data, and newspaper accounts. Historical background is included only to the extent that it bears on contemporary politics.

Paper approx. 416 pages August 1977 $5.95

CASES IN COMPARATIVE POLITICS

Third Edition

James B. Christoph
Indiana University

Bernard E. Brown
City University of New York

Extensively revised and updated, this new Third Edition of CASES IN COMPARATIVE POLITICS features eight case studies—two each on Britain, France, Germany, and the U.S.S.R.—that examine foreign and domestic policy-making in those countries. All of the cases were written by specialists expressly for this volume. New cases on university reform in France, codetermination in West Germany, and Ostpolitik have been added, and important new material has been incorporated into all the other cases. An introductory section deals with the case method in comparative politics and the concept of decision-making.

Paper 350 pages August 1976 $6.95

POLITICS IN FRANCE

Third Edition

Henry W. Ehrmann

Since 1971, when the Second Edition of this leading study of politics in France appeared, that country has gone through one national referendum, one parliamentary election, one presidential election, and a rather drastic restructuring of its party system. The new Third Edition of POLITICS IN FRANCE will help students understand these important events in both their new and old contexts.

Professor Ehrmann was also prompted to write this revision by the continuing, impressive output of studies on modern French politics. He critically reviews the most important discussions and indicates major areas that remain open for further investigation.

Paper 372 pages August 1976 $5.95

COLLEGE DIVISION

LITTLE, BROWN AND COMPANY

34 Beacon Street, Boston, Massachusetts 02106

OXFORD

Freedom and the Court

Civil Rights and Liberties in the United States

Third Edition

HENRY J. ABRAHAM, University of Virginia. ☐ The third edition of this well-known text articulates the role of the United States Supreme Court in its continuing endeavor to define the rights of the individual and of the state in our democratic society. Particular emphasis is placed on major issues of public law, such as freedom of expression, racial and political equality, criminal justice, and religion.

1977 480 pp. cloth $17.95 paper $5.00

Marxism and the Metropolis

New Perspectives in Urban Political Economy

Edited by **WILLIAM K. TABB,** Queens College, City University of New York; and **LARRY SAWERS,** The American University. ☐ Twelve papers, originally presented at a conference at The New School for Social Research, offer a unique historical approach to urban studies. Covering such topics as changes in technology and organization of distribution activities, location decisions of individuals and corporations, conflicts between interest groups, and how transportation, housing, and urban renewal decisions are made, the volume will be of interdisciplinary interest.

March 1978 288 pp. cloth $8.50 paper $5.50

Karl Marx: Selected Writings

Edited by **DAVID McLELLAN,** University of Kent. ☐ This comprehensive account of the thought of Karl Marx offers substantial portions from celebrated works as well as from seldom anthologized writings, arranged chronologically. Marx's early writings are generously sampled and a number of his letters are included.

1977 624 pp. cloth $12.00 paper $6.00

Political Paradoxes and Puzzles

ARUN BOSE, University of Delhi. ☐ The author draws here on game theory and on the theory of collective choice to provide a well-integrated framework within which to resolve a number of political paradoxes and puzzles. Among the contemporary issues he considers are Chinese Communism, the balance of global superpower, and several Indo-West Asian problems.

October 1977 256 pp. paper $5.00

Prices and publication dates are subject to change.

Party Dynamics

The Democratic Coalition and the Politics of Change

RICHARD L. RUBIN, Columbia University. ☐ In a penetrating analysis of the Democratic Party, the author provides insights into the dynamics of the American party system in general. Professor Rubin focuses systematically on the internal life of the Democratic Party as declining support among traditionally Democratic groups erodes the coalitional bases of presidential dominance. "Professor Rubin has rendered great service in this taut, fact-laden book."—George Daly in *New York Law Journal*

1976 220 pp. cloth $9.95 paper $4.00

Reconstruction of Society Series
General Editors: Robert E. Lana and Ralph L. Rosnow

Psyche and Demos

Individual Psychology and the Issues of Population

WARREN B. MILLER, American Institutes of Research; and **R. KENNETH GODWIN,** Oregon State University. ☐ This book integrates current behavioral science and social science knowledge about individual decision making and adaptation with the making of public policy as these two levels of analysis and discourse bear on current population problems. It proposes a general model useful for analyzing and understanding the relations of individuals to the larger populations.

1977 336 pp.; 14 line drawings
cloth $12.00 paper $5.00

Media, Politics, and Democracy

BERNARD RUBIN, Boston University. Professor Rubin explores the tremendous influence of the mass media on basic social and political education and makes a strong case for media reform.

1977 208 pp. cloth $8.00 paper $4.00

The End of Imprisonment

ROBERT SOMMER, University of California, Davis

1976 (paper, 1977) 224 pp. cloth $8.95
paper $2.50

Aggression and Crimes of Violence

JEFFREY H. GOLDSTEIN, Temple University

1975 (paper, 1976) 208 pp. cloth $8.95
paper $2.50

OXFORD UNIVERSITY PRESS
200 MADISON AVENUE
NEW YORK, N.Y. 10016

BETWEEN FAITH AND REASON

An Approach to Individual and Social Psychology

By Francisco José Moreno

"The originality and major contribution of Moreno's work is not in identifying fear as a distorting influence on thinking, but in locating the root of fear in reason itself. Moreno's description of how reason generates fear through an internal dialectic marks a decisive, original, and far-reaching step beyond previous contextual critiques of rationalism . . . Moreno's contribution is an important achievement, not only to political, social and psychological theory, but to the attainment of civilized values."
—Michael A. Weinstein, Purdue University, *from the Introduction*

In BETWEEN FAITH AND REASON, Professor Moreno challenges the idealization of reason which is so much a part of contemporary social science, political ideology and popular mythology.

Moreno examines the constant struggle between our ability to reason and our need for faith. In doing so, he locates the primary force motivating our actions and thoughts: "basic fear," which is the result of the inability of reason to answer the most fundamental questions concerning human existence.

Through this theory of "basic fear," Moreno defines our primary goal in life as an attempt to escape from a reality we cannot comfortably confront. Our sexual customs, religions, and politics are determined and influenced by "basic fear" and the contemporary idealization of reason is only another effort to create faith as a buffer against reality. Thus, through his original analysis, Professor Moreno establishes important connections between individual psychology and social and political behavior.

133 pages, $8.95

Available at bookstores or from

NEW YORK UNIVERSITY PRESS
Washington Square, New York, N.Y. 10003

Congress against Itself

By Roger H. Davidson and Walter J. Oleszek

A primer on the perils and problems of reorganization based on the story of the reform resolution brought before the House by the Bolling Committee. ". . . 'must' reading for anyone who wants a fuller understanding of the House and its internal dynamics." —Rep. Donald M. Fraser (D.-Minn.)

$15.00

Southern Africa in Crisis

Edited by Gwendolen M. Carter and Patrick O'Meara

Introduction by Colin Legum

This timely book looks behind the headlines to explain what is really going on in the various countries of Southern Africa in terms of historical traditions, the positions of internal factions, and American involvement in the area. Contributors include: John Marcum, Patrick O'Meara, Elizabeth Landis, E. Philip Morgan, Absolom Vilakazi, Tony Hodges, and Gwendolen M. Carter.

(A main selection of the Library of Political and International Affairs)

$12.50

The Case for the Welfare State

By Norman Furniss and Timothy Tilton

This innovative book argues that political debate should be focused on the current problems of advanced industrial societies: bureaucratic responsiveness, citizen participation, persistent poverty. The comparative discussion of welfare provision in Britain, Sweden, and the United States is enlightening.

$14.00

Working-Class Women and Grass-Roots Politics

By Kathleen McCourt

Reports on the involvement of white, working-class women in community organizations and analyzes the role transformation and resulting conflicts some working-class women are experiencing as well as the potential for change represented by this new grass-roots populism.

$10.00

The Great Debates

Kennedy vs. Nixon, 1960

A Reissue

Edited by Sidney Kraus

Introduction by Harold D. Lasswell

In view of the attention stimulated by the Ford-Carter debates, this reissue of an analysis of the Kennedy-Nixon debates by thirty experts in communications, political analysis, and opinion research will stir renewed interest. Includes the texts of the debates.

paper $5.95 cloth $17.50

Indiana University Press

Tenth and Morton Streets
Bloomington, Indiana 47401

"This is the most impressive book by Piven and Cloward yet..."

—MURRAY EDELMAN, Professor of Sociology, Columbia University

"**This beautifully written book is the most exciting and important** political study in years. Informative and analytical, it will long pose the vital questions for political science, political sociology, and social welfare studies...It challenges the received wisdom about reform and social change. In particular, the discussions of the civil rights and welfare rights movements are gems. This book is essential reading."

—S.M. MILLER, Chairman, Department of Sociology, Boston University

"**A powerful and persuasive debunking** of the conventional wisdom of the Left, forcing us all to rethink once more how American society can become more egalitarian...This book may turn out to be the most memorable retrospective on the events of the 1960s...*Regulating the Poor* is already a classic; *Poor People's Movements* will surely become one as well."

—HERBERT GANS, Professor of Sociology, Columbia University

POOR PEOPLE'S MOVEMENTS

WHY THEY SUCCEED, HOW THEY FAIL

by FRANCES FOX PIVEN and RICHARD A. CLOWARD

authors of *Regulating the Poor*

$12.95, now at your bookstore

PANTHEON

Our 35th Year

201 E. 50th St., New York, N.Y. 10022

PRAEGER SPECIAL STUDIES

WOMEN'S RIGHTS AND THE LAW
The Impact of the ERA on State Laws
BARBARA A. BROWN, ANN E. FREEDMAN, HARRIET N. KATZ
and ALICE M. PRICE
ca. 480 pp. Sept. 1977 LC 77-9961 ISBN 0-03-022316-4 ca. $30.00
PSS Student Edition: ISBN 0-03-022311-3 ca. $8.95

ARMS CONTROL AND EUROPEAN SECURITY
A Guide to East-West Negotiations
JOSEPH I. COFFEY
ca. 272 pp. Sept. 1977 LC 76-29615 ISBN 0-275-24340-0 $18.50

AGROPOLITICS IN THE EUROPEAN COMMUNITY
Interest Groups and the Common Agricultural Policy
WILLIAM F. AVERYT, JR.
ca. 175 pp. Oct. 1977 ISBN 0-03-039666-2 ca. $18.50

THE FRENCH COMMUNIST PARTY IN TRANSITION
PCF-CPSU Relations and the Challenge to Soviet Authority
ANNETTE E. STIEFBOLD
ca. 230 pp. Nov. 1977 LC 77-83477 ISBN 0-03-040946-2 ca. $16.50

CONFLICT AND COOPERATION IN THE PERSIAN GULF
Edited by MOHAMMED MUGHISUDDIN
ca. 225 pp. Oct. 1977 ISBN 0-03-021851-9 ca. $17.50

EASTERN EUROPE'S UNCERTAIN FUTURE
A Selection of Radio Free Europe Research Reports
Edited by ROBERT R. KING and JAMES F. BROWN
ca. 375 pp. Oct. 1977 ISBN 0-03-040861-X ca. $25.00

PUBLIC OPINION IN EUROPEAN SOCIALIST SYSTEMS
WALTER D. CONNOR and ZVI Y. GITELMAN with ADELINE HUSZCZO
and ROBERT BLUMSTOCK
ca. 240 pp. Nov. 1977 LC 77-83471 ISBN 0-03-040931-4 ca. $18.50

THE MILITARY IN CONTEMPORARY SOVIET POLITICS
An Institutional Analysis
EDWARD L. WARNER III
ca. 250 pp. Nov. 1977 LC 77-83476 ISBN 0-03-040346-4 ca. $18.50

YUGOSLAVIA AFTER TITO
ANDREW BOROWIEC
ca. 160 pp. Nov. 1977 LC 77-83466 ISBN 0-03-040916-0 ca. $15.00

PRAEGER SPECIAL STUDIES • PRAEGER PUBLISHERS
200 Park Avenue, New York, N.Y. 10017
A Division of HOLT, RINEHART AND WINSTON, CBS PUBLISHING GROUP
Please direct all purchase orders to Holt, Rinehart and Winston
383 Madison Avenue, New York, N.Y. 10017
Prices are subject to change without notice.

Do you need current facts on...

☐ Japan's *new* national leaders?
☐ *new* U.S. military and trade pacts?
☐ *new* regional problems in Eastern Europe?
☐ *new* political conditions in Kuwait?
...here's the first place to look!

Just one source brings you immediate, up-to-date answers to *these* questions —and *thousands of others* covering important topics on every independent nation on earth.

Political Handbook of the World: 1977

Edited by Arthur S. Banks

604 pages $24.95

The 1977 edition of this unique reference brings you completely up to date on developments in the 163 independent nations of the world. Compiled and organized by experts, the Handbook explains national-level and intergovernmental structures, analyzes important events, and identifies political leaders. It provides thorough general coverage of national political parties and movements, including assessments of the platforms and constituencies of both government and major opposition groups.

This timely and authoritative reference also covers each country's legislative structure, news media, diplomatic representation, plus information on population levels, currency, and official languages. *In addition, there are special sections on socio-*economics, political and constitutional background, as well as treatments of 135 major international organizations. No other single-volume reference gives you such quick access to so much useful, reliable information on international conditions.

What's NEW about the 1977 edition

—*new* data on recently recognized independent nations (TRANSKEI . . . the SEYCHELLES)
—*new* borders (INDONESIA . . . MAURITANIA . . . MALAYSIA)
—*new* names (KAMPUCHEA . . . CENTRAL AFRICAN REPUBLIC . . . MADAGASCAR . . . LIBYAN ARAB PEOPLE'S REPUBLIC . . . SOCIALIST REPUBLIC OF VIETNAM)
—*new* political conditions (SWEDEN . . . the PEOPLE'S REPUBLIC OF CHINA . . . ARGENTINA . . . LEBANON . . . HAITI . . . KUWAIT . . . PORTUGAL . . . SOMALIA . . . SPAIN . . . THAILAND . . . TRINIDAD AND TOBAGO . . . JAMAICA)
—*new* national leaders (JAPAN . . . MALAYSIA . . . NAURU . . . NIGERIA . . . PARAGUAY . . . PORTUGAL . . . SWAZILAND . . . SYRIA . . . the U.S. . . . WESTERN SAMOA)

—*new* military and trade pacts between the U.S. and IRAN, KENYA, TURKEY; the U.S.S.R. and KUWAIT, SUDAN, and EGYPT.

—and, for the first time, the *Political Handbook* includes an entirely *new* section dealing with current regional issues in Western Europe, Eastern Europe, Latin America, South Asia, S.E. Asia, and Sub-Saharan Africa.

About the Editor

Arthur S. Banks is Director of the Center for Social Analysis of the State University of New York at Binghamton.

The Political Handbook of the World is sponsored by the Council on Foreign Relations and is prepared by the Center for Social Analysis of the State University of New York at Binghamton.

15-DAY FREE EXAMINATION!

McGraw-Hill Book Company
1221 Avenue of the Americas, New York, N.Y. 10020

Please send me the POLITICAL HANDBOOK OF THE WORLD: 1977 (003641-1) for 15 days' free examination. At the end of that time I will remit payment of $24.95 in full, plus tax, postage, and handling charges, or return the book postpaid to McGraw-Hill without further obligation. If I remit in full with this order, plus tax, McGraw-Hill pays all regular postage and handling charges and full return privileges still apply. *This offer good only in U.S. and subject to acceptance by McGraw-Hill.*

Name______________________________

Address______________________________

City____________State________Zip________

57 J 054-4070-3

CANADIAN JOURNAL OF PHILOSOPHY

The Executive Editors announce the publication in Spring 1977 of
Supplementary Volume III

New Essays on
Contract Theory

Edited by Kai Nielsen and Roger A. Shiner

CONTENTS

Classical Liberalism and Rawlsian Revisionism—*Elizabeth Rapaport*
Rawls on Method—*C. F. Delaney*
Rawls and Intuitionism—*M. B. E. Smith*
E Pluribus Unum—*D. A. Lloyd-Thomas*
Fair Contracts and Beautiful Intuitions—*G. E. Pence*
Rawlsian Contractarianism: Liberal Equality or Inequality?—
    Virginia McDonald
Rights, Utilities and Contracts—*Alan H. Goldman*
Rawls and the Contract Theory of Civil Disobedience—*L. W. Sumner*

PRICE CN $10.00 (Free to individual and student subscribers to Vol. VII (1977))

Order from: **Canadian Journal of Philosophy**, 4-108 Humanities Centre,
The University of Alberta, Edmonton, Alberta, Canada T6G 2E5

New from Princeton

The Model Ombudsman

Institutionalizing New Zealand's Democratic Experiment

LARRY B. HILL

The first intensive study of New Zealand's "model" ombudsman, this book seeks to understand the process by which the institution was successfully adapted from the Scandinavian countries and made a part of New Zealand's political system. $17.50

Peasants in Power

Alexander Stamboliski and the Bulgarian Agrarian National Union, 1899-1923

JOHN D. BELL

Contending that an understanding of the agrarian contribution is necessary for an appraisal of the full dynamic of East European politics, Bell explores the history of the Bulgarian Agrarian National Union, the strongest of the East European organizations. Illus. • $16.50

Social Change and Political Participation in Turkey

ERGUN ÖZBUDUN

Contrary to the argument that political participation tends to increase with economic and social modernization, Özbudun shows in this study of Turkey that rapid socio-economic growth has coincided with a substantial decline in turn-out at the polls. *Written under the auspices of the Center for International Affairs, Harvard University* $13.50

The Politics of Cultural Nationalism in South India

MARGUERITE ROSS BARNETT

By exploring the history of Dravida Munnetra Kazhagam party, Barnett provides a new perspective on political identity and challenges the interpretation of cultural nationalism as an inherent threat to the integrity of territorially defined nation-states and to the progress of modernization. $22.50

Parties Out of Power in Japan, 1931-1941

GORDON MARK BERGER

"Berger's fine study provides the fullest account of Japanese politics in the 1930s and helps explain the decline of the political parties as well as the failure to replace them completely. Its richness of documentation and careful analysis guarantee it an important place in the literature for many years to come."
—Marius B. Jansen, Princeton University Illus. • $20.00

Now in Paperback

KENYA: The Politics of Participation and Control

Henry Bienen

Written under the auspices of the Center of International Studies, Princeton University, and the Center for International Affairs, Harvard University • Paper, $3.95
Cloth, $11.00

COMMUNISM IN ITALY AND FRANCE

Edited by Donald L. M. Blackmer and Sidney Tarrow

Limited Paperback Edition, $9.75
Cloth, $25.00

Princeton University Press
Princeton, New Jersey 08540

down to specifics

These are the first three titles in a new series, American Politics and Public Policy, edited by Jeffrey L. Pressman and Martha Wagner Weinberg:

The Implementation Game:
What happens After a Bill Becomes a Law
by Eugene Bardach
$17.95

Decision to Prosecute:
Organization and Public Policy in the Antitrust Division
by Suzanne Weaver
$14.95

The Ungovernable City
by Douglas Yates
$17.50

The MIT Press
Massachusetts Institute of Technology
Cambridge, Massachusetts
02142

Values and Development:
Appraising Asian Experience
edited by Harold Lasswell, Daniel Lerner, and John D. Montgomery
$19.95

Multinationals from Small Countries
edited by Tamir Agmon and Charles P. Kindleberger
$15.00

The New International Economic Order:
The North-South Debate
edited by Jagdish N. Bhagwati
$17.50 hardcover
$9.95 paperback

Managing the State
by Martha Wagner Weinberg
$15.00 hardcover
$5.95 paperback

Police Work:
The Social Organization of Policing
by Peter K. Manning
$17.50

Autonomous Technology:
Technics-out-of-Control as a Theme in Political Thought
by Langdon Winner
$17.50

Science Textbook Controversies and the Politics of Equal Time
by Dorothy Nelkin
$16.95

NEW PAPERBACK EDITIONS

The Tin Kazoo:
Television, Politics, and the News
by Edwin Diamond
$4.95
(hardcover, $9.95)

The Politics of Neglect:
Urban Aid from Model Cities to Revenue Sharing
by Bernard J. Frieden and Marshall Kaplan
$5.95
(hardcover, $14.95)

Housing the Urban Poor:
A Critical Analysis of Federal Housing Policy
by Arthur P. Solomon
$4.95
(hardcover, $12.00)

Urban Guerrilla Warfare in Latin America
by James Kohl and John Litt
$4.95
(hardcover, $12.50)

Revolution and Political Leadership:
Algeria 1954-1968
by William B. Quandt
$2.95
(hardcover, out of print)

The Coming Crisis in Israel:
Private Faith and Public Policy
by Norman L. Zucker
$4.95
(hardcover, $15.00)

National University Publications

NEW FALL 1977

BOSSES, MACHINES, AND URBAN VOTERS
An American Symbiosis
by John M. Allswang

An effectively argued work dealing with five famous politicial bosses from Tweed in New York City to Daley in Chicago. The first use of careful voting analysis to determine the exact nature of the political support of the various urban machines.
cloth, $9.95, paperback, $5.95

FRANKLIN D. ROOSEVELT AND THE CITY BOSSES
by Lyle W. Dorsett

An inside look at vote bargains, connivance, and patronage between F.D.R. and the six great city bosses of the New Deal years. cloth, $8.95, paperback, $4.95

SIX WHO PROTESTED: Radical Opposition to the First World War
by Frederick C. Giffin

An examination of the anti-war thought and activities of six American radicals who opposed their country's involvement in World War I: Eugene V. Debs, Max Eastman, Morris Hillquit, John Reed, Emma Goldman, and William D. Haywood.
$9.95

NO PLEDGE OF PRIVACY: The Watergate Tapes Litigation, 1973-1974
by Howard Ball

The central role of the courts in convincing Congress and the American public that the president is subject to Constitutional checks and balances, that the executive privilege protecting confidential presidential communications is subordinate to the courts' demand for evidence, and that the Judiciary, not the Executive, is the final interpreter of the Constitution. $9.95

ALSO

THE AMERICAN DIPLOMATIC REVOLUTION:
A Documentary History of the Cold War 1941-1947
by Joseph M. Siracusa
Spring 1977 (First American Edn.)
cloth, $12.95, paperback, $7.95

WAR, BUSINESS AND AMERICAN SOCIETY:
Historical Perspectives on the Military-Industrial Complex
edited by
Benjamin Franklin Cooling
Spring 1977 $12.50

THE IMPACT OF THE COLD WAR: Reconsiderations
edited by Joseph M. Siracusa
and Glen St. John Barclay
Spring 1977
cloth, $12.50, paperback, $6.95

THE PALESTINE STATE:
A Rational Approach
by Richard J. Ward, Don Peretz
and Evan M. Wilson
1976 $9.95

SEND FOR OUR COMPLETE CATALOG

KENNIKAT PRESS
90 South Bayles Ave. Port Washington, New York 11050

NEW FROM HOOVER

SOCIAL SECURITY
Promise and Reality
P179–368 pp–$14.95 cloth *Rita Ricardo Campbell*

THE POLITICAL ECONOMY OF THE OIL IMPORT QUOTA
Yoram Barzel
P172–96 pp–$8.95 cloth *Christopher D. Hall*

TECHNOLOGY AND CIVILITY
The Skill Revolution in Politics
P167–111 pp–$4.95 paper *Heinz Eulau*

JAPAN'S SEARCH FOR OIL
A Case Study in Economic Nationalism and International Security
P165–116 pp–$4.95 paper *Yuan-li Wu*

DRUG LAG
Federal Government Decision Making
S55–62 pp–$3.00 paper *Rita Ricardo Campbell*

HOOVER INSTITUTION PRESS
Stanford University, Stanford, California 94305

Johns Hopkins

completely revised and updated for course use

American Defense Policy

Fourth Edition.

edited by John E. Endicott and Roy W. Stafford, Jr.

Containing 70 percent new material, the fourth edition emphasizes a systematic account of the evolution of U.S. strategy; arms control, including a thorough discussion of SALT; defense policy-making processes, including the budgetary process; new and changing institutional and cultural factors.

from reviews of previous editions

An attempt to assemble in one volume a representative selection of the recent extensive American writings on defense matters; it succeeds brilliantly . . . its editors have done a considerable public service by concise compilation." — *International Affairs*

"There is not only a wide lateral coverage ranging through the major aspects of the spectrum of confrontation, but there is a welcome depth of historical perspective imparted by many of the selections." — *The Western Political Quarterly*

"Would make useful reading for any college course in international politics and post-World War II history." — *Military Affairs*

$7.95 paperback, $22.50 hardcover

The First Duce

D'Annunzio at Fiume

Michael A. Ledeen

One of the most flamboyant figures in the history of modern Italy, poet Gabriele D'Annunzio has often been described as the "John the Baptist" of Italian Fascism, for virtually the entire ritual of Fascist politics made familiar by Mussolini — the balcony address, the Roman salute, the dramatic dialogues with the crowd — were taken over from the "free state of Fiume." Yet there were, as Ledeen brings sharply into focus, profound differences between D'Annunzio's vision of a new world and that offered by Fascism. Ledeen presents the 16 months of the poet's personal rule in Fiume as a microcosm of the larger chaos of our modern world.

$13.50

new paperbacks

Israel Divided

Ideological Politics in the Jewish State

Rael Jean Isaac

"In her fine study of some of Israel's lesser-known political movements, Rael Isaac sheds much light on the singular character of Israeli politics." — *Commentary*

"This learned, well-documented work brings together in readable form a great deal of material otherwise inaccessible to those who read English." — *Jerusalem Post*

$3.95 paperback, $10.95 hardcover

Inside Foreign Aid

Judith Tendler

"One finishes her book believing that there are many causes for our various failures. But one also finishes having learned a great deal and having been stimulated. . . . Should be read not only by everyone in AID but by all those in aid business and in the Foreign Service." — *Foreign Service Journal*

"Judith Tendler has wirtten a very good book indeed. In the literature of aid, it is one of the few thoroughly original contributions to have appeared in recent years." — *Economic Journal*

$3.45 paperback, $9.95 hardcover

The Johns Hopkins University Press, Baltimore, Maryland 21218

Introducing two strong new candidates for '78

GOVERNING AMERICA
An Introduction

ROBERT SHERRILL, author and noted political journalist in consultation with
James David Barber, Duke University
Benjamin I. Page, University of Wisconsin, Madison
Virginia W. Joyner
Under the General Editorship of
JAMES DAVID BARBER

This is an impressive new survey of the American political system that is refreshingly free of pieties, abstractions, and generalities — relying instead on concrete examples and a strong, consistent point of view to introduce students to the realities of American political life. The same qualities that award-winning writer Robert Sherrill brings to his journalism — commitment, savvy, fluency, a health skepticism, and an active social conscience — inform this textbook as well. Sherrill skillfully conveys a very real sense of the way politics works, sounds, *feels;* instructors and students will find the book provocative and a delight to read.

Governing America is also distinguished by the thoroughness of its scholarship, coverage, and documentation. The close consultation of collaborators Barber, Page, and Joyner has resulted in a textbook complete in all particulars, especially in regard to the most important recent findings in political science.

652 pages (probable)
Publication: February 1978
Accompanied by *Study Guide* by Virginia W. Joyner
and *Instructor's Test Booklet*

POWER AND BALANCE
An Introduction to American Constitutional Government

IRA H. CARMEN, University of Illinois, Urbana-Champaign

Ira Carmen's innovative new textbook for the introductory course in American government is the first to survey the entire American political system from a constitutional and legal perspective — a viewpoint particularly attuned to the burgeoning interest in the law among undergraduates and the general public. A thorough analysis of the constitution, the Supreme Court, and the lower courts in the first three chapters prepares students for the subsequent application of constitutional rules and norms to the whole range of governmental processes and institutions normally covered in the introductory course. The author avoids legal jargon and provides the necessary historical background; extensive footnotes guide students to supplementary readings.

Paperbound. 736 pages (probable)
Accompanied by *Instructor's Test Booklet*
Publication: January 1978

HARCOURT BRACE JOVANOVICH, INC.
New York • San Diego • Chicago • San Francisco • Atlanta

200 YEARS OF THE REPUBLIC IN RETROSPECT

Edited by WILLIAM C. HAVARD and JOSEPH L. BERND. *xii, 348 pp. $12.50*

Seventeen original essays by specialists from several disciplines, *200 Years of the Republic in Retrospect* examines both past and current issues, comparing today's political institutions with their counterparts at the Republic's foundation.

"To see where we once were, where we now are, and where we are likely to go as a political system, here is an excellent source. It will not answer all questions, but it will achieve the greater goal of making the reader think." — JAMES ROBERTSON

IMMIGRANTS IN INDUSTRIAL AMERICA, 1850-1920

Edited by RICHARD L. EHRLICH. *240 pp. (approx.), tables, figs, index.*
(*Eleutherian Mills — Balch Institute*) *$12.50*

This collection of ten essays reflects new directions in interpreting American social history. From various perspectives the authors counter the renowned "melting pot" thesis by rejecting the notion that industrial America was destructive of ethnic traditions. They assert that immigrants who flocked to the United States during the industrial boom brought with them highly resilient cultures which were able to survive the new environment and, to some extent, influence the institutions of American society.

BALLOTS BEFORE BULLETS

The War Referendum Approach to Peace in America, 1914-1941

By ERNEST C. BOLT, JR. *xvii, 207 pp., index.* *$15.00*

The first published study of the war referendum movement of 1914-1941, *Ballots before Bullets* examines the history of an unusual but, at the time, popular peace plan — the idea of a direct vote of the American people on declarations of war. Professor Bolt stresses the plan's faith in "the people" and its affinity with Progressives such as Jane Addams, Amos Pinchot, and William Jennings Bryan. And he gives the fullest account yet available of the tactics of both proponents and opposition leaders such as Franklin Roosevelt.

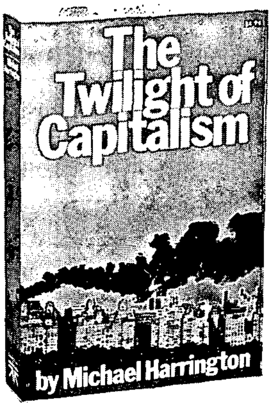
University Press of Virginia
Box 3608 University Station Charlottesville, VA 22903

TOUCHSTONE

PAPERBACKS

SIMON AND SCHUSTER
1230 Avenue of the Americas
New York, N.Y. 10020

For complimentary examination copies and catalogs, write on your departmental letterhead to Educational and Library Services, address above. Include details on course in which you are considering using title requested, and advise us where you have seen announcement. A 50¢ per book service charge must accompany each examination copy requested.



Harrington, Michael
THE TWILIGHT OF CAPITALISM

Arguing that America in the mid-1970's is irreversibly collectivizing itself under conservatives as well as liberals, Michael Harrington claims that it remains to be resolved whether the outcome will be humane or brutally repressive and bureaucratic. Harrington begins with a new, clear, straight-forward reexamination of Marxist thought, clarifying Marx's conceptions of capital, value and surplus value, labor, the dialectic, and history. He proposes a program for a democratic future through an extraordinary method of "rediscovered" Marxism. "This is an important and extremely interesting book."—John Kenneth Galbraith, *The New York Times Book Review*. "His documentation of . . . the American government's collusion with the oil companies both at home and abroad, the government's subsidies to giant agribusiness interests, or the inequities of our tax system is devastatingly effective."—*Political Science Quarterly*. "Harrington expounds difficult texts lucidly, and exhibits exceptionally wide knowledge of Marx's philosophy, as well as the views of later Marxian thinkers. . . . this book will be of great use in college courses."—*Library Journal*. "This is one of those rare books that can be, and deserves to be, read by most everyone, regardless of academic preparation or political persuasion. In this regard, the work ranks with the very best of Galbraith, Heilbroner, Mills, Sweezy, and Baran."—*Choice*.
☐ 22759 $4.95

Goldfarb, Ronald L. and Linda R. Singer
AFTER CONVICTION: A Definitive and Compelling Study of the American Prison System

"The broad field covered includes the historical development, the current status and evaluations of juvenile and adult institutions, sentencing, probation, clemency, prisoners' rights, and community correction . . . the book is well written, with much care taken in the arrangement of the material from the introduction to the epilogue. . . . most of the recommendations are based on research by recognized scholars. . . . provocative work."—*American Bar Association Journal*. "A chapter this well done on clemency is hard to find. The style is smooth and the authors never get lost in history or rhetoric. . . . It is an exhaustive and readable book that does not exhaust the reader."—*Texas Bar Journal*. "*After Conviction* may well be the most clinical, analytical and academic examination yet to have been written about the entire criminal justice system. . . . *After Conviction* is an artistic achievement which was designed for academe and . . . it presents many fascinating trends of thought and points of view."—*De Paul Law Review*. "[A] superbly written and researched book. . . . This book will prove a valuable resource for all interested in the correctional process. . . . [It] is as complete and thorough a treatment as can be found."—*Library Journal*.
☐ 22785 $8.95

Barnet, Richard J. and Ronald E. Müller
GLOBAL REACH
The Power of the Multinational Corporations
☐ 22104 $4.95

Quadrangle
Political Science for Fall

The Nuremberg Mind
The Psychology of the Nazi Leaders
Florence R. Miale and Michael Selzer

Based upon the heretofore little known results of Rorschach tests administered to the Nazis awaiting trial, *The Nuremberg Mind* largely disproves both claims that the Nazis were simply obeying orders within the context of a military operation and theories that they were acting within the expanded limits of an extraordinary political and psychological climate. This work, a fascinating blend of psychology and history, presents fresh new material and adds a whole new dimension to the discussion of Nazi war crimes and the rise and fall of the Third Reich.

Illustrated 320 pages paper $3.95

Seeds of Repression
Harry S. Truman and the Origins of McCarthyism
Athan Theoharis

An eye-opening account of American politics in the Cold War years and their continuing legacy in the national political debate, *Seeds of Repression* places the responsibility for McCarthy's rise at the doorstep of the White House. This book is certain to revise historical thinking and raise new questions about Harry S. Truman's accomplishments as president.

". . . soundly reasoned and researched . . . deserves careful reading by every student of contemporary history."
—*Journal of American History*

256 pages paper $2.95

The Grand Jury
The Use and Abuse of Political Power
Leroy D. Clark

Through careful analyses of the role of the grand jury throughout American history and by asking important questions about its recent, less honorable role in the political arena, Professor Leroy Clark confirms the immediate relevance of the grand jury. The author presents, as well, a program for the reform of the grand jury so that it will better serve its original purpose of protecting the people against arbitrary violations.

180 pages paper $2.95

The Paris Commune 1871
Stewart Edwards

In this definitive volume Stewart Edwards traces the events that took place during the two months of the Paris Commune, a desperate attempt to achieve political and social reforms demanded for more than half a century. This was a period of vivid drama crucially important for the growth of the European communist and socialist movement.

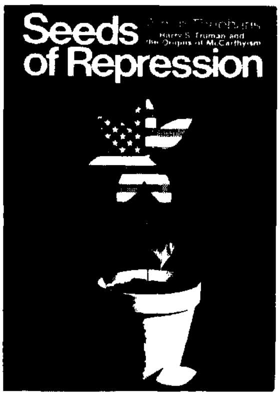
"Exceptionally well documented, incorporating the latest research in France and in England . . . it will become the standard book in English on the Commune for some time to come."—*London Times*

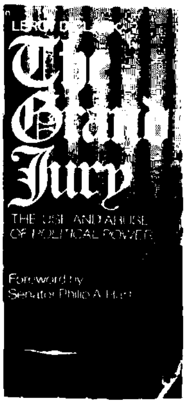
Illustrated 448 pages paper $4.95

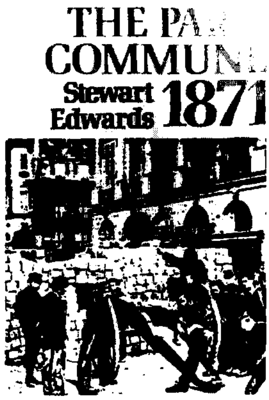
Write for our complete paperback catalog.

Quadrangle Books
Three Park Avenue
New York, New York 10016

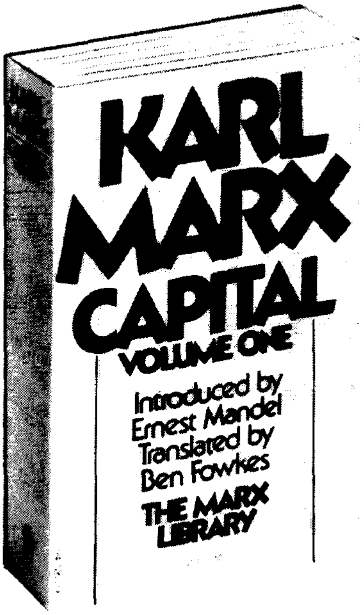
The NUREMBERG MIND
with a special introduction by G. M. Gilbert




The Grand Jury
THE USE AND ABUSE OF POLITICAL POWER
Foreword by Senator Philip A. Hart



Classics-
in NEW EDITIONS

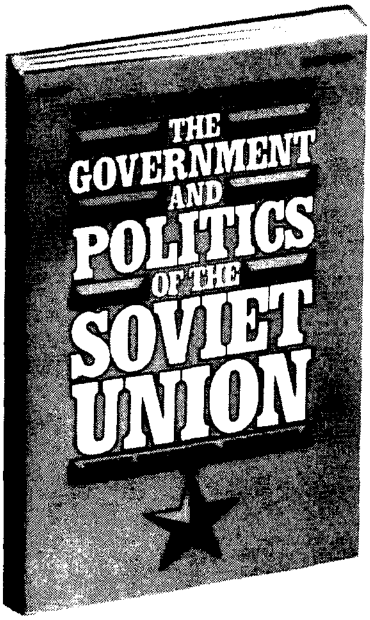

KARL
MARX
CAPITAL
VOLUME ONE
Introduced by
Ernest Mandel
Translated by
Ben Fowkes
THE MARX
LIBRARY

This new translation of *Das Kapital* by Ben Fowkes, which attempts to stress the literary merits of the work, is an important addition to the Vintage Marx Library. The introduction is by Ernest Mandel, author of *Late Capitalism.*

Capital
VOLUME ONE
by KARL MARX

$7.95, paperback. *A Vintage Original.*

Now at your bookstore
VINTAGE BOOKS
A division of Random House


THE
GOVERNMENT
AND
POLITICS
OF THE
SOVIET
UNION

Since its first publication in 1965, this book has established itself as the classic guide to the Soviet government, tracing the growth of Russia's government from its earliest days to the 1960's. Written by "the West's foremost scholar of the Soviet Union" *(National Review)*, the revised edition includes a new chapter covering changes and developments through 1976.

The Government and Politics of the Soviet Union
by LEONARD SCHAPIRO

$2.95, now in paperback.